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Can Work Alter Welfare Recipients' Beliefs?

The Self-Sufficiency Project

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Table of Contents

| | |
|---------------------------|-----|
| Tables | v |
| Acknowledgements | vii |
| Abstract | ix |
| Introduction | 1 |
| Two Perspectives | 3 |
| Empirical Issues | 5 |
| Data and Measures | 7 |
| Results | 9 |
| Descriptive Statistics | 9 |
| Effect of Work on Beliefs | 12 |
| Conclusions | 17 |
| Appendix | 19 |
| References | 21 |

Tables

| Table | Page |
|---|-------------|
| 1 Baseline Characteristics of Treatment and Control Group Members | 9 |
| 2 Change in Employment Status of Treatment and Control Group Members | 10 |
| 3 Percentage Who Agree With Statements at the Baseline Interview, by Employment Status | 11 |
| 4 Probit Estimates of the Probability of Greater Disagreement With the Statement at 36-Month Interview Than at the Baseline Interview | 12 |
| 5 Probit Estimates of the Probability of Greater Agreement With the Statement at the 36-Month Interview Than at the Baseline Interview | 13 |
| 6 Locus of Control Index (OLS) | 15 |
| 7 Locus of Control Index (IV) | 16 |
| 8 Strength of Agreement/Disagreement of the Baseline and 36-Month Surveys | 19 |

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Abstract

A common argument in support of work-based welfare reform is that exposure to work will lead welfare recipients to revise their beliefs about how they will be treated in the labour market. This paper explores the analytical and empirical basis for this argument. The difficulty in testing the assumption that work leads to a change in beliefs is that there is an inherent simultaneity between work and beliefs. Welfare recipients who work may have different beliefs because they learn about the world of work once they enter the labour market. Alternatively, welfare recipients who have a more positive view of work are the ones who are more likely to work. We use a unique data set that helps solve this simultaneity problem. We find that exogenous increases in work induced by an experimental earnings supplement led to the predicted change in beliefs.

Introduction

The possibility that getting welfare recipients to enter the labour market can have cumulative benefits is central to jobs-oriented welfare reform. The argument for the long run benefits of a work-based strategy can take two forms. The first is the usual human capital argument that welfare recipients lack job relevant skills and that “learning by doing” or “on-the-job” training can lead to skill acquisition. The second argument for getting welfare recipients into jobs focuses on changing their perceptions about their ability to function in the world of work:

The major challenge in workfare administration is to get people involved and to overcome their pessimism. (Mead, 1997)

This argument is based either implicitly or explicitly on the presumption that work can change a welfare participant’s beliefs about her ability to succeed, which can in turn break the cycle of welfare dependency. If this presumption is true, then work not only provides current income but may also change beliefs that may lead to future success in the labour market.

Can labour market activity change people’s perceptions about the obstacles they face? This issue, which was central to much of the early work on the culture of poverty and expectancy theories of poverty,¹ was abandoned by economists in the 1970s and 1980s who viewed the question as hopelessly vague and based on concepts more in the realm of sociology and psychology than in the realm of economics.² The recent interest in the intersection of economics and psychology (Benabou & Tirole, 2002) suggests that it may be fruitful to revisit this old question.

This paper has two objectives. The first is to provide a bridge between the concept of locus of control developed in the social psychology literature and the concept of informational updating, which is more familiar to economists. We argue that these two concepts lead to the same conclusion, namely, that entering the labour market can potentially lead welfare recipients to revise their beliefs about their chances of succeeding in the labour market. We stress that this outcome is possible but by no means assured. The second objective of this paper is to test the prediction that work leads to a change in beliefs.

The central problem in testing whether work alters beliefs is that there is an inherent simultaneity between these two outcomes. It has long been recognized that the correlations between work and beliefs do not identify the causal impact of either. There is ample evidence that respondents who work are less likely to think that they will be treated unfairly or to think that they do not have control over the events that shape their lives. But this could simply reflect the fact that individuals who have these beliefs are more likely to work. This would lead to the observed relationship even if work did not affect beliefs.

¹See Bane and Ellwood (1994) for a review.

²This shift also affected data collection. For example, the 1968 to 1972 Panel Study of Income Dynamics asked a set of attitudinal questions that were subsequently dropped.

This problem has long been recognized in the literature. The problem has been to find a credible exogenous source of variation in work. We use a unique data set from the Self-Sufficiency Project (SSP) to partially overcome this problem.³

This experimental project offered an earnings subsidy to a random group of long-term welfare recipients. Randomization ensures that the treatment is not correlated with the recipient's beliefs about her ability to succeed in the world of work. Since the earnings subsidy has been shown to have a substantial effect on work, it also satisfies the criterion for a valid instrument, namely, that the instrument must be correlated with work as well as not being correlated with beliefs. Our empirical results suggest that getting long-term welfare recipients to work does alter the beliefs of the young.

This paper consists of the following five sections. We start by presenting the social psychology and economic arguments that lead to the prediction that work can result in a change in beliefs. This is followed by a discussion of the statistical issues in estimating the causal impact of work on beliefs. We then describe the data, present the results, and conclude the paper.

³See Morris and Gennetian (2002) for another example in which the experimental design is used as an identifying instrument.

Two Perspectives

While psychologists and social psychologists use a language that is often unfamiliar to economists, their interest in the causes of low achievement is directly relevant to this study, which focuses on the formation of beliefs.⁴ This literature suggests that low achievement can reflect two conceptually different mechanisms. Individuals may not seek job opportunities because (1) they do not value the rewards that come from work (i.e. they are not motivated) or (2) because they do not believe that a realization of the outcome is under their control.⁵ Individuals have to both desire an outcome and to believe that they have control over the outcome in order for them to achieve a goal. If either of these is lacking then the individual does not undertake the task.

While social psychologists and economists start from different paradigms, it is possible to partially bridge the gap between these disciplines. The social psychology concept of motivation and control can be translated into terms more familiar to economists. Motivation can be viewed as reflecting tastes (i.e. what people want) and control can be viewed as the ability to choose the desired outcome. Achievement can be increased either by motivating people (i.e. changing their tastes) or by changing their perceptions of what is possible (i.e. changing their beliefs about the constraints they face).⁶

The latter can include skill acquisition (i.e. increasing human capital) or changes in perceptions of how these skills are evaluated (i.e. changes in prior beliefs). Individuals who believe that they cannot attain the ends they desire feel they have little control over their lives (i.e. their opportunity set is highly constrained by either skills or discrimination). In the extreme, they view that they have no options to choose from and conclude that they have no control over their own lives.⁷

These two frameworks open the possibility that getting welfare recipients to enter the job market can have cumulative effects by providing information on the constraints they face. For example, welfare recipients may believe that employers will not hire them because they lack the required skills or because employers are prejudiced against young single mothers. However, some employers may have had successful previous experience with welfare recipients and may believe that mothers who want to work are particularly good employees. If this is the case, then getting welfare recipients into the labour market may change their beliefs about how they will be treated. In the language of social psychology, this will lead to a change in perceived locus of control. In the language of economics, this will lead to a change in prior beliefs about the constraints they face in the labour market.

⁴Skinner, Zimmer-Gembeck, and Connell (1998) offer a useful overview.

⁵See Haidt and Rodin (1999) for a discussion of the concepts of control and efficacy in the broader social science literature.

⁶Economists study the impact of changes in constraints (or the perception of constraints) on the behaviour of individuals who are assumed to have preferences (tastes) determined by non-economic factors.

⁷In his seminal work in this area, Rotter (1966) introduced the concept of internal and external locus of control.

One of the contributions of the economic approach is that it highlights that, while entering the world of work may lead to more optimistic appraisal of one's ability to succeed, this is by no means a necessary consequence. Work can lead some welfare recipients to conclude that they had been too optimistic about how the labour market would treat them. The net impact of work oriented welfare programs depends on the relative number of welfare recipients who are overly pessimistic, compared with those who are overly optimistic. If these two cancel then the policy will have no net effect on beliefs.⁸ If a program is to have a positive effect on beliefs, then it must be true that, on average, welfare recipients affected by the program were too pessimistic about how they would be treated in the labour market. While one can make a credible case that welfare recipients are overly pessimistic, this remains an empirical issue.⁹

⁸In the language of economics, expectations are rational even if individual members of the group are overly optimistic or pessimistic. All that is required is that these cancel so that the average expectation is correct.

⁹Note that welfare recipients may be overly pessimistic because they are the people who are most likely to enter the welfare system (i.e. a pure selection effect) or because the welfare system causes pessimism (i.e. true state dependence).

Empirical Issues

Very few economic studies explore the determinants of people's beliefs. The vast majority of the literature treats beliefs as an explanatory variable by including attitudinal variables in an equation with labour market or welfare outcomes as the dependent variable. Andrisani (1977), Andrisani (1981), Duncan and Morgan (1981), Corcoran, Duncan, Gurin, and Gurin (1985), Goldsmith, Veum, and Darity Jr. (1997), and Dunifon and Duncan (1998) examine the impact of various attitudinal variables on labour market outcomes. Kunz and Kalil (1999) and Edwards, Plotnick, and Klawitter (2001) estimate the impact of differences in beliefs on welfare utilization. Coleman and DeLeire (2003) examine the relationship between locus of control and human capital accumulation.

The problem in isolating the causal effect of work is that work and beliefs are likely to be determined simultaneously. Individuals are more likely to enter the labour market if they believe they will be treated fairly, but their beliefs about how they will be treated may also reflect their labour market experiences. This two-way causation is explicitly recognized in Rotter (1990) and Kane (1987) but is often ignored in the empirical literature.¹⁰ One of the reasons for this gap is that it has been difficult to find an exogenous source of variation in labour-market outcomes that could affect beliefs.

We use data from a unique data set that allows us to overcome this serious econometric problem.¹¹ The Self-Sufficiency Project (SSP) was an experimental program that offered a wage subsidy to a randomized group of Canadian welfare recipients. Evaluation studies of SSP show strong positive impacts on employment.¹² Therefore, the SSP treatment satisfies the first criterion for a good instrumental variable, namely, that it be correlated with the endogenous variable. The second criterion is that the unobservable factors that affect the change in beliefs must not be correlated with the instrument. The randomized nature of the treatment satisfies this criterion.¹³ While persons who held more positive beliefs might have been more likely to work, they were no more likely to be assigned to the treatment group.

¹⁰There are some exceptions. Dunifon and Duncan (1998) avoid the issue of endogeneity of beliefs by using long lags of the attitudinal variables. Corcoran et al. (1985) and Goldsmith et al. (1997) use exclusionary restrictions to estimate causal impacts.

¹¹The New Hope Project offers another potential data set that includes information on attitudinal variables (see Huston et al., 2001). This experimental project, however, offered earnings subsidies only as part of a larger package, which may have had a direct impact on attitudes.

¹²See Michalopoulos et al. (2002).

¹³We exclude the SSP-Plus treatment group, which received some services that may have had a direct impact on beliefs.

Data and Measures

The Self-Sufficiency Project (SSP) was designed to determine the impact of instituting an earnings supplement for welfare recipients in order to reduce the reliance on long-term income assistance (IA). The hope was that the subsidy would lead not only to an increase in employment but also to a change in beliefs toward work and welfare. A random sample of single parents in New Brunswick and British Columbia, aged 19 and over, who had received IA for at least 12 of the previous 13 months, were assigned to either a treatment group or a control group. The treatment group was offered a substantial earnings subsidy for a period of three years so that most eligible families would find work to be financially preferable to continued receipt of IA.

SSP sample members were eligible for the earnings subsidy if they worked at least 30 hours per week in an eligible job. Each eligible individual had 12 months from the time of eligibility to begin working full time and to start collecting SSP payments. These individuals could claim the supplement for a maximum of 36 consecutive months, but only during those months they were employed full time. The supplement was equal to 50 per cent of the difference between the individual's monthly earnings and a target earnings level. In 1993 the monthly target earnings for an individual in New Brunswick were \$2,500, translating to a maximum subsidized wage of \$19 per hour for someone working 30 hours per week. The target earnings in British Columbia were \$3,083, or \$23 per hour for someone working 30 hours.

A person earning the minimum wage of \$5.00 per hour working 30 hours per week would, therefore, receive a supplement of over \$7.00 per hour in New Brunswick, effectively more than doubling her wage. It was expected that a subsidy of this size would increase labour supply and that the resulting work experience would lead to a change in belief toward work and welfare.

The analysis file contains 2,858 sample members assigned to the SSP treatment and 2,827 to the control group. A baseline interview was conducted with all sample members in late 1992. This instrument contained detailed information on employment and a set of questions about the recipients' beliefs at baseline. Sample members were re-interviewed 18 months and 36 months after the baseline interview. The questionnaire gathered information on employment histories and beliefs at the time of each survey. Since sample members did not have to start working until the 12th month after random assignment and since beliefs may change only slowly, we contrast beliefs in the 36-month interview with beliefs at baseline.¹⁴ While roughly 13 per cent of both the control and treatment groups attrited before the 36-month interview, Michalopoulos, Card, Gennetian, Harknett, and Robins (2000) conclude that this attrition does not lead to non-response bias in estimates of the impact of the program on employment.¹⁵

¹⁴This was the most recent data available at the time of this study. Data covering the 54-month interview has recently become available.

¹⁵See Appendix A of Michalopoulos et al. (2000).

While the SSP interview did not ask questions directly about attitudes toward work, it did ask a set of questions used by social psychologists, including three questions used to measure locus of control. The following questions were asked at both the baseline interview and the 36-month interview. Respondents were asked whether they agreed strongly, agreed, disagreed or disagreed strongly with the following statements:¹⁶

1. There is little that I can do to change many of the important things in my life.
2. I have little control over the things that happen to me.
3. Sometimes I feel as if I'm being pushed around in life.
4. I am often angry that people like me never get a fair chance to succeed.

The first three statements have been used extensively in the social psychology literature to measure locus of control. Individuals who view themselves as having limited options think there is little they can do to change their lives so they believe they have little control over what happens to them.¹⁷ These beliefs may arise from the belief that they do not have the skills to change their lives or that they are treated unfairly. In the context of the labour market, individuals may believe either that they lack skills or that there is discrimination against them. While the fourth question is not used to measure locus of control it captures another dimension of recipient's beliefs that could be affected by work experience.

¹⁶See Table 3.4 in Michalopoulos et al. (2000) for a previous analysis of these questions.

¹⁷Appendix A of Pearlin and Schooler (1978) provides the set of seven questions standardly used to construct indices measuring locus of control. Only three of these items were asked in the SSP interviews.

Results

DESCRIPTIVE STATISTICS

Table 1 presents descriptive statistics on the demographic and economic characteristics of sample members at the baseline interview. Columns 1 and 2 show the characteristics of all treatment and control group members, while columns 3 and 4 show the baseline characteristics for the persons who had not attrited by the 36-month interview. Comparing columns 1 and 2 shows that the characteristics of treatment group members and control group members were very similar at baseline, which is consistent with random assignment. Comparing columns 3 and 4 shows that similar patterns hold when the sample is restricted to those who had not attrited by the 36-month interview. This is consistent with Michalopoulos, Card, Gennetian, Harknett, and Robins (2000), who find that attrition was largely random.

Table 1: Baseline Characteristics of Treatment and Control Group Members

| | (1) | (2) | (3) | (4) |
|------------------------|-----------------|---------------|-----------------|---------------|
| | All | | Non-Attriters | |
| | Treatment group | Control group | Treatment group | Control group |
| Demographic | | | | |
| Female | 0.95 | 0.95 | 0.96 | 0.95 |
| Age | 31.90 | 31.90 | 31.80 | 31.80 |
| Never married | 0.48 | 0.48 | 0.48 | 0.49 |
| British Columbia | 0.53 | 0.53 | 0.50 | 0.52 |
| Speaks French | 0.13 | 0.14 | 0.14 | 0.13 |
| Number of children | 1.67 | 1.68 | 1.67 | 1.68 |
| Less than high school | 0.54 | 0.55 | 0.54 | 0.53 |
| Labour market | | | | |
| Previous experience | 7.30 | 7.50 | 7.50 | 7.30 |
| Unemployed at baseline | 0.82 | 0.81 | 0.80 | 0.81 |
| Part-time at baseline | 0.12 | 0.12 | 0.13 | 0.13 |
| Full-time at baseline | 0.06 | 0.07 | 0.07 | 0.06 |
| Observations | 2,858 | 2,827 | 2,458 | 2,503 |

Table 1 shows that sample members were almost all female and that the majority had less than a high school degree. The average number of children was 1.7, and nearly half of the respondents had never been married. Both treatment group members and control group members had only limited prior work experience and few were working at the baseline interview. The average work experience for both treatment group members and control group members at the baseline interview was a little over seven years.¹⁸ At baseline, less than 20 per cent of respondents were working and only about one third of these were working full

¹⁸This is considerably lower than potential work experience for these sample members with an average age of 32.

time. These descriptive statistics confirm that our sample of SSP welfare recipients had low education and limited work experience.

Table 2 confirms previous findings that the SSP earnings subsidy had a substantial impact on employment.¹⁹ The first three columns contrast the change in employment between the baseline interview and the 36-month interview for the control group and for all members of the treatment group. Since many of the welfare recipients assigned to the treatment group did not choose to participate, column 3 shows the change in employment for those who took up the program.²⁰

Table 2: Change in Employment Status of Treatment and Control Group Members between the Baseline and 36-Month Surveys

| | (1) | (2) | (3) |
|-----------------------------|---------------|-----------------|---------|
| | | Treatment Group | |
| | Control Group | All | Take-up |
| Months of experience | 10.6 | 13.2 | 24.9 |
| Hours of work | 304 | 405 | 839 |
| Per cent full-time | 0.099 | 0.196 | 0.416 |

The first row in Table 2 shows the number of months in which the treatment and control group members worked between the baseline and 36-month interviews. While the control group worked an average of 10.6 months, members of the treatment group worked nearly 30 per cent more (13.2 months.) Not surprisingly, those who took up the program worked even more. Among this subset of the treatment group, the average number of months worked is 24.9 months. Therefore, those who took up the program worked in nearly two out of the three years. Row 2 shows a similar pattern in the number of hours worked. While control group members worked an average of 304 hours between baseline and the 36-month interview, treatment group members worked about a third more hours (405 hours) and those treatment group members who took up the program worked more than twice as many hours (839 hours). The bottom row in Table 2 shows that treatment group members not only worked more, but they were more likely to work full time. This largely reflects the fact that the subsidy was available only for full-time work.

While the subsidy increased full-time work it does not seem to have had a large impact on the types of jobs held. Foley and Schwartz (2002) report that treatment group members and control group members who worked tended to find similar jobs in terms of occupations and industries. These jobs were rarely unionized and tended to pay low wages and to offer few benefits. Foley and Schwartz (2002) conclude that the similarity in jobs held by treatment and control group members reflects the low levels of education of both groups. This severely limited their opportunities. Furthermore, since employers did not know whether or not job applicants were eligible for the subsidy, the program did not alter the types of jobs offered.

¹⁹See Michalopoulos et al. (2002) for a summary of previous studies.

²⁰Taking up the program is defined as receiving an SSP supplement during at least one month. The decision to take up the program is clearly endogenous. Therefore, those assigned to the treatment group who do not take up the program are still included in our analysis of the effects of work on beliefs.

The similarity of jobs held by treatment and control group members is important for our study since it implies that any change in beliefs reflects differences in work, not differences in the types of jobs held. This is important in interpreting our results. If the earnings subsidy had led to better jobs that were only available as long as the subsidy was in place, then changes in beliefs might be temporary. If, on the other hand, it is work and not the type of job that matters, then the change in beliefs is likely to be longer lasting.

Table 3 presents descriptive statistics that confirm the popular notion that working gives a person a greater sense that they are in control of the factors that affect them. In the language of social psychologists, welfare recipients who work may have a greater locus of control than non-working welfare recipients or welfare recipients who are working part time. Entries in each row show the proportion who agree or agree strongly with each statement. The first row shows that 31.8 per cent of unemployed welfare recipients agreed with the statement “There is little that I can do to change many of the important things in my life.” In contrast, only 21.6 per cent of employed welfare recipients agreed with this statement and the 10.2 percentage point difference is statistically significant. Full-time workers are even less likely to concur (17.9 per cent), and the difference with part-time workers is again statistically significant. The pattern of full-time workers being the least likely to agree and unemployed workers being the most likely to agree holds for all four statements. While these descriptive statistics should not be interpreted as showing a causal relationship between work and beliefs, they do show that there is a correlation that needs to be explained.

Table 3: Percentage Who Agree With Statements at the Baseline Interview, by Employment Status

| | Part Time | Full Time | Employed | Unemployed | Unemployed -Employed | Part Time- Full Time |
|----------------------|-----------|-----------|----------|------------|-----------------------------|------------------------------|
| Can't change | 0.235 | 0.179 | 0.216 | 0.318 | 0.102 (0.014) [0.000] | 0.057 (0.026) [0.026] |
| Can't control | 0.249 | 0.200 | 0.232 | 0.309 | 0.077 (0.014) [0.000] | 0.049 (0.027) [0.067] |
| Pushed around | 0.353 | 0.339 | 0.349 | 0.401 | 0.053 (0.016) [0.001] | 0.014 (0.030) [0.847] |
| Angry | 0.476 | 0.482 | 0.478 | 0.557 | 0.079 (0.017) [0.000] | -0.006 (0.032) [0.847] |

Notes: Standard error of difference is in parentheses (.).
Two-sided p-values are in square brackets [].

EFFECT OF WORK ON BELIEFS

Our estimates of the causal effect of accumulated work experience on answers to these four questions are shown in Tables 4 and 5. Table 4 shows the impact of the number of hours worked on the probability that the respondent reports greater disagreement (or less agreement) with each statement at the 36-month interview than at the baseline interview.²¹ The converse of greater disagreement is not greater agreement since roughly half of all respondents report the same level of agreement at the two interviews. Table 5, therefore, shows the impact of work on the probability that the respondent reports greater agreement (or less disagreement) with each statement.

Table 4: Probit Estimates of the Probability of Greater Disagreement With the Statement at 36-Month Interview Than at the Baseline Interview

| | Can't Change | | Can't Control | | Pushed Around | | Angry | |
|---------------------------|------------------------------|------------------------------|------------------------------|------------------------------|------------------------------|------------------------------|------------------------------|------------------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | Probit | Probit IV |
| Hours/1000 | 0.037 (0.013) [0.002] | 0.215 (0.115) [0.031] | 0.009 (0.014) [0.266] | 0.081 (0.120) [0.251] | 0.041 (0.013) [0.001] | 0.167 (0.114) [0.072] | 0.093 (0.015) [0.000] | 0.373 (0.136) [0.003] |
| Age | -0.013 (0.004) [0.002] | -0.017 (0.005) [0.001] | 0.002 (0.005) [0.352] | 0.00 (0.006) [0.500] | 0.001 (0.004) [0.448] | -0.003 (0.005) [0.313] | -0.001 (0.005) [0.423] | -0.008 (0.007) [0.105] |
| Age squared | 0 (0.000) [0.014] | 0 (0.000) [0.004] | 0 (0.000) [0.285] | 0 (0.000) [0.443] | 0 (0.000) [0.325] | 0 (0.000) [0.406] | 0 (0.000) [0.384] | 0 (0.000) [0.189] |
| British Columbia | 0.006 (0.012) [0.313] | 0.01 (0.013) [0.218] | 0.025 (0.013) [0.028] | 0.026 (0.013) [0.024] | 0.005 (0.012) [0.327] | 0.008 (0.012) [0.258] | -0.022 (0.014) [0.057] | -0.016 (0.015) [0.136] |
| Female | -0.031 (0.031) [0.154] | -0.028 (0.031) [0.180] | -0.054 (0.033) [0.045] | -0.053 (0.033) [0.049] | -0.025 (0.031) [0.199] | -0.023 (0.031) [0.219] | 0.085 (0.032) [0.006] | 0.088 (0.032) [0.006] |
| Speaks French | 0.015 (0.018) [0.197] | 0.006 (0.019) [0.369] | -0.016 (0.018) [0.199] | -0.019 (0.019) [0.163] | 0.017 (0.018) [0.175] | 0.01 (0.187) [0.289] | -0.001 (0.020) [0.471] | -0.015 (0.022) [0.247] |
| Number of children | -0.007 (0.007) [0.144] | 0.001 (0.009) [0.447] | 0.001 (0.007) [0.450] | 0.004 (0.009) [0.319] | -0.002 (0.007) [0.372] | 0.004 (0.009) [0.337] | -0.006 (0.008) [0.227] | 0.007 (0.010) [0.244] |
| Observations | 4,958 | 4,958 | 4,958 | 4,958 | 4,958 | 4,958 | 4,958 | 4,958 |

Notes: Marginal effect on probability evaluated at means.

Standard errors are in parentheses ().

P-values are in square brackets [].

²¹The Appendix presents the cross-tabulation of the raw data (i.e. the five-part answers from “strongly agree” to “strongly disagree” for each of the questions) reported at the baseline and the 36-month interviews.

Table 5: Probit Estimates of the Probability of Greater Agreement With the Statement at the 36-Month Interview Than at the Baseline Interview

| | Can't Change | | Can't Control | | Pushed Around | | Angry | |
|---------------------------|------------------------------|------------------------------|------------------------------|------------------------------|------------------------------|------------------------------|------------------------------|------------------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | Probit | Probit IV |
| Hours/1000 | -0.037 (0.014) [0.005] | -0.077 (0.122) [0.266] | -0.035 (0.013) [0.005] | -0.198 (0.115) [0.044] | -0.033 (0.014) [0.012] | -0.334 (0.129) [0.005] | -0.056 (0.013) [0.000] | -0.199 (0.107) [0.031] |
| Age | 0.011 (0.005) [0.014] | 0.012 (0.006) [0.022] | 0.001 (0.004) [0.406] | 0.005 (0.005) [0.166] | -0.005 (0.005) [0.163] | 0.003 (0.006) [0.310] | 0.006 (0.004) [0.087] | 0.009 (0.005) [0.031] |
| Age squared | 0 (0.000) [0.046] | 0 (0.000) [0.056] | 0 (0.000) [0.469] | 0 (0.000) [0.230] | 0 (0.000) [0.145] | 0 (0.000) [0.313] | 0 (0.000) [0.169] | 0 (0.000) [0.061] |
| British Columbia | 0.022 (0.013) [0.043] | 0.022 (0.013) [0.053] | 0.023 (0.012) [0.029] | 0.02 (0.013) [0.059] | 0.044 (0.013) [0.001] | 0.038 (0.014) [0.004] | 0.025 (0.011) [0.012] | 0.022 (0.012) [0.028] |
| Female | -0.004 (0.031) [0.454] | -0.004 (0.031) [0.447] | 0.027 (0.028) [0.171] | 0.025 (0.028) [0.197] | 0.035 (0.031) [0.132] | 0.031 (0.032) [0.173] | 0.003 (0.027) [0.460] | 0.001 (0.027) [0.492] |
| Speaks French | -0.036 (0.018) [0.027] | -0.035 (0.019) [0.040] | 0.034 (0.019) [0.030] | 0.042 (0.020) [0.015] | -0.001 (0.019) [0.479] | 0.014 (0.021) [0.262] | -0.001 (0.016) [0.480] | 0.006 (0.018) [0.366] |
| Number of children | -0.011 (0.007) [0.076] | -0.012 (0.009) [0.092] | -0.002 (0.007) [0.404] | -0.009 (0.009) [0.145] | 0.014 (0.007) [0.028] | 0.000 (0.010) [0.493] | -0.005 (0.006) [0.225] | -0.012 (0.008) [0.078] |
| Observations | 4,958 | 4,958 | 4,958 | 4,958 | 4,958 | 4,958 | 4,958 | 4,958 |

Notes: Marginal effect on probability evaluated at means.

Standard errors are in parentheses ().

P-values are in square brackets [].

For each of the four questions about beliefs we estimate probit models that include the number of hours worked between the baseline and the 36-month interview and a set of control variables. These variables are used in models that explain whether the respondent reported greater disagreement (Table 4) or greater agreement (Table 5) with each of the statements.

For each statement we show two columns of estimates. The first column shows the results of estimating probit equations that ignore the potential simultaneity of work and beliefs. The second column addresses the potential endogeneity issue by using experimental status as an instrument for work. The estimates in these columns can be viewed in terms of the standard Instrumental Variables (IV) procedure used in linear models.²² The first stage is to estimate the number of hours worked as a function of control variables plus the identifying instrument, which is a dummy variable equal to one if the person was randomly assigned to

²²While we use the estimator developed in Newey (1987), the described two-step estimator developed by Rivers and Vuon (1988) is also consistent.

the treatment group.²³ These estimated coefficients are then used to form predicted values of hours worked, which are then included as a regressor in the probit equation.²⁴ This yields consistent estimates of the causal affect of work on beliefs.

For ease of interpretation we show the derivatives evaluated at the means, rather than the probit coefficients. Standard errors are shown in parenthesis and the corresponding one-sided p-values are shown in brackets. We start by focusing on the probit models that do not correct for the potential endogeneity of work. These are in columns 1, 3, 5 and 7, labelled “Probit.” For all four questions the derivatives on the change in hours worked are of the expected sign, positive in Table 4 and negative in Table 5.²⁵ These point estimates indicate that welfare recipients who spent more time employed were more likely to report that they disagreed (and were less likely to report that they agreed) with each statement. All but one of the coefficients in these columns is statistically significant. This is not surprising since the estimated coefficients capture the effect of beliefs on work as well as the effects of work on beliefs, which were shown to be significant in Table 4.

The IV estimates, shown in columns 2, 4, 6, and 8, suggest that at least part of this correlation reflects the causal impact of work on beliefs. While the IV standard errors are larger, five of the eight coefficients continue to be significant at the 0.05 level, and one is significant at the 0.10 level. Only one of the coefficients changes from being significant before instrumenting for work to insignificant in the IV estimates. The fact that the coefficients on work continue to be significant in almost all cases when we use only the exogenous variation in work suggests that the relationship is not spurious.

The effects are not only statistically significant, but also large. When the mean change in hours is multiplied by the derivatives in Table 4 this yields large predicted increases in the probability of greater disagreement with each of the statements. The increase ranges from 0.03 (can’t control) to 0.13 (angry). The impact of work on the probability of less agreement with each statement is also large, ranging from 0.03 to 0.12.

Finally, we explore differences across demographic groups in the effect of work on beliefs. For example, one might expect that younger sample members were more flexible in their beliefs and might be more likely to change their beliefs as a result of work. In order to keep the analysis manageable we use the three questions on locus of control to form an index and use this index, instead of the individual questions, as the dependent variable in a set of Ordinary Least squares (OLS) and IV regressions for different demographic groups.²⁶

²³The first-stage regression of the change in hours worked on the experimental dummy and the other covariates indicates that the experimental dummy is not a weak instrument. The estimated coefficient of 0.102 has a standard error of 0.012, which yields a t-statistic of 8.1.

²⁴The major difference between the linear IV procedure and the two-stage procedure for discrete outcomes proposed by Rivers and Vuon (1988) is that the residuals from the first-stage regression as well as the predicted values are included in the second-stage probit equation.

²⁵Greater disagreement indicates that the respondent believed that she was better able to change her life, was more in control, or was less likely to believe that she was treated unfairly or pushed around. Therefore, the expected sign on accumulated work experience is positive since increases in this variable should lead to greater disagreement with each statement.

²⁶We construct the index by adding the responses to the three questions on locus of control. Since the answers to each question can take values from 1 to 4, the index can take values between 3 and 12.

Table 6 presents OLS estimates of the relationship between work and this index of beliefs.²⁷ Column 1 shows estimates for all individuals while columns 2 and 3 present estimates for persons disaggregated by age and columns 4 and 5 disaggregated by presence of children. The cut-off age of 30 is chosen to test whether beliefs become ingrained once respondents enter their 30s.

Table 6: Locus of Control Index (OLS)

| | (1) | (2) | (3) | (4) | (5) |
|---------------------------|------------------------------|------------------------------|------------------------------|------------------------------|------------------------------|
| | All | Age | | Number of Children | |
| | | <30 | 30+ | <2 | 2+ |
| Hours worked/1,000 | 0.213 (0.057) [0.000] | 0.301 (0.092) [0.001] | 0.163 (0.073) [0.014] | 0.245 (0.079) [0.001] | 0.187 (0.084) [0.013] |
| Age | -0.018 (0.020) [0.187] | 0.013 (0.122) [0.458] | -0.001 (0.050) [0.496] | -0.026 (0.025) [0.152] | -0.007 (0.035) [0.425] |
| Age squared | 0.000 (0.000) [0.395] | -0.001 (0.003) [0.369] | 0.000 (0.001) [0.404] | 0.000 (0.000) [0.234] | 0.000 (0.000) [0.353] |
| British Columbia | -0.088 (0.053) [0.049] | -0.008 (0.081) [0.463] | -0.146 (0.071) [0.020] | -0.111 (0.073) [0.066] | -0.058 (0.772) [0.227] |
| Female | -0.155 (0.129) [0.116] | 0.074 (0.326) [0.410] | -0.201 (0.141) [0.077] | 0.079 (0.165) [0.315] | -0.496 (0.208) [0.009] |
| Speaks French | 0.043 (0.077) [0.287] | -0.034 (0.118) [0.386] | 0.091 (0.102) [0.188] | -0.056 (0.101) [0.288] | 0.174 (0.119) [0.073] |
| Number of children | -0.01 (0.030) [0.377] | 0.039 (0.059) [0.253] | -0.021 (0.036) [0.276] | 0.15 (0.225) [0.253] | 0.006 (0.053) [0.458] |
| Constant | 0.52 (0.347) [0.067] | -0.038 (1.482) [0.490] | 0.31 (1.022) [0.381] | 0.207 (0.472) [0.331] | 0.712 (0.621) [0.126] |
| Observations | 4,761 | 2,069 | 2,692 | 2,435 | 2,326 |
| R-squared | 0.008 | 0.007 | 0.007 | 0.008 | 0.011 |

Notes: Standard errors are in parentheses ().

P-values are in square brackets [].

Consistent with the prior tables, Table 6 shows that the index of locus of control increased more for persons who worked more. The relationship between work and this index of locus of control is significant for all groups. If one does not correct for endogeneity of work hours one would conclude that beliefs were as likely to be affected by work for older sample members as for younger sample members.

²⁷The number of observations is smaller than in Tables 4 and 5 because this index can only be constructed for persons with valid responses to all three questions used to construct the index.

Table 7 shows that correcting for endogeneity of work has a substantial effect on the estimated coefficients. The first thing to notice is that the estimated impact of work on our index of locus of control is substantially larger after correcting for endogeneity. For example, the coefficient for all persons, shown in column 1 of each table increases by more than a factor of four (from 0.21 to 1.06). While the estimated standard errors also increase, the IV coefficients are still statistically significant.

Table 7: Locus of Control Index (IV)

| | (1) | (2) | (3) | (4) | (5) |
|---------------------------|------------------------------|------------------------------|------------------------------|------------------------------|------------------------------|
| | All | Age | | Number of Children | |
| | | <30 | 30+ | <2 | 2+ |
| Hours worked/1,000 | 1.059 (0.494) [0.016] | 1.646 (0.661) [0.007] | 0.474 (0.747) [0.263] | 1.111 (0.694) [0.055] | 1.081 (0.714) [0.065] |
| Age | -0.043 (0.025) [0.044] | -0.021 (0.129) [0.435] | -0.013 (0.059) [0.412] | -0.052 (0.033) [0.058] | -0.033 (0.042) [0.212] |
| Age squared | 0.000 (0.000) [0.109] | -0.001 (0.003) [0.424] | 0.000 (0.001) [0.489] | 0.001 (0.000) [0.087] | 0.000 (0.001) [0.383] |
| British Columbia | -0.075 (0.055) [0.087] | 0.009 (0.085) [0.457] | -0.139 (0.073) [0.029] | -0.105 (0.075) [0.082] | -0.032 (0.082) [0.346] |
| Female | -0.135 (0.133) [0.156] | 0.271 (0.356) [0.223] | -0.201 (0.141) [0.078] | 0.104 (0.170) [0.270] | -0.477 (0.214) [0.013] |
| Speaks French | -0.004 (0.083) [0.483] | -0.147 (0.136) [0.139] | 0.081 (0.105) [0.222] | -0.133 (0.120) [0.134] | 0.167 (0.122) [0.086] |
| Number of children | 0.033 (0.040) [0.201] | 0.153 (0.083) [0.032] | -0.01 (0.045) [0.413] | 0.107 (0.234) [0.324] | 0.054 (0.067) [0.210] |
| Constant | 0.522 (0.355) [0.071] | -0.257 (1.562) [0.435] | 0.395 (1.046) [0.353] | 0.284 (0.487) [0.280] | 0.741 (0.636) [0.122] |
| Observations | 4,761 | 2,069 | 2,692 | 2,435 | 2,326 |

Notes: Standard errors are in parentheses (.).
P-values are in square brackets [].

Looking across subgroups shows that the OLS estimates in Table 6 and the IV estimates in Table 7 give similar differences between persons with and without children. The coefficients for both groups are larger in Table 7 than Table 6 but these coefficients show that the effect of work on beliefs is similar for persons with two or more children or less than two children. Correcting for endogeneity, however, has a substantial effect on differences across age groups. Table 7 shows that the effect of work on beliefs is no longer statistically significant for older sample members once we correct for endogeneity. This indicates that the causal impact of work on beliefs is limited to those under 30 years of age.

Conclusions

Standard concepts in social psychology and in economics are both consistent with the theory that getting welfare recipients to work can change their beliefs. Social psychologists place considerable emphasis on the role played by success. Welfare recipients who do not think they can successfully achieve a goal do not try, even if they desire to reach that goal. Showing welfare recipients that they can be successful in the labour market may, therefore, have positive repercussions.

A parallel economic argument can be made using the concepts of imperfect information and learning. Some welfare recipients may be pessimistic about their abilities to succeed in the world of work. If they enter the labour market, they obtain new information. This can lead to an updating of information if the reality does not match prior beliefs. If this is the case, then getting pessimistic welfare recipients into the world of work can have positive repercussions. Overly optimistic welfare recipients may, however, find that their optimism is not met, so for them, work leads to greater pessimism.

While a plausible argument can be made that work can lead to more optimistic beliefs, the issue can ultimately be resolved only by going to the data. The difficulty in estimating the causal impact of work on beliefs is that the two are simultaneously determined. Work can affect beliefs, but beliefs are also likely to affect work. The raw correlation between these two outcomes can, therefore, reflect either (or both) direction of causation. The problem has been to find an exogenous source of variation in work. We address this potential problem by using data from a unique Canadian experiment in which a random sample of welfare recipients were given large earnings subsidies that led to large increases in work. We use this exogenous source of variation in work to estimate the causal impact of work on a set of measures of beliefs. Our results indicate that this exogenous increase in hours worked did lead to a change in beliefs, especially among the young.

Appendix

Table 8 shows the cross-tabulation of answers given to each of the questions at the baseline interview and at the 36-month interview. Rows show the percentage of persons giving each answer at the 36-month interview classified by their answers to these same questions at the baseline interview. For example, 39.1 per cent of the persons in the treatment group who strongly agreed with the statement “There is little that I can do to change many of the important things in my life,” at baseline, answered that they disagreed with this statement at the 36-month interview. The corresponding number for the control group is 31.4 per cent.

Table 8: Strength of Agreement/Disagreement of the Baseline and 36-Month Surveys

| | 36-Month Survey | | | | | | | |
|-----------------------|-----------------|-------|----------|-------------------|----------------|-------|----------|-------------------|
| | Control | | | | Treatment | | | |
| | Strongly Agree | Agree | Disagree | Strongly Disagree | Strongly Agree | Agree | Disagree | Strongly Disagree |
| Can't Change | | | | | | | | |
| Baseline | | | | | | | | |
| Strongly agree | 17.7 | 43.1 | 31.4 | 7.8 | 21.7 | 32.6 | 39.1 | 6.5 |
| Agree | 8.5 | 51.4 | 35.5 | 4.7 | 8.8 | 44.0 | 42.2 | 4.9 |
| Disagree | 3.4 | 26.8 | 57.7 | 12.1 | 3.1 | 24.8 | 58.7 | 13.4 |
| Strongly disagree | 4.5 | 10.2 | 52.1 | 33.2 | 3.6 | 13.4 | 48.6 | 34.4 |
| Little Control | | | | | | | | |
| Baseline | | | | | | | | |
| Strongly agree | 11.4 | 32.5 | 46.5 | 9.7 | 10.9 | 32.7 | 43.6 | 12.9 |
| Agree | 5.7 | 38.5 | 51.7 | 4.1 | 4.8 | 37.6 | 51.3 | 6.3 |
| Disagree | 1.7 | 19.8 | 65.3 | 13.3 | 2.0 | 18.7 | 65.4 | 13.9 |
| Strongly disagree | 1.5 | 6.2 | 59.2 | 33.1 | 0.3 | 9.2 | 51.2 | 39.3 |
| Pushed Around | | | | | | | | |
| Baseline | | | | | | | | |
| Strongly agree | 40.9 | 41.9 | 14.0 | 3.2 | 25.3 | 42.9 | 26.4 | 5.5 |
| Agree | 16.2 | 45.2 | 34.0 | 4.7 | 12.8 | 48.4 | 35.1 | 3.7 |
| Disagree | 5.4 | 30.1 | 55.4 | 9.0 | 5.2 | 26.9 | 57.8 | 10.2 |
| Strongly disagree | 1.5 | 15.7 | 51.5 | 31.3 | 5.7 | 17.7 | 47.6 | 29.0 |
| Angry | | | | | | | | |
| Baseline | | | | | | | | |
| Strongly agree | 23.2 | 43.4 | 28.8 | 4.6 | 20.1 | 41.7 | 30.3 | 7.9 |
| Agree | 11.2 | 43.0 | 42.3 | 3.5 | 9.2 | 42.5 | 44.5 | 3.9 |
| Disagree | 4.2 | 22.7 | 62.8 | 10.3 | 3.6 | 22.4 | 61.0 | 13.0 |
| Strongly disagree | 0.8 | 12.9 | 52.4 | 33.9 | 4.2 | 10.0 | 44.2 | 41.7 |

Note: Row percentages shown.

Information in Table 8 is the basic data used to construct the summary measures in the body of the paper. Entries on the diagonal are for persons who gave the same answer at the 36-month and baseline interview. Entries above the diagonal are for persons who were more likely to disagree with the statement at the 36-month interview than at the baseline.²⁸ Entries below the diagonal are for persons who were more likely to agree with each statement.

Table 8 confirms that treatment group members were more likely than control group members to increase their disagreement with each statement, even when persons are classified by their detailed answers to these questions. This table also shows that these changes in beliefs occurred throughout the distribution. While 13 of the 16 entries show the probability of strong disagreement at the 36-month interview higher for treatment group members than control group members, the change in beliefs also holds for smaller changes. For example, consider changes on the first off diagonal, which measures one unit change in beliefs (e.g. from “agree” to “disagree”). Nine out of the twelve entries show treatment group members were more likely to report one unit greater disagreement at the 36-month interview than at the baseline.

²⁸The overall proportion reporting greater disagreement with each question also depends on the proportion of persons in each row.

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