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ABSTRACT

A critical impediment to research on school-to-work programs has been the absence of large representative data sets with information on such programs. In contrast, the new NLSY (NLSY97) offers researchers opportunities to analyze direct evidence on school-to-work programs. In the NLSY97, individuals are asked a set of survey questions about "programs schools offer to help students prepare for the world of work," and an accompanying survey includes information on school-to-work programs offered by schools attended by the interviewees. These data, coupled with observations on multiple individuals in the same schools, potentially allow researchers to estimate the effects of school-to-work programs on individuals while accounting for possible bias from selection into these programs, although apparent data problems pose some limitations. Because Round One of the NLSY97 covers workers only up to age 17, this paper focuses on the consequences of school-to-work programs for youth employment and schooling decisions while in high school, and students' subjective assessments of the likelihood of future schooling and work behavior.

Overall, the evidence does not point to a causal effect of school-to-work program participation on behavior likely associated with future college attendance. On the other hand, school-to-work program participation does appear to have positive effects on educational attainment in terms of respondents' subjective probabilities of obtaining a high-school diploma. More in accordance with the traditional view of school-to-work programs, the data indicate that participation in these programs increases the perceived likelihood of future labor market activity, both for the year following the survey and at age 30. However, school-to-work programs do not appear to boost the probability of current employment.

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I. Introduction

Early labor market experiences of youths in the U.S. are often characterized as "churning" or "milling about" in the form of initial periods of joblessness or a series of "dead-end" jobs (U.S. General Accounting Office, 1990), or as "floundering" from one job to another, representing a "waste of human resources" (Stern, et al., 1994). This characterization of U.S. labor markets has motivated policy initiatives to address the school-to-work transition by helping to transform the youth labor market from the current "chaotic" system in the U.S. to a more "orderly" system, like that of the German apprenticeship system or the informal contracts between Japanese schools and employers, in which youths leave school for further career training or stable employment (Commission on the Skills of the American Workforce, 1990; Hamilton, 1990; Lerman and Pouncy, 1990; Glazer, 1993; and other work reviewed in Heckman, 1993). This perspective on youth labor markets in the U.S. probably provided much of the impetus for the 1994 School-to-Work Opportunities Act (STWOA), which sought to create an integrated system of youth education, job training, and labor market information, to provide a faster and more successful transition from school to stable employment.

The need for school-to-work programs or other means of increasing early job market stability is predicated on the view that the chaotic nature of youth labor markets in the U.S. is costly. However, there is a counter-argument to this negative view of the turbulent nature of youth labor markets in the U.S. Specifically, there is evidence that workers receive positive returns to job shopping (e.g., Topel and Ward, 1992), presumably as workers (and employers) learn about their skills, aptitudes, and interests by trying different jobs, leading to increasingly better matches as young workers move through a series of jobs. Thus, it remains an open question whether school-to-work programs or other means of increasing early job market stability would result in greater ultimate labor market success, as compared with the current functioning of youth labor markets in the U.S.

Answering this question is extremely difficult, probably requiring carefully designed experiments. A compendium of research by Stern, et al. (1994) suggests that researchers are a long way from a definitive answer to this question. An alternative, indirect approach that has been taken to address the need for school-to-work programs is to explore the effects of early labor market stability–which school-to-work programs are supposed to encourage–on adult labor market outcomes (Gardecki and Neumark, 1998; Neumark, 1998).

A critical impediment to research on school-to-work programs has been the absence of large representative data sets with information on such programs, in part, no doubt, because such programs were less prevalent and of less interest to policy makers in the past. In contrast, the new NLSY (NLSY97) potentially offers researchers opportunities to analyze direct evidence on school-to-work programs. In the NLSY97, individuals are asked a set of survey questions about "programs schools offer to help students prepare for the world of work." For the most part, these questions cover the types of programs in which individuals participated, including things such as job shadowing, mentoring, cooperative education, and internships or apprenticeships. In addition to the survey questions administered to individuals, a survey of schools also includes information on school-to-work programs offered by schools. As explained later in the paper, these data, coupled with observations on multiple individuals in the same schools, potentially allow researchers to estimate the effects of school-to-work programs on individuals, while accounting for possible bias from selection into these programs, a well-known and potentially serious problem in program evaluation.

The long-term goal of this research agenda is to follow members of the NLSY97 through their early years leaving school and entering the labor market, to evaluate the full set of effects of school-to-work programs. Over time, the consequences of school-to-work programs for adult wages and employment will be examined. However, because Round One of the NLSY97 covers workers only up to age 17, and in the spirit of an "Early Results" conference, attention at this point is limited to consequences of school-to-work programs for youth employment and schooling decisions while in high school, and students' subjective assessments of the likelihood of future schooling and work behavior. Thus, this paper uses the NLSY97 to begin to assemble evidence on some of the consequences of school-to-work programs, and to attempt to lay the groundwork for future analyses.

II. Existing Research

Research on School-to-Work Programs

The existing research basis for the conclusion that school-to-work programs would improve labor market outcomes is weak. A 1994 report of the National Center for Research in Vocational Education (NCRVE) provides a thorough compendium of research on school-to-work programs (Stern, et al., 1994). Based on this compendium, in our view it is safe to say that there is as yet relatively little persuasive evidence of positive impacts of these programs on adult labor market outcomes. First, few studies have focused on labor market outcomes more than a year or two after completion of the programs. But there is limited evidence that over a period of a few years any beneficial effects of some types of school-to-work programs dissipate, as comparison group members find good jobs on their own. Second, many of these studies do not construct a reasonable comparison group, let alone consider the problem of selection into the program on the basis of unobserved characteristics that might also be correlated with outcomes. Third, even those studies that attempt to construct a good comparison group find no beneficial short-term labor market effects, with the possible exception of those students who remained with the employer with whom they "apprenticed" during the program. Finally, some of the evidence suggests that school-to-work programs may discourage post-secondary education. In general, though, based on the existing evidence we regard any definitive conclusions about school-to-work programs as premature, awaiting better evaluation studies.

The recent Mathematica evaluation of the STWOA does not even attempt to provide a true program evaluation, arguing that school-to-work implementation "generally involves broad and diverse initiatives that in varied ways touch most or all students, so it is impossible to distinguish between participants and an unaffected comparison group" (Hershey, et al., 1999, p. xviii). This study does present some evidence that is intended to speak to the effects of school-to-work programs. For example, the study reports that students in paid positions arranged as part of school-to-work programs are employed in a wider array of industries and receive more training than other students in paid positions, and concludes that "Schools develop positions in a wide range of industries, increasing the chances that students can work in a setting relevant to their career interests" (Hershey, et al., 1999, p. 89). However, nothing in the evidence implies that students who found these jobs as part of school-to-work programs would not have found the same types of jobs absent such programs; students most likely to do so may simply have sorted into school-to-work programs. As will become clear later, we think that research attempting a program evaluation based on treatment and control groups is important and possible, although not simple to execute, and prone to problems identified in the literature on the evaluation of other types of social programs (e.g., Heckman, et al., 1999).[1] Our claim is not that the Mathematica study fails to provide valuable information. Clearly policy makers need information on problems encountered in implementation, descriptions of populations served, etc. But we do contend that attempts to estimate the causal effects of school-to-work programs are also critical, and in this paper we consider how the NLSY97 might be used to do so.[2]

Research on the Consequences of Early Labor Market "Instability"

A different but related goal that has been addressed in earlier research without the benefit of data on school-to-work programs is whether youths who appear to have more unstable or "chaotic" early labor market experiences suffer adverse labor market consequences as adults. It seems that, minimally, a case for attempting to replace current methods of job shopping with programs that induce earlier job or labor market stability requires evidence that those youths who experience "floundering about" in their early years in the labor market suffer longer-term consequences.

Earlier research considered evidence on the relationship between early job market stability and adult wages by exploring the correlations between a wide range of individuals' youth labor market experiences and their labor market outcomes as more mature adults, in a multivariate framework that controlled for other adult

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[1]Given that the Mathematica study documents variation across school districts and states in the incidence of school-to-work partnerships supported by grants under the STWOA, there seems to be a natural way to construct treatment and control groups. Instead, the study focuses on eight states all of which have a high percentage of school districts covered by such partnerships.

[2]A recent paper by Bassi and Ludwig (2000) provides some descriptive information on costs and benefits to firms of a select handful of school-to-work programs. However, there is no way to know how these compare to other strategies firms would have chosen had they not implemented school-to-work. Moreover, as the authors note, the sample is not representative, so we cannot draw inferences about costs and benefits to the population of firms.

characteristics (Gardecki and Neumark, 1998, hereafter GN). GN report estimates of wage regressions for individuals in their late-20s to mid-30s, controlling for the usual ingredients of wage regressions–schooling, experience, etc.–at the time the wage was measured, but adding in measures of youth labor market experiences over the first five years in the labor market, including number of jobs, longest job held, industry and occupation changes, etc. The results suggest that adult wages are for the most part unrelated to the stability of early labor market experiences, especially for men, although as many studies have found, training bestowed longer-term benefits. GN also look at other adult labor market outcomes, including health and pension benefits and full-time employment, and conclude similarly that early job market stability is largely unrelated to these outcomes. Such findings suggest (but by no means demonstrate) that school-to-work programs, to the extent that their purpose is to encourage more stable early labor market experiences, may not help to achieve the goals pursued by their proponents.[3]

However, because unobservables related to match quality and patterns of search and job shopping in the early years in the labor market may be related to measures of early job market stability, this evidence may not reveal the causal effect of early job market stability per se on adult labor market outcomes. Consideration of models of on-the-job search reveals that the bias in the OLS estimates can go in either direction. To remedy this problem, Neumark (1998) uses instrumental variables methods to eliminate endogeneity or heterogeneity bias in the estimated relationship between early job market stability and adult wages. Specifically, time-series and geographical variation in labor market conditions facing workers entering the labor market is used to generate instrumental variables for the job stability experienced by workers as youths. This empirical approach, in principle, yields estimates that measure the potential effects of policies that would increase early job stability. Its findings indicate that once account is taken of the endogenous determination of early job

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[3]We should not necessarily view school-to-work programs as narrowing career choices. In a Mathematica Policy Research, Inc. study funded by the U.S. Department of Education to evaluate the 1994 STWOA, Hershey, et al. note that school-to-work programs are sometimes perceived this way. They argue, however, that these programs may instead "broaden the career options students are aware of and ... encourage them to prepare for a direction of their own choice" (1999, p. 136). They suggest that qualitative evidence on implementation "approaches and priorities" for school-to-work programs is more consistent with the latter interpretation, while acknowledging that they do not have statistical evidence to support this view.

stability and adult wages, the evidence points to positive effects of early job stability on adult wages, suggesting that policies that exogenously increase early job stability might have net beneficial effects.

However, there is a compelling need for evidence on the effects of school-to-work programs, both to address this question more directly, and to obtain more insight about the effects of school-to-work programs, aside from those acting through the channel of early job market stability. The latter issue is particularly important because school-to-work programs are not necessarily geared exclusively toward placement of young workers in more permanent jobs, but also aim toward increasing information about labor markets and young workers' own skills, in order to enhance their career decision making.

III. Data on School-to-Work Programs and Outcomes in the NLSY97

School-to-Work Programs

In the NLSY97 individuals are surveyed about "programs schools offer to help students prepare for the world of work." The questions are asked of those with a highest grade attended of nine through twelve. For the most part, these questions cover the types of programs in which individuals participated (at all and most recently).[4] The school-to-work programs covered include:

(1) job shadowing
(2) mentoring (matching students to an individual in an occupation)
(3) cooperative education (combining academic and vocational studies)
(4) work in a school-sponsored enterprise
(5) tech prep (a planned program of study with a defined career focus)
(6) internships or apprenticeships
(7) other school-to-work programs.[5]

For the analysis reported in this paper, we focus for the most part simply on whether the individual participated in any school-to-work program, although we devote some attention to more-detailed information on school-to-work program participation.

In addition to the survey questions administered to individuals, the survey of schools also includes

[4]There is also information on duration and intensity of programs, and whether a participant received pay.

[5]The NLSY97 also includes information on "career major" programs, which are a sequence of courses based on an occupational goal. However, information on these programs is elicited in a different manner from the questions used for other school-to-work programs, and hence is not analyzed here. (In the school survey, in contrast, information on all types of school-to-work programs comes from a similar set of questions.)

information on each of these types of school-to-work programs offered by schools. As explained below, an attempt is made to use the school survey information to address statistical issues in inferring the causal effects of school-to-work programs.

Dependent Variables

As already noted, the range of outcomes of school-to-work programs that can be studied at this time with the NLSY97 is limited by the youth of the sample. However, there are a number of questions that relate to current enrollment and employment, and both shorter-term and longer-term expectations regarding school and work. These include:

(1) current school enrollment
(2) current employment and hours of work
(3) current wage if working
(4) usual hours per week if working
(5) whether respondent took the SAT or ACT exam
(6) whether respondent took an Advanced Placement exam
(7) subjective probability that respondent will be in regular school one year later
(8) subjective probability that respondent will work more than 20 hours per week one year later if in school then
(9) subjective probability that respondent will work more than 20 hours per week one year later if not in school then
(10) subjective probability of obtaining high school diploma by age 20
(11) subjective probability of obtaining four-year college degree by age 30
(12) subjective probability of working more than 20 hours per week at age 30.

School-to-work programs have been advocated as a means of encouraging better matches of workers to jobs, via a two-way flow of information, including general information about the workplace and information about particular employers flowing to students, and perhaps also information about students flowing to employers. As the above list indicates, the NLSY97 at this point offers little data with which to address the hypotheses that school-to-work programs have these effects. Thus, our analysis is more eclectic, dictated in large part by data availability.

One possible exception is the current wage if working (and perhaps also hours), which we might expect to be higher if a student, through a school-to-work program, has found a better match. However, this is not a strong prediction, as a better match may also entail more human capital investment and hence lower current wages. At the same time, we doubt that the central hypotheses regarding school-to-work programs concern jobs held by youths in high school. Rather, they concern jobs held after completing school, in which case information on current wage effects is not very informative from a policy perspective. Related dependent variables are the subjective probability that the respondent will work more than 20 hours per week one year later, if in school and if not in school, and that the respondent will work more than 20 hours per week at age 30. Again, these variables do not relate directly to the quality of job matches, but perhaps reflect a commitment to the workforce that school-to-work programs may nurture.

Many of the possible dependent variables concern plans regarding education. These include the subjective probabilities of being in regular school one year later, getting a high school diploma by age 20, and earning a four-year college degree by age 30; in addition, there is a measure of current enrollment, indicating whether a student has dropped out already. An alternative but possibly complementary perspective on school-to-work programs is that by teaching students about the workplace, students learn the value of education in the labor market. Thus, we might expect school-to-work programs to have some positive effects on students' plans to stay in school and obtain degrees.[6] There is also some objective information on whether students have taken tests required for admission (SAT or ACT) or credit (Advanced Placement) in college, either of which should indicate a higher likelihood of college attendance.[7]

Thus, the NLSY97 data permit examination of some hypotheses pertinent to evaluating school-to-work programs, while not encompassing all of the outcomes central to the school-to-work debate, and at this point relying more on expectations regarding the future than on realized behavior. The next section describes our empirical methods, and the following section reports the results.

[6]The STWOA calls for, among other things, "effective linkages between secondary and post-secondary education."

[7]One potential problem is that many of the students are in younger high school grades at which these tests are less likely to have been taken. Nonetheless, there are some students at all high school grades who have taken these tests, and a higher incidence of test taking, controlling for grade, is probably still indicative of a higher likelihood of college attendance.

IV. Methods

Generic Approach and the Selection Problem

The analysis of the effects of school-to-work programs will begin with individual-level data, examining the multivariate relationships between participation in school-to-work programs and the range of dependent variables described above. Generically, let Y_{ij} denote outcome Y for individual i in school j, let STW_{ij} be a dummy variable for whether the individual reports participating in any school-to-work program, let X_{ij} be a vector of characteristics describing the individual, let Z_{ij} denote a vector of characteristics describing the individual's school,[8] and let ϵ_{ij} represent a random error term. We then estimate equations of the form:

(1) $$Y_{ij} = \alpha + \beta STW_{ij} + X_{ij}\gamma + Z_{ij}\delta + \epsilon_{ij} .$$

In all cases of dichotomous dependent variables we estimate linear probability models, which can easily handle instrumental variable estimation, random and fixed effects estimation, etc.

We are interested in estimates of β, of course, which we will denote b. The obvious problem from a program evaluation perspective is that there is no compelling reason to believe that we can treat individuals as if they are randomly assigned to school-to-work programs. In particular, it is plausible that individuals choose to participate in such programs, and the participation decision may well be driven by some of the unobservable variables (such as aptitudes, interests, resources, etc.) that also influence the dependent variables Y_{ij}. To provide a specific example, an individual who believes he is unlikely to continue on to college might view a school-to-work program (such as an apprenticeship or internship) as a way to begin building human capital of value for the job that will be taken after high school. In this case, we might find spurious evidence that school-to-work programs cause individuals to forego college. In this example, and many others one can easily imagine, STW_{ij} is correlated with ϵ_{ij}, so that b is a biased estimate of the causal effect of school-to-work programs. There may also be unobserved school characteristics that bias the estimated relationship between our outcome variables and participation in school-to-work programs. For example, schools with a student

[8]Some of these may vary across individuals because of differences by school level or school program, or reporting errors, whereas others (e.g., student-teacher ratio) are school characteristics taken from other data sources, and hence are constant across individuals.

population that tends not to go to college may be more likely to offer school-to-work programs and hence elicit higher participation, but this does not imply a causal effect of school-to-work participation on schooling and work outcomes.

Proxy Variables

There are three types of solutions to this problem available in the NLSY97. The first, and perhaps preferable one, is to use as full a set as possible of proxy variables for the individual-level and school-level unobservables in ϵ_{ij} that are potentially correlated with STW_{ij}. This has two purposes. First, the specifications with the most detailed set of proxy variables give us the best estimates we can obtain of the causal effects of school-to-work programs without resorting to other methods (based on other assumptions) of correcting for selection into either participating in these programs (from the perspective of the individual) or offering these programs (from the perspective of schools). Second, comparing the estimates of β using a narrow set of control variables and a detailed set of proxy variables can help us to gauge the extent to which biases from remaining unobservables are likely.[9] Specifically, if the inclusion of the detailed proxy variables has little or no impact on the estimates of β, it is arguably less plausible (more so the more complete we think the set of proxy variables) that remaining unobservables generate a correlation between ϵ_{ij} and STW_{ij}, conditional on the included variables.

To implement this approach we estimate specifications for each of our dependent variables beginning only with demographic controls. We then add controls for student achievement (grades), family background, and parents' education,[10] and a set of school characteristics to account for the possibility that certain types of schools or school programs tend to offer school-to-work programs or attract students interested in such programs, while their students tend to follow particular career paths, such as a lower likelihood of college

[9]The distinction between proxy and other control variables is semantic. What researchers typically identify as "proxy" variables are those that capture the important–perhaps idiosyncratic–factors thought to influence a particular empirical relationship under study, aside from a narrower set of demographic and other controls available in standard data sets.

[10]Ideally, we would also like to use the ASVAB scores from the tests administered to NLSY97 respondents. However, these scores have not yet been made available to researchers.

attendance. Across the set of dependent variables we study, the estimates were generally robust to the inclusion of the wider set of controls. Moreover, the estimated effects did not uniformly move closer to zero when the full set of controls was included. However, there are some cases with sharper differences, which are noted below. While comparing the estimated coefficients of the school-to-work program participation dummy variable across alternative sets of controls is somewhat informative about the role of selection, and the robustness of the results suggests that this may not be too serious an issue, we also consider other potentially more precise ways of addressing this problem.

School Fixed Effects

The NLSY97 is a stratified random sample of primary sampling units, which are metropolitan areas or counties. All schools in the sampling units were identified and assigned a unique code. As a consequence, we can exploit the repeated observations on different individuals within the same school to control for all unobservable characteristics of schools (including individual and family characteristics that are common within schools) that might affect both participation in school-to-work programs and our dependent variables. For this estimator to provide causal effects of school-to-work programs, we have to assume that equation (1) can be written as:

(2) $$Y_{ij} = \alpha + \beta STW_{ij} + X_{ij}\gamma + Z_{ij}\delta + \eta_j + \nu_{ij} ,$$

where η_j is the part of ϵ_{ij} that is correlated with STW_{ij}, and varies only across schools but not individuals within schools, while ν_{ij} varies within schools, but is uncorrelated (conditionally) with STW_{ij}, X_{ij}, and Z_{ij}.

Under these assumptions, we can form the within-school deviations (equivalent to including dummy variables for each school), and obtain consistent estimates of the causal effects of school-to-work programs from:

(3) $$(Y_{ij} - Y_{.j}) = \beta(STW_{ij} - STW_{.j}) + (X_{ij} - X_{.j})\gamma + (\nu_{ij} - \nu_{.j}) ,$$

where the '.j' subscript indicates the mean across individuals in the same school j.[11]

[11]Note that while Z_{ij} is a vector of school characteristics some of which are reported by individuals, we have assumed in equation (3) that the school characteristics drop out of the fixed effects specification. In fact, some of these vary across individuals within schools, in which case the within-group means appear in the version of equation (3) that we estimate.

Instrumental Variable Estimates

The ability to use within-school estimates in tbe NLSY97 is potentially powerful. Nonetheless, the method requires the assumption that the part of the error term ϵ_{ij} that is correlated with STW_{ij} varies only at the school level (i.e., that in equation (3), $(v_{ij} - v_{.j})$ is uncorrelated with $(STW_{ij} - STW_{.j})$ and $(X_{ij} - X_{.j})$). This would be violated if there are individual-level unobservables that influence both participation in school-to-work programs and our dependent variables. Although we have argued (and will discuss the evidence below) that the proxy variable approach suggests a relatively unimportant role for such unobservables, they cannot be ruled out, and could still influence the estimates if their effects are not totally captured by the proxy variables available to us.

What is needed in this case is an instrumental variable for STW_{ij}, a variable that predicts participation in school-to-work programs, but is uncorrelated with the error term. A natural candidate, it seems to us, is whether the school offers a school-to-work program (denoted $OSTW_j$).[12] While this could vary across types of schools (e.g., vocational versus private) in ways that are correlated with student characteristics, it would seem that conditional on school type (and other school characteristics), this is relatively unlikely to be the case, and therefore that $OSTW_j$ may be a valid instrument for STW_{ij}. Of course, since $OSTW_j$ has no within-school variation, we can only consider the instrumental variables estimation for the equation in levels, rather than within-school deviations (i.e., with school fixed effects).

To preview some of the results, however, we find that whether a school offers a school-to-work program is of essentially no value in predicting whether an individual participates in a school-to-work program, despite the fact that there is a large fraction of schools that report offering such programs, and a large fraction that apparently do not.[13] Thus, while this instrument may satisfy the requirement of being uncorrelated with the error term, it apparently fails to be an informative instrument. We therefore also

[12]The school survey does ask about number of participants, but this information is missing very frequently even when schools respond regarding offerings of school-to-work programs. In addition, we would not want to use such information to construct an instrumental variable, since it is more likely to be correlated with the unobservables that affect individual participation.

[13]If all schools offered them, then of course this variable would have no predictive power.

consider an alternative–admittedly less satisfactory on a priori grounds–instrumental variable approach to this problem, as explained below.

V. Results

Samples

Different sets of the variables we use are available for different numbers of observations, because of eligibility for or applicability of particular questions.[14] Because this gets a bit complicated, Table 1 describes the samples used and in so doing lays out the samples we analyze. Row (1) of the table reports the overall sample size of 8984, in the first column. A number of our dependent variables are subjective expectations regarding future work and schooling. However, these are only asked of respondents born in 1980 and 1981. As the second column reports, this would appear to greatly restrict the sample available for analysis of these outcomes. However, as reported in row (2), eligibility for the school-to-work questions is also rather sharply limited, asked only of those whose highest grade attended is nine or higher, just about one-half of the sample, 76 percent of whom are eligible for the expectations questions.

As reported in rows (3)-(7), we lose relatively few observations because of invalid school-to-work data. Similarly, rows (8) and (9) indicate that we have data on respondents' demographic characteristics and achievement (grades) for most of the observations. However, once we require information on family background and school characteristics, the sample is sharply reduced. A substantial part of the reduction, shown in row (10), stems from the unavailability of a parent interview, generally because of refusal, or inability of the interviewer to obtain an interview. As shown in row (11), there are also many observations missing data on household income, according to the codebook largely because of invalid skips, "don't knows," or refusals, although most observations with a parent interview have valid data on parents' education (row (12)). Finally, row (13) indicates some loss of observations with missing information on schools. This is due mainly to "don't knows"; non-enrolled respondents are asked about their most recent schools.

[14]There is also item non-response, but conditional on a particular set of questions being asked of or applicable to a subsample, we generally choose samples with all data available for the analysis of particular specifications, to enhance comparability of estimates.

As indicated in the next four rows, fewer than half of the respondents report some work experience, and of course only this subsample is available for the analysis of wages, although we estimate some unconditional models for hours of work including those with zero hours. Row (17) reports the sample size with complete information on school-to-work, all of the explanatory variables, and all of the outcome variables, without conditioning on work. Finally, rows (18)-(21) report the numbers of observations with multiple observations available for the same school, for some of the samples we analyze. These figures indicate that for most individuals in the samples we analyze there are observations on other individuals in the same school.

Descriptive Statistics

Tables 2-4 report descriptive statistics for both control variables and outcomes, broken down by participation in a school-to-work program. Table 2 reports descriptive statistics on demographic characteristics and family background.[15,16] Overall, these data indicate that school-to-work participants are very similar to non-participants, although there are some minor demographic differences. In particular, school-to-work participants are slightly less likely to be male, white, and Hispanic.[17] On the other hand, the average family background characteristics appear nearly identical across the two groups.

Table 3 reports descriptive statistics on schools. School program, type, and level are reported by respondents, whereas size and student-teacher ratio are taken from the Quality Educational Database, a proprietary database of all primary and secondary educational institutions in the U.S. School-to-work program participation is largely unrelated to most of these school characteristics, including size, student-teacher ratio, type, and level. Not surprisingly, though, school program is related to school-to-work participation. School-to-work participants are overrepresented by a factor of 1.9 to 2.8 among students in vocational technical or

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[15]For parents' education, we report a more aggregated version of the variable than we use in the regressions.

[16]For a good deal more descriptive information on participation in school-to-work programs, see Joyce and Neumark (1999).

[17]The sex and race differences in school-to-work program participation mirror findings reported in Hershey, et al. (1999, Table C.1), based on the most recent survey (1998) of 12th graders, who probably overlap with the cohorts covered by the NLSY97.

business and career programs, or with combined academic and vocational programs. They are underrepresented among students in general programs, although not college preparatory, academic, or specialized academic programs.

Table 4 reports the raw differentials in these outcomes associated with school-to-work program participation. The first panel indicates that participants are slightly more likely to be currently enrolled. Of course this may simply reflect the fact that those currently enrolled are at greater risk of "exposure" to a school-to-work program. The second panel indicates that school-to-work program participants are more likely to be employed, and to work more hours on average, when zero values are included for non-workers. The next panel considers those with some work experience, indicating that program participants, conditional on working, earn slightly higher hourly wages (about 15 cents) and work a shade less (about .3 hours), both small differences relative to the standard errors of the means. The third panel looks at the incidence of taking SAT or ACT examinations, or Advanced Placement examinations, which are precursors to applying to college and therefore likely to predict college attendance. Here we find rather sizable differentials, especially for taking SAT or ACT examinations, as school-to-work program participants are 34 percent more likely to do so.

Finally, the last panel looks at the subjective expectations, with probabilities expressed as percentage chances, as elicited by the survey. There is little difference between participants and non-participants in the probability of enrollment one year later, but school-to-work participants do hold higher expectations of the likelihood of getting a high school diploma (by age 20) or a four-year college degree (by age 30). In addition, for all three variables regarding expectations of working 20 or more hours per week, school-to-work participants report higher subjective expectations, most notably for expectations regarding work while still in school.

Regression Estimates

<u>Contemporaneous outcomes</u>

We next report the multivariate analyses. Table 5 looks at the "contemporaneous" outcomes. The first set of regression estimates in the table (going across the columns) is simply OLS estimates with the limited set of demographic controls, followed by the estimates with the full set of control variables included; standard errors robust against heteroscedasticity and sample clustering (by school) are reported.[18] In both cases, the OLS estimates at the top of Table 5 indicate that school-to-work program participation is positively associated with current enrollment as well as current employment, although only the latter is strongly significant ($p < .05$). The employment differential is also reflected in the estimates of the unconditional hours equation ($p < .05$). However, conditional on working, there is no statistically significant differential in hours between participants and non-participants, or in wages. Because respondents who participated in school-to-work programs may have different human capital investment profiles, though, we cannot necessarily draw any inferences about productivity or match quality. Also, as noted earlier, because the enrollment effect may simply reflect exposure, we regard the principal finding to be the higher likelihood of current employment for school-to-work program participants.

Below the OLS estimates, we report random effects estimates, which are more efficient.[19] Instead of using the conventional random effects estimator, we quasi-difference the data using the formula in Hausman (1978), and then use OLS on the quasi-differenced data with the same robust standard errors as we used for the conventional OLS estimates. This allows for the possibility that the individual-level idiosyncratic error is heteroscedastic, and that there are still covariances among observations for the same school because the conventional random effects estimator assumes that for each school the covariance in the error terms within schools is the same. However, we also know that random effects estimates are weighted averages of the within and between estimates, and therefore tend to move in the direction of the fixed effects estimates (as the variance of the school-specific error goes up relative to the variance of the idiosyncratic error). In general, the evidence of higher enrollment, employment, and (unconditional) hours is a bit weaker using the random effects specifications, although the signs of the estimates are unchanged, the magnitudes fall only slightly, and the

[18]Most of these control variables were covered in Tables 2 and 3, and the footnote to the table gives more details.

[19]The fact that the standard errors of the conventional OLS estimates are robust to sample clustering explains why the standard errors do not necessarily go up in the random effects estimations.

estimated differentials for employment and unconditional hours remain statistically significant at the five- or ten-percent level.

Next, the table reports the estimates with fixed school effects. In these estimates, any evidence of positive effects of school-to-work program participation on enrollment, employment, or (unconditional) hours evaporates completely, and for all five outcomes there are no statistically significant effects of school-to-work programs. The final panel of the table reports Hausman tests of the null hypothesis that the school-specific error terms and the school-to-work participation dummy variable are uncorrelated, based on the difference between the random effects and fixed effects estimates. We use a version of the test that is robust to inefficiency of the random effects estimator.[20] We did this test for just the school-to-work variable, since we really only care about bias in the coefficient estimate of this variable.[21] It turns out that for four of the five dependent variables examined in this table, the Hausman test leads to rejection of the null at the five-percent or ten-percent significance level, with strong rejections for the three dependent variables (enrollment, employment, and unconditional hours) for which the conventional OLS estimates indicated statistically significant differentials associated with school-to-work program participation. This rejection implies that we need the fixed school effects, which in turn implies that for the dependent variables studied in this table, there is no statistical evidence of effects of school-to-work programs. That is, participation in these programs does not appear to have much impact on current enrollment, employment, hours of work (unconditional or conditional), or wages.

Short-term subjective expectations regarding school and work

The next table reports a similar set of analyses for subjective expectations regarding the following year, in particular the chance that the respondent will be in school, and will work more than 20 hours per week

[20]To do this test, we regress the quasi-differenced dependent variable on the quasi-differenced independent variables as well as the school-specific mean vector of the independent variables. The null of no selection bias implies that the vector of coefficients on the mean vector is zero, which we test using an F-test that is robust to heteroscedasticity and clustering.

[21]One argument against this approach is that if other coefficient estimates are biased, we cannot interpret the test of a single coefficient that rests on the assumption that its estimate is consistent under the null.

if in school and if not in school. The OLS estimates indicate positive differentials for each of these variables associated with school-to-work program participation, although the evidence is stronger for the work variables ($p < .01$ for work if in school, and $p < .05$ for work if not in school) than for the enrollment variable (for which the difference is insignificant). The estimates are insensitive to the inclusion of the controls. The estimated magnitude for work while in school (using the full set of controls) indicates that participants in school-to-work programs rate themselves as 5.2 percentage points more likely to work more than 20 hours per week than non-participants, ceteris paribus. In the random effects estimates, the statistical evidence consistent with school-to-work boosting the likelihood of working, especially if still in school, persists, although the relationship between the likelihood of enrollment and school-to-work participation weakens. Finally, in the fixed effects estimates the evidence of positive effects on the likelihood of working if out of school becomes statistically insignificant, although the coefficient is larger, while for work if in school the estimated coefficient falls slightly while remaining statistically significant ($p < .05$). For the work-related variables, the Hausman tests indicate that we do not need the school fixed effects. Thus, the evidence suggests that participation in school-to-work programs increases the subjective probability that respondents will work more than 20 hours a week in the next year, whether in school or not.

College-preparatory test taking

Table 7 turns to the question of whether school-to-work program participation increases the likelihood that students take SAT or ACT examinations, or Advanced Placement (AP) examinations. For both types of tests, the answer appears to be yes, although the evidence is stronger for SAT or ACT examinations. For these, the OLS (with limited or full controls), random effects, and fixed effects estimates all indicate a probability that is higher by about four or more percentage points. For the AP tests, in the OLS and random effects estimates the estimated effect is significant at the five-percent level. The fixed effects estimation results in a higher standard error and therefore an insignificant coefficient estimate, but the Hausman test indicates that the school fixed effects are not needed for this specification. Thus, it appears that school-to-work program participation has positive effects on the probability that respondents take tests used for either admission to or advanced credit at college.

Long-term subjective expectations regarding school and work

Prior to looking at the dependent variables regarding longer-term subjective expectations, including obtaining a four-year college degree, we consider some evidence on the role of the tests considered in Table 7. Although we cannot of course observe the relationship between taking these tests and actual college attendance, we can observe the relationship for the subjective probability of obtaining a four-year degree (by age 30). As documented in Table 8, taking the SAT or ACT test is positively associated with the subjective probability of getting a four-year degree, with a differential of about three percentage points. The same is not true for AP tests, once the full set of controls is included.[22]

Table 9 turns to the regressions for subjective probabilities of a high school diploma by age 20, a four-year college degree by age 30, and working more than 20 hours per week at age 30. The OLS estimates indicate statistically significant positive relationships between school-to-work program participation and expected receipt of a high school diploma ($p < .05$), and the expectation of working more than 20 hours per week ($p < .05$). The estimated coefficients for receipt of a four-year degree are positive, but not significant. These results are little changed in the random effects estimation. The fixed effects estimate of the effect of school-to-work participation on expected receipt of a high school diploma is much smaller and insignificant, and the p-value from the Hausman test (.179) suggests that we do not need the school fixed effects; however, the p-value is sufficiently low that we do not view the evidence of a positive effect on high school graduation as strong. On the other hand, the estimated effect of school-to-work participation for work at age 30 is slightly larger, and remains statistically significant ($p < .05$). Thus, the only strong evidence that emerges from this table is an apparent positive effect of school-to-work participation on work at age 30.

One thing that is curious about the findings for a four-year college degree is that Table 7 indicated a statistically significant effect of school-to-work program participation on taking the SAT or ACT, even in the

[22]In both specifications in Table 8, as well as one specification in Table 9, note that the random effects and OLS estimates are identical. This happens because the consistent estimate of the variance of school-specific error (which can be thought of as the covariance of residuals for observations in the same school) turns out to be negative.

school fixed effects estimation, while Table 8 indicated a positive relationship between taking these tests and the subjective probability of obtaining a four-year college degree, although not significant in the fixed effects estimation. So it seems that school-to-work program participation increases the probability of taking the SAT or ACT, and taking these tests increases the subjective probability of obtaining a four-year college degree, yet school-to-work program participation does not have an effect on this latter probability. It turns out, though, that the positive relationship between school-to-work program participation and taking SAT or ACT (or AP) tests is not robust across samples. When we restrict our attention to a common sample for which we can estimate both the test-taking and college degree equations, the positive effect of school-to-work program participation on test taking–particularly for the SAT or ACT test–becomes small and insignificant once we include the school fixed effects, as the Hausman test suggests we probably need to do (the p-value is .133).

Individual School-to-Work Programs and Intensity of Participation

To this point, we have lumped all school-to-work programs together. We have no prior expectations that particular types of school-to-work programs will have stronger or weaker effects on the outcomes we consider. However, it is natural to expect that variation in the intensity of participation would be positively associated with variation in the effect. The NLSY97 contains information on hours spent at the work site for the most recent school-to-work program in which the respondent participated. Some descriptive information on these hours is reported in Table 10. The table indicates two things. First, looking at the mean hours (in the second column of figures) reveals that the average number of work-site hours under either apprenticeship or internship programs or in tech prep is quite high, while hours spent in other types of programs is often quite low. Second, however, there is a high incidence of individuals reporting zero hours. To some extent, this could simply be a result of the question focusing on hours at the work site.[23] For example, this could explain low hours for mentoring, which could conceivably consist more of classroom interaction or other meetings.

[23]The set of questions is actually a bit inconsistent. The respondent is first asked "Altogether, how many weeks or days did you spend at a work site that was part of this (most recent) program?" The response is then coded as weeks or days. Following this, respondents who reported positive weeks or days are asked "How many hours per (week/day) did you spend at a work site or participating in this most recent program?" So even though the hours question, as it is asked, is not restricted to the work site, the skip pattern imposes this restriction; even if the skip pattern did not do this, however, using the weeks or days measure to obtain an estimate of total hours would have the same effect.

However, it seems difficult to understand the high incidence of zero hours for apprenticeships or internships, for school-based enterprise, or for cooperative education, all of which would appear to involve substantial work-site activity. Thus, we suspect that the hours data, and in particular the observations reporting zero hours, are somewhat unreliable.

As a result, we take two steps in trying to incorporate information on intensity of school-to-work programs. First, we do not use individual-level data on hours in the regressions, in large part because we are skeptical of reports of zero hours. Rather, we simply use the hours information to draw some broad generalizations regarding the intensity of different types of school-to-work programs. Second, we use the hours data conditional on reporting positive hours to draw these generalizations. Means for these data are reported in the last column of the table. They reveal relatively high intensity of apprenticeship or internship programs or tech prep programs (over 150 hours, on average), moderate intensity of mentoring, cooperative education, and school-based enterprise (35 to 72 hours, on average), and low intensity of the remaining types of programs (fewer than 22 hours, on average).

In Table 11, we maintain this categorization, breaking out separate effects of school-to-work programs for these three categories. We do this by adding separate dummy variables corresponding to the type of program in which the respondent most recently participated. We use the same specifications as reported in the previous tables where we used a single dummy variable for school-to-work participation, but here report only the specifications with the full set of controls, and report either random effects or fixed effects estimates, based on the Hausman test results. For most of the outcomes we study, there are no noteworthy differences across these categories of school-to-work programs. However, there are a few exceptions. Low-intensity school-to-work programs have a positive and statistically significant effect on the subjective probability of receiving a high school diploma by age 20, whereas the relationship for medium- and high-intensity programs is weaker and insignificant. Paralleling this result to some extent are those for the subjective probability of a four-year college degree by age 30. Here we find a negative effect (significant at the ten-percent level) only for the high-intensity school-to-work programs. Together, these results might be viewed as suggesting that low-intensity school-to-work programs if anything encourage education, which would be consistent with playing largely an "information" role, while high-intensity programs discourage education, perhaps encouraging work over education (keeping in mind possible biases from selection of the non-college bound into these programs). Furthermore, it is of interest that these effects of school-to-work participation on education were either not apparent or weaker when we did not distinguish school-to-work programs by intensity. Thus, information on intensity of programs, as well as other characteristics of programs, may prove to be useful. However, at this point we regard our evidence exploiting information on the intensity of participation in school-to-work programs as provisional, given some of our data concerns.[24]

Instrumental Variables?

Finally, as noted in Section IV, we are interested in exploring data on school-to-work programs offered by schools as a source of an instrumental variable for individual participation. The data for this instrumental variable come from a supplemental data collection effort that was part of the NLSY97, the School Administrator's Survey of 1996. The sample for this survey was all schools with a 12th grade located within primary sampling units (either metropolitan areas or counties) of the NLSY97. This survey was mailed to 7,985 schools. Of the 7,390 that were ultimately determined to still exist and offer 12th grade instruction, responses were received from 5,295, or 71.6 percent of eligible respondents.

One potentially serious problem with the school survey is that, aside from non-response to the school survey as a whole, there is substantial non-response to individual items on the "grid" that asks about school-to-work programs–about 14 to 20 percent per type of program. We treated overall non-respondents as missing data, as well as a small number of respondents who failed to answer general questions about school-to-work programs that preceded the detailed questions in the grid. However, for the remaining respondents we treated

[24]We also performed an analysis like that in Table 11 keeping each type of school-to-work program separate. These estimates revealed that both of the low-intensity programs (job shadowing and "other") encourage schooling, expected work for those anticipating remaining in school, and expected work at age 30. Among the moderate-intensity programs, cooperative education increases expected work for those who anticipate remaining in school, whereas the other programs (mentoring and school-based enterprise) do not. Finally, among the high-intensity programs (apprenticeship or internship, or tech prep), the significant relationships broke down upon disaggregating. Thus, to some extent the disaggregation may ask too much of the data. These results are available from the authors upon request.

individual missing responses as zero (i.e., not offering the specific program). We think this is a reasonable thing to do because the pattern of missing data is such that many schools failed to respond to a subset of items on the grid, rather than a smaller number of schools failing to respond to most items on the grid. This suggests to us that something about the survey design often led respondents to leave a particular item missing when the school did not offer that particular type of school-to-work program.

There are two requirements of the school-to-work "offering" instrumental variable. The first, which cannot be tested in the absence of other identifying information, is that whether a school offers a school-to-work program is not correlated with the error term of the equations we estimate. This is a potential problem to the extent that respondents are able to choose particular schools offering particular types of programs; in such a case, there is selection on schools based on the programs they offer, so we would be back to the same problem posed by simply using school-to-work program participation. However, it would seem that by restricting the sample–for example, to respondents attending public schools–or by conditioning on program or school type, we can minimize this concern.

The second requirement is that the instrument be able to predict the endogenous variable. In this case, our prior belief was that this requirement would be easily satisfied. We fully expected that whether a school offered a school-to-work program would help predict whether individuals in the school participate in such programs. We would of course not expect all respondents in schools offering school-to-work programs to report participation in them, but we would expect respondents *not* to report participation in schools that do *not* offer them. Of course, there is likely to be some measurement or misclassification error, as the respondent and school administrator responding to the different surveys interpret the questions differently, the administrator gives incomplete responses, or respondents were previously at different schools. However, it seems to us that under any conceivable scenario, attendance at a school that reports offering a school-to-work program should be a strong predictor of whether the individual reports participation.

To our surprise, though, this was not the case. As reported in the top panel of Table 12, whether schools offer any school-to-work programs has scarcely any predictive power for what the individual reports.

Among respondents attending schools that report offering such programs, 37.90 percent report participation, whereas among those attending schools that do not report offering such programs only slightly fewer, 35.71 percent, report participation. Driving this point home, the Pearson chi-square statistic indicates that the null hypothesis that the schools' and respondents' reports are *independent* is not rejected, with a p-value of .241.[25]

We considered a number of possible explanations of what could underlie this. First, one category of program available in both surveys is a generic "other" category, for types of programs not covered by those listed. It seems likely that respondents to different surveys might be most likely to give non-corresponding answers for this category. We therefore recomputed this contingency table re-classifying the "other" entries as no school-to-work program. However, this made no difference, as reported in the second panel of the table. Next, we considered whether the individual and school responses might correspond better for specific types of school-to-work programs that are less ambiguous, such as "school-based enterprise" or "cooperative education." If so, we might be able to look at an isolated subset of programs and come up with a good instrument. As shown in the next six panels, however, things generally look no better by program type. The difference in the fraction of respondents reporting participation between schools that do and do not report offering school-to-work programs is generally less than one percentage point, although for job shadowing and school-based enterprise the difference is larger and the p-value is below .05. Next, since schools are asked about programs offered contemporaneously, the correspondence with individual student reports might be higher using their most recent school-to-work program, rather than any program in which they ever participated, some of which may no longer be offered. The bottom six contingency tables in Table 12 explore this.[26] One of the p-values for the test of independence is notably lower (for cooperative education), but note

[25]We see no obvious reason based on the wording of questions in the survey why this problem should arise. In the individual survey, prior to being presented with a list of possible school-to-work programs, respondents are queried as follows: "Here is a list of some of the kinds of programs schools offer to help students prepare for the world of work. Have you ever participated in any of these programs through your school?" In the school survey, as a lead-in to the grid on which respondents indicate the types of programs offered, respondents are instructed: "The questions on the following pages are about work-based and career-oriented activities offered at your school." In the grid that follows, for columns corresponding to each type of school-to-work program, respondents simply answer yes or no to the question "Do you offer this activity?"

[26]Note that in these panels, the reported incidence of each type of program based on individual responses is lower, because here respondents can indicate only one (i.e., their most recent) program.

that the test rejects independence in the wrong direction, as the participation rate reported by individuals is actually higher for those whose schools do not report offering such a program. Thus, it still largely appears that schools' reported offerings of school-to-work programs have essentially no predictive power for reported participation by respondents.

Finally, we explored an alternative approach to handling the missing data on school-to-work programs in the school survey (discussed above), treating the missing cases as true missings rather than negative responses. This never affects the schools we code as offering a school-to-work program, since all we needed to code a school this way was an affirmative answer to a question regarding at least one school-to-work program. On the other hand, this gives us many fewer schools coded as not offering such programs, because now a school is coded this way only if all of the questions covering each type of school-to-work program are answered in the negative. If the treatment of the missing data in Table 12 incorrectly codes some schools as not offering a program when in fact they do, this could explain the lack of correspondence between the individual and school data in that table. However, this did not turn out to be the case, as the proportions of students reporting school-to-work program participation among schools coded as not offering programs when treating the missing data in this alternative way were very similar to those reported in Table 12, as were the results from the tests of independence.[27]

We verified further that information on school-to-work offerings could not serve as an instrument by estimating the first-stage regression that would be used in instrumental variables estimation. Specifically, we regress the dummy variable for individual school-to-work participation on a dummy variable for whether the school offers a program, and all of the other independent variables included in the regressions.[28] The F-

[27]We also considered the possibility that the school codes had been incorrectly assigned so that our match between schools and individuals was incorrect. To check this, we examined variables for which there is information in both surveys. Comparable categories for school size can be constructed from information in both data sets, with the information in the individual survey coming from the QED rather than respondents. These categorical data matched up very well between the two surveys. Perhaps more importantly, school type is reported by respondents in each of the two surveys. Dividing the categories into public and non-public, we found that 93 percent of responses corresponded. Not only does this indicate that the match is correct, it also indicates that consistent data can be collected from individuals and schools, implying that the problem may lie with the school-to-work components of the surveys per se.

[28]This, of course, is for the specifications that do not use the school fixed effects.

statistic for the school offering of a program was below one. We also examined whether particular combinations of programs reported by schools, including simply the number of programs offered (entered in a non-restricted fashion as dummy variables), would in some way provide a better prediction for the individual's response. Perhaps, for example, if a school reports offering *many* programs, this would have more predictive power for whether a student reports *any* participation. However, despite searching over many specifications, we were unable to find any predictive relationship, never finding one with an F-statistic for the instruments exceeding even one.[29]

We are surprised by the lack of correspondence between individuals' reports of school-to-work participation and school reports of offerings of school-to-work programs. In the spirit of a conference exploring the uses, quality, etc., of the new NLSY, we think it is important to report this evidence in the hope of inviting constructive feedback on either alternative uses of these data or survey design issues that could be improved upon in future waves of the NLSY97, before respondents complete high school and the chance to collect reliable school-to-work information is lost. In particular, it seems to us that it might be useful to test the individual and school instruments on a small sample of schools and their students, and, assuming that the same lack of correspondence that we find surfaces, follow up with interviews of survey respondents and perhaps school visits to try to find the sources of the discrepancies.

In the meantime, though, our evidence has two implications. The first, obvious, one is that we cannot successfully use this particular instrumental variable approach to help evaluate the effects of school-to-work program participation. The second, potentially more troubling implication is that the problem does not necessarily lie exclusively or even predominantly with the school survey. There are reasons to be wary of the school survey data, in particular the missing data problems described above. On the other hand, this does not necessarily imply that we want to accept the individual data as "truth." Rather, it is possible that in these latter data there is some misclassification of school-to-work program participation. In thinking about the potential implications for these estimates, note that this would tend to bias our estimated effects of school-to-

[29]Of course even if we could do so, this approach would be prone to data mining–finding an empirical relationship relating schools' offering to individuals' responses that was a result of chance.

work program participation toward finding no effect. Thus, for those dependent variables for which we found no effect, the conclusion might be interpreted cautiously. But for those for which we found an effect, the evidence would presumably only be stronger absent this misclassification error.

We can at this point say a little about the reliability of the data on school-to-work participation from the survey of individuals by looking at how the reported incidence of school-to-work participation varies with characteristics with which participation is known to vary. There are not many such characteristics that come to mind, in large part because of the paucity of research on this topic. However, school program and school type seem natural candidates. In particular, individuals in vocational technical or business and career programs, or in combined academic and vocational programs, should be more likely to report school-to-work participation. The same should be true of technical or vocational high schools relative to other schools.

Table 13 reports differences in the reported incidence of school-to-work participation by individuals across school program and school types. We observe some correspondence between expected and observed patterns. For school program, the rate of participation in school-to-work programs is 89 percent higher in vocational technical or business and career programs than in general programs, and 61 percent higher in combined academic and vocational programs than in general programs. Similarly, with respect to type of school, reported school-to-work participation is 32 percent higher in technical or vocational high schools than in public schools. This evidence is not necessarily decisive, since the school program and type are reported by the respondent and could reflect the same errors as the school-to-work responses. However, as best we can tell, it does appear that the respondent reports of school-to-work participation exhibit some reliability, and that we can therefore learn something about the effects of school-to-work programs from the NLSY97 data on school-to-work participation. Nonetheless, this does nothing to resuscitate the school reports as instrumental variables.

Finally, we consider one other alternative approach to instrumental variables estimation. Although we have documented our inability to use variation in whether schools offer school-to-work programs–based on school reports–as an instrumental variable for individual participation, we nonetheless have some potentially useful information on what schools offer based on the school-to-work participation of other individuals in the same school. As noted earlier, this arises because of the school cluster design of the NLSY sample. We first consider a "jackknife" instrumental variable constructed for each respondent as the mean of school-to-work participation over all individuals at the same school *except* the respondent.[30] This jackknife mean should be correlated with the respondent's own school-to-work participation, since it is likely to be affected by what the school offers, as well as by the type of students that attend the school. Whether this mean is likely to be uncorrelated with the error term in equation (1) is potentially more problematic. To the extent that its variation is driven by school decisions that are unrelated to characteristics of students, it is likely to be a valid instrumental variable (the same conditions under which direct data on school offerings would be a valid instrumental variable). But to the extent that it is driven by student characteristics, it is unlikely to be uncorrelated with the error term, since the respondent is likely to share some of the same characteristics. We also take this approach one step further, using the mean of the residual from the first-stage regression for school-to-work participation across all individuals in the cluster except the respondent. This assures that the variation in school-to-work participation is not driven by variation in the control variables describing the other individuals, their academic program, etc., but rather is driven by variation in their school-to-work participation independently of these influences.

The results of this instrumental variable estimation are reported in Table 14. We first report the OLS results using the full set of controls; these can differ from the estimates reported previously because the sample is restricted to clusters of multiple observations within schools. We then report the two alternative IV estimates, in each case also reporting the Hausman test statistic for endogeneity of the individual's school-to-work participation. There are some generalities that can be drawn from the many estimates reported in this table. First, for all specifications, the estimated standard errors of the school-to-work participation variable rise sharply, by a factor of about eight or more. Second, the alternative IV estimates are very similar, so we do not need to comment on them separately. Third, many of the estimated coefficients of the school-to-work

[30]Clearly, this implies that respondents for which there are not other individuals in the same school are dropped.

participation variable change markedly, including those for the following outcomes: usual hours worked per week currently (conditional or unconditional); the subjective probability of working more than 20 hours per week (in school or out of school); and the subjective probabilities of a high school diploma by age 20, and the work and schooling outcomes at age 30. However, as the Hausman tests indicate, in most cases there is weak evidence against the null, with p-values in the .3 and higher range. Nonetheless, there are some exceptions, with low or relatively low p-values for the hourly wage, remaining in school ("in regular school" in the next year), and the two variables for working more than 20 hours in the next year (in school or not). However, some of the sign patterns may be inconsistent, with the estimates indicating that school-to-work participation boosts work expectations of those that remain in school, while perhaps lowering work expectations for those who do not remain in school (although these latter estimates are not statistically significant).

The large standard errors and frequency of failure to reject the null suggests that we use caution in drawing any conclusions from these instrumental variable estimates. However, there are perhaps some conclusions that emerge that differ from or reinforce those reached without instrumenting. First, work in the following year for those who remain in school may be boosted by school-to-work participation. This reinforces the conclusion from the earlier estimates, with the instrumental variable estimates suggesting a larger effect; interestingly, taking the instrumental variable estimates at face value, the direction of the endogeneity bias is not in the direction we would expect if those with the highest work expectations selected into school-to-work programs. Alternatively, the reader may regard this as reason to be skeptical of the instrumental variable estimates. On the other hand, the evidence that school-to-work participation boosts work expectations conditional on leaving school no longer appears in the instrumental variable estimation, with the estimated effect if anything negative. But our sense is that the most reliable evidence at this point comes from the fixed or random effects estimates reported in earlier tables, and it is these we summarize in the conclusion.

VI. Conclusion

Overall, the evidence does not support the finding that there is a causal effect of school-to-work program participation on behavior likely associated with future college attendance–either test taking or the subjective probability of obtaining a four-year college degree. We do find such evidence prior to including school fixed effects, but it apparently arises because of a correlation between school-to-work programs and unobservables positively related to future college attendance; in contrast, once we fully control for school characteristics, the relationship is no longer present. On the other hand, school-to-work program participation does appear to have positive effects on educational attainment in terms of respondents' subjective probabilities of obtaining a high-school diploma. Although the mean subjective probability is well over 90 percent, the estimates indicate that school-to-work program participation appears to increase this probability by nearly two percentage points.

More in accordance with the traditional view of school-to-work programs, the data indicate that participation in these programs increases the perceived likelihood of future labor market activity, both for the year following the survey and at age 30. The former effects are in the three to five percentage-point range, and the latter estimates around two percentage points. However, school-to-work programs do not appear to boost the probability of current employment.

But there are three important qualifications. First, the assumptions under which the estimations with fixed school effects identify the causal effects of school-to-work programs may be overly strong. We had hoped to obtain complementary evidence using information on school-to-work programs offered by schools to construct an instrumental variable for individual participation. To our surprise, though, school offerings had *no* predictive power for individual participation. This is disappointing, because school-to-work offerings seems like a good candidate for an instrumental variable approach. But it is also a curious result that merits further investigation. As an alternative, we have implemented an instrumental variable estimator based on school-to-work participation by other survey members in the same school as the respondent; to a large extent, this estimator did not provide decisive evidence. Second, this lack of correspondence between the information on school-to-work programs reported by individuals and that reported by schools raises some questions about the overall quality of the data, including the individual-level data on which we rely in this paper. We have presented some evidence, however, suggesting that the individual-level data on school-to-work participation correspond to expected patterns across school types and school programs. Finally, most of our evidence concerns subjective probabilities of future events. It remains to future research to see how well these results for subjective expectations accord with actual behavior.

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Table 1: Sample construction

		Number of observations	Number of observations with eligibility for expectation questions
(1)	Total observations in NLSY97	8984	3565
	Condition on STW information		
(2)	Eligible for STW questions	4489	3392
(3)	Valid answer for STW participation question	4471	3377
(4)	(3) with valid answer for hours spent on STW question	4454	3364
(5)	(3) with valid answer for most recent STW category question	4456	3367
(6)	Sample with valid answer for hours spent on STW question and most recent STW category question	4439	3354
(7)	(6) with valid answer regarding wage payment for STW program	4439	3354
	Condition on explanatory variables		
(8)	(7) with demographic variables	4389	3318
(9)	(8) with student achievement variables	4340	3284
(10)	(9) with parents' interview conducted	3798	2910
(11)	(10) with household gross income	3049	2322
(12)	(11) with parents' education	3048	2321
(13)	(12) with school information	2734	2079
	Condition on outcome variables		
(14)	(13) with valid hours of work including zero hours of work	2730	2076
(15)	(13) with work experience and data on both hourly rate of compensation and usual hours of work per week	1223	1055
(16)	(13) with data on tests taken	2703	2054
(17)	(13) with complete work and schooling expectations data	2036	2036
	Condition on multiple observations within school		
(18)	(14) with multiple observations within school	2174	1655
(19)	(15) with multiple observations within school	988	856
(20)	(16) with multiple observations within school	2149	1635
(21)	(17) with multiple observations within school	1625	1625

Notes: The questions about tests are asked only of respondents whose highest grade attended is nine or higher. The questions about work and schooling expectations were asked only of the respondents who were born in 1980 and 1981 (age on 12/31/96 was 15 or higher). The sample in line (10) also requires valid answers regarding the relationship with parent or foster care provider.

Table 2: Descriptive statistics for demographic characteristics for full analysis sample

Variable name	STW program non-participant	STW program participant
Male	0.511	0.477
	(0.013)	(0.017)
White	0.772	0.748
	(0.010)	(0.014)
Black	0.128	0.156
	(0.008)	(0.011)
Native American	0.004	0.009
	(0.002)	(0.003)
Asian	0.020	0.022
	(0.004)	(0.005)
Other race	0.075	0.065
	(0.006)	(0.008)
Hispanic	0.121	0.096
	(0.008)	(0.009)
Live with both biological parents at time of interview	0.537	0.540
	(0.013)	(0.017)
Gross household income last year	53511.28	54302.48
	(1165.831)	(1423.586)
Household size	4.242	4.258
	(0.035)	(0.045)
Father's highest grade completed		
<=12	0.342	0.359
	(0.013)	(0.017)
>12	0.368	0.366
	(0.013)	(0.017)
Mother's highest grade completed		
<=12	0.489	0.459
	(0.013)	(0.017)
>12	0.429	0.466
	(0.013)	(0.017)
N	1723	1011

Notes: "Full analysis sample" refers to the sample in line (13) of Table 1. Standard errors of means are reported in parentheses. Estimates are weighted.

Table 3: Descriptive statistics for school information for full analysis sample

Variable name	STW program non-participant	STW program participant
	Full analysis sample	
School size		
<100	0.001	0.001
	(0.001)	(0.001)
100-299	0.041	0.035
	(0.005)	(0.006)
300-499	0.083	0.097
	(0.007)	(0.010)
500-749	0.140	0.129
	(0.009)	(0.011)
750-999	0.108	0.103
	(0.008)	(0.011)
1000+	0.627	0.634
	(0.013)	(0.017)
Student-teacher ratio		
<14	0.233	0.247
	(0.011)	(0.015)
14-17	0.362	0.365
	(0.013)	(0.017)
18-21	0.234	0.228
	(0.011)	(0.014)
22+	0.172	0.160
	(0.010)	(0.013)
School program		
General	0.591	0.501
	(0.013)	(0.017)
College preparatory, academic, or specialized academic	0.338	0.343
	(0.012)	(0.016)
Vocational technical or business and career	0.025	0.071
	(0.004)	(0.009)
Combination academic and vocational program	0.040	0.077
	(0.005)	(0.009)
Special education	0.004	0.002
	(0.002)	(0.002)
Other	0.003	0.006
	(0.001)	(0.002)
School type		
Public	0.965	0.967
	(0.005)	(0.006)
Technical or vocational high school	0.007	0.012
	(0.002)	(0.004)
Catholic school	0.019	0.016
	(0.004)	(0.004)
Private school - other religious affiliation	0.005	N/A
	(0.002)	
Private school - no religious affiliation	0.002	0.002
	(0.001)	(0.002)
School level		
Middle school/ intermediate school / junior high school	0.047	0.037
	(0.005)	(0.007)
High school	0.953	0.963
	(0.005)	(0.007)
N	1723	1011

Notes: See notes to Table 2.

Table 4: Descriptive statistics for potential outcomes for analysis samples

Variable name	STW program non-participant	STW program participant
Sample (13)		
Enrolled regular school	0.964	0.975
	(0.005)	(0.005)
N	1723	1011
Sample with valid hours data (14)		
Employed (positive hours)	0.487	0.515
	(0.013)	(0.017)
Usual hours worked per week (including zeros)	8.915	9.356
	(0.333)	(0.459)
N	1722	1008
Sample with working experience (15)		
Hourly rate of compensation	546.228	561.357
	(10.497)	(19.479)
Usual hours of work per week	18.434	18.167
	(0.469)	(0.656)
N	729	494
Sample with test-taking behavior (16)		
Take SAT or ACT examination	0.167	0.223
	(0.010)	(0.014)
Take Advanced Placement test	0.054	0.072
	(0.006)	(0.009)
N	1702	1001
Sample with complete data on expectation (17)		
Subjective probability (100% scale) of being in regular school one year later	94.029 (0.619)	93.863 (0.877)
Subjective probability of working more than 20 hours per week one year later if respondent is in school	58.658 (1.021)	63.623 (1.291)
Subjective probability of working more than 20 per week next year if respondent is not in school	83.090 (0.826)	84.445 (1.015)
Subjective probability of obtaining high school diploma by age 20	95.152	96.381
	(0.462)	(0.547)
Subjective probability of obtaining four-year college degree by age 30	74.174 (0.963)	74.894 (1.247)
Subjective probability of working more than 20 hours per week at age of 30	94.127 (0.414)	95.415 (0.411)
N	1225	761

Notes: Analysis sample numbers are given in Table 1. See notes to Table 2.

Table 5: Contemporaneous outcomes

Dependent variable	Enrollment in regular school	Employment	Usual hours worked per week	Usual hours worked per week (conditional)	Log hourly wage (cents)
Mean of dependent variable (levels)	0.968	0.459	8.525	18.613	6.188
OLS (Demographic controls)					
Participation in STW	0.011	0.067	1.176	-0.305	-0.019
	(0.007)	(0.021)	(0.506)	(0.716)	(0.034)
R^2	0.007	0.048	0.027	0.019	0.009
OLS (Full controls)					
Participation in STW	0.010	0.050	0.983	-0.085	-0.011
	(0.006)	(0.020)	(0.474)	(0.676)	(0.036)
R^2	0.062	0.146	0.137	0.129	0.071
Random effects (GLS)					
Participation in STW	0.008	0.046	0.922	-0.123	-0.011
	(0.007)	(0.020)	(0.471)	(0.676)	(0.036)
Fixed effects (OLS)					
Participation in STW	0.001	0.013	0.139	-1.883	0.067
	(0.007)	(0.029)	(0.691)	(1.255)	(0.066)
Robust Hausman tests					
Single coefficient, F-statistic	3.36	3.86	3.13	2.13	3.54
P-value for F-statistic	(0.067)	(0.050)	(0.077)	(0.145)	(0.060)
Observations	2734	2730	2730	1223	1223

Notes: For the OLS estimates, standard errors robust against heteroscedasticity and sample clustering are reported in parentheses. The demographic controls include dummy variables for male, race (white, black, and Native American/Asian/other), Hispanic ethnicity, and dummy variables for current grade (11 and 12 combined). The region dummy variables are at the level of four Census regions; an urban dummy variable is also included. Controls for student achievement includes dummy variables for grades received in eighth grade. (The available categories are: mostly below D's; mostly D's; about half C's and half D's; mostly C's; about half B's and half C's; mostly B's; about half A's and half B's; mostly A's; and A's to C's). Family background controls include dummy variables for whether the respondent lives with both biological parents at the time of interview, whether the parent who responds to the survey is born in U.S., whether a parent speaks a language other than English at home, and both father's and mother's highest grade completed (using as categories 9-11, 12, some college, college degree, and any postgraduate education), as well as variables measuring household gross income last year, household size, number of household members aged less than 18, and number of household members aged less than six. School characteristics include school size, school type, and level (middle or high school).

Table 6: Subjective expectations for following year

Dependent variable	In regular school	Work more than 20 hour per week if remains in school	Work more than 20 hours per week if leaves school
Mean of dependent variable	93.790	61.086	82.442
OLS (Demographic controls)			
Participation in STW	0.787	5.817	2.645
	(0.962)	(1.495)	(1.288)
R^2	0.009	0.022	0.013
OLS (Full controls)			
Participation in STW	1.020	5.205	2.733
	(0.962)	(1.501)	(1.272)
R^2	0.074	0.103	0.050
Random effects (GLS)			
Participation in STW	0.426	5.048	2.733
	(0.982)	(1.490)	(1.272)
Fixed effects (OLS)			
Participation in STW	-1.744	4.817	3.225
	(1.520)	(2.451)	(2.145)
Robust Hausman tests			
Single coefficient, F-statistic	8.36	0.07	0.13
P-value for F-statistic	(0.004)	(0.788)	(0.723)
Observations	2036	2036	2036

Notes: Responses are measured as the percentage chance of event. See notes to Table 5 for details regarding control variables, estimation, etc.

Table 7: College testing

Dependent variable	SAT or ACT	AP
Mean of dependent variable	0.181	0.057
OLS (Demographic controls)		
Participation in STW	0.067	0.023
	(0.016)	(0.010)
R^2	0.013	0.008
OLS (Full controls)		
Participation in STW	0.057	0.021
	(0.016)	(0.010)
R^2	0.054	0.036
Random effects (GLS)		
Participation in STW	0.053	0.020
	(0.015)	(0.010)
Fixed effects (OLS)		
Participation in STW	0.043	0.015
	(0.023)	(0.016)
Robust Hausman tests		
Single coefficient, F-statistic	0.36	0.17
P-value for F-statistic	(0.549)	(0.676)
Observations	2703	2703

Notes: See notes to Table 5 for details regarding control variables, estimation, etc.

Table 8: College testing and subjective probability of obtaining a four-year college degree by age 30

Dependent variable	Four-year college degree by age 30	Four-year college degree by age 30
Mean of dependent variable	74.043	
OLS (Demographic controls)		
SAT or ACT	4.580	
	(1.552)	
AP		7.275
		(2.460)
R^2	0.043	0.042
OLS (Full controls)		
SAT or ACT	2.838	
	(1.465)	
AP		2.516
		(2.475)
R^2	0.250	0.249
Random effects (GLS)		
SAT or ACT	2.838	
	(1.465)	
AP		2.516
		(2.475)
Fixed effects (OLS)		
SAT or ACT	2.562	
	(2.746)	
AP		0.902
		(4.069)
Robust Hausman tests		
Single coefficient, F-statistic	0.05	0.22
P-value for F-statistic	(0.819)	(0.640)
Observations	2012	2012

Notes: See notes to Table 5 for details regarding control variables, estimation, etc.

Table 9: Subjective probabilities regarding schooling and work at ages 20 and 30

Dependent variable	High school diploma by age 20	Four-year college degree by age 30	Work more than 20 hours per week at age 30
Mean of dependent variable	94.919	73.831	93.716
OLS (Demographic controls)			
Participation in STW	1.917	1.732	1.753
	(0.722)	(1.348)	(0.639)
R^2	0.027	0.040	0.027
OLS (Full controls)			
Participation in STW	1.545	1.234	1.897
	(0.701)	(1.214)	(0.644)
R^2	0.127	0.248	0.071
Random effects (GLS)			
Participation in STW	1.440	1.234	1.944
	(0.722)	(1.214)	(0.647)
Fixed effects (OLS)			
Participation in STW	0.634	0.030	2.149
	(1.197)	(2.229)	(1.080)
Robust Hausman tests			
Single coefficients, F-statistic	1.81	1.91	0.14
P-value for F-statistic	(0.179)	(0.167)	(0.710)
Observations	2036	2036	2036

Notes: See notes to Table 5 for details regarding control variables, estimation, etc.

Table 10: Hours spent at work site for each category of STW, based on most recent STW participation

Type of STW	Proportion with hours spent at work site = 0	Mean hours spent at work site	N	Mean hours spent at work site (conditional on hours > 0)	N
Job shadowing	0.081	16.495	260	17.940	236
	(0.017)	(2.996)		(3.245)	
Mentoring	0.413	20.657	83	35.216	46
	(0.062)	(5.067)		(7.673)	
Cooperative education	0.342	47.518	104	72.225	4
	(0.052)	(16.771)		(24.771)	
School-based enterprise	0.388	22.339	172	36.503	104
	(0.041)	(6.557)		(10.439)	
Tech prep	0.625	61.741	159	164.517	58
	(0.043)	(31.674)		(81.970)	
Apprenticeship or internship	0.408	111.447	66	188.230	36
	(0.069)	(33.681)		(51.929)	
Other	0.145	18.987	167	22.198	141
	(0.029)	(3.226)		(3.710)	

Notes: See notes to Table 2.

Table 11: Effect of STW participation on outcome by hours intensity of STW type

	Enrollment in regular school (1)	Employment (2)	Usual hours worked per week (3)	Usual hours worked per week (conditional) (4)	Log hourly wage (5)	In regular school (6)	Work more than 20 hour per week if remains in school (7)
Low intensity	-0.007	0.011	0.775	-0.923	0.062	-2.272	7.120
	(0.014)	(0.039)	(0.665)	(0.987)	(0.089)	(2.072)	(2.041)
Moderate intensity	0.007	0.002	1.310	0.639	0.025	-0.515	4.551
	(0.012)	(0.042)	(0.712)	(1.014)	(0.080)	(2.076)	(2.162)
High intensity	0.006	0.038	0.570	0.295	0.133	-2.950	1.985
	(0.021)	(0.054)	(0.850)	(1.160)	(0.117)	(3.167)	(2.427)
Estimator	FE	FE	RE	RE	FE	FE	RE
Hausman	2.83	2.76	1.82	1.26	2.74	3.32	0.86
(p-value)	(0.037)	(0.041)	(0.142)	(0.288)	(0.004)	(0.019)	(0.463)
N	2734	2730	2736	1223	1223	2036	2036

	Work more than 20 hours per week if leaves school (8)	SAT or ACT (9)	AP (10)	High school diploma by age 20 (11)	Four-year college degree by age 30 (12)	Work more than 20 hours per week at age 30 (13)
Low intensity	3.265	0.043	0.008	2.278	4.011	2.682
	(1.673)	(0.021)	(0.013)	(0.777)	(3.114)	(0.778)
Moderate intensity	1.416	0.050	0.028	1.396	-0.451	1.315
	(1.880)	(0.023)	(0.016)	(1.052)	(3.117)	(0.935)
High intensity	3.841	0.076	0.026	-0.092	-8.547	1.578
	(2.191)	(0.031)	(0.018)	(1.515)	(4.542)	(1.215)
Estimator	RE	RE	RE	RE	FE	RE
Hausman	1.47	1.08	0.62	1.42	4.29	1.46
(p-value)	(0.222)	(0.358)	(0.600)	(0.234)	(0.005)	(0.225)
N	2036	2703	2703	2036	2036	2036

Notes: See notes to Table 5 regarding estimation, control variables, sample, etc. Only the specifications with the full set of controls are reported. The row labeled "estimator" indicates whether fixed effects (FE) or random effects (RE) estimates are reported. The Hausman test statistics reported are for the null hypothesis that random effects is the correct specification. Fixed effect estimates are reported instead of random effects estimates when the p-value from the Hausman test was less than 0.1. The reported Hausman statistics are robust against heteroscedasticity. The hours categories are as follows (see Table 10): low intensity (job shadowing and other); moderate intensity (mentoring, cooperative education, and school-based enterprise); high intensity (tech prep and apprenticeship or internship). The estimates in columns (1)-(5) correspond to Table 5, those in columns (6)-(8) correspond to Table 6, those in columns (9)-(10) correspond to Table 7, and those in columns (11)-(13) correspond to Table 9.

Table 12: Relationship between respondent reports of participation in school-to-work programs and school reports of offering school-to-work programs (missing value in school survey treated as not offered)

		School offers STW		Pearson chi-square, p-value
		No (column %)	Yes (column %)	
Respondent participation in STW	No	64.29	62.10	
	Yes	35.71	37.90	0.241
	N	1136	1598	
Data based on "ever participated"				
Excluding "other" category	No	73.40	71.90	
	Yes	26.60	28.10	0.386
	N	1154	1580	
Job shadowing	No	88.44	84.85	
	Yes	11.56	15.15	0.013
	N	2041	693	
Mentoring	No	94.93	93.93	
	Yes	5.07	6.07	0.393
	N	2306	428	
Cooperative education	No	92.58	93.72	
	Yes	7.42	6.28	0.263
	N	1778	956	
School-based enterprise	No	91.31	87.88	
	Yes	8.69	12.12	0.042
	N	2404	330	
Tech prep	No	91.69	92.13	
	Yes	8.31	7.87	0.691
	N	1806	928	
Apprenticeship or internship	No	95.78	96.03	
	Yes	4.22	3.97	0.793
	N	2180	554	
Data based on "most recent" participation				
Job shadowing	No	91.23	88.31	
	Yes	8.77	11.69	0.024
	N	2041	693	
Mentoring	No	97.09	96.26	
	Yes	2.91	3.74	0.356
	N	2306	428	
Cooperative education	No	95.73	97.07	
	Yes	4.27	2.93	0.079
	N	1778	956	
School-based enterprise	No	94.01	91.52	
	Yes	5.99	8.48	0.080
	N	2404	330	
Tech prep	No	94.35	93.86	
	Yes	5.65	6.14	0.601
	N	1806	928	
Apprenticeship or internship	No	97.61	97.47	
	Yes	2.39	2.53	0.846
	N	2180	554	

Table 13: Descriptive statistics for school information for full analysis sample

Variable name	STW participation rate (youth response)	N
School program		
General	0.332 (0.013)	1581
College preparatory, academic, or specialized academic	0.373 (0.018)	859
Vocational technical or business and career	0.628 (0.050)	126
Combination academic and vocational program	0.534 (0.046)	145
Special education	0.250 (0.153)	9
Other	0.532 (0.157)	14
School type		
Public	0.371 (0.010)	2651
Technical or vocational high school	0.490 (0.104)	29
Catholic school	0.331 (0.076)	41
Private school - other religious affiliation	0	8
Private school - no religious affiliation	0.383 (0.216)	5

Notes: See notes to Table 2.

Table 14: Effect of STW participation on outcome using "Jackknife" IV estimation

	Enrollment in regular school	Employment	Usual hours worked per week	Usual hours worked per week (conditional)	Log hourly wage	In regular school	Work more than 20 hour per week if remains in school
OLS (Full controls)							
Participation in STW	0.002	0.042	0.827	-0.282	-0.019	-0.100	6.374
	(0.009)	(0.022)	(0.526)	(0.741)	(0.040)	(1.105)	(1.687)
R^2	0.052	0.161	0.146	0.128	0.081	0.074	0.113
IV 1							
Participation in STW	0.024	0.232	5.449	4.235	-0.494	9.294	24.082
	(0.052)	(0.163)	(4.330)	(6.188)	(0.256)	(7.593)	(12.915)
First-stage F-statistic	19.35	19.47	19.47	11.87	11.87	18.44	18.44
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
T-statistic for exogeneity test	-0.422	-1.232	-1.138	-0.785	2.182	-1.299	-1.523
	(0.673)	(0.218)	(0.256)	(0.433)	(0.030)	(0.194)	(0.128)
IV 2							
Participation in STW	0.022	0.196	4.114	2.532	-0.472	10.719	24.527
	(0.050)	(0.164)	(4.102)	(5.537)	(0.249)	(7.776)	(13.357)
First-stage F-statistic	18.45	18.68	18.68	11.78	11.78	17.00	17.00
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)
T-statistic for exogeneity test	-0.399	-0.984	-0.844	-0.539	2.106	-1.474	-1.514
	(0.690)	(0.326)	(0.399)	(0.590)	(0.036)	(0.151)	(0.131)
N	2176	2174	2174	988	988	1625	1625

	Work more than 20 hours per week if leaves school	SAT or ACT	AP	High school diploma by age 20	Four-year college degree by age 30	Work more than 20 hours per week at age 30
OLS (Full controls)						
Participation in STW	1.980	0.058	0.024	0.465	0.676	1.292
	(1.442)	(0.017)	(0.011)	(0.808)	(1.370)	(0.720)
R^2	0.061	0.061	0.039	0.120	0.258	0.082
IV 1						
Participation in STW	-14.145	0.129	0.065	-3.604	3.627	-4.307
	(10.827)	(0.133)	(0.080)	(5.940)	(9.983)	(5.234)
First-stage F-statistic	18.44	18.75	18.75	18.44	18.44	18.44
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
T-statistic for exogeneity test	1.622	-0.544	-0.517	0.707	-0.301	1.118
	(0.105)	(0.587)	(0.605)	(0.480)	(0.764)	(0.264)
IV 2						
Participation in STW	-13.711	0.121	0.100	-3.669	0.031	-4.539
	(11.057)	(0.135)	(0.081)	(6.279)	(10.128)	(5.336)
First-stage F-statistic	17.00	17.80	17.80	17.00	17.00	17.00
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
T-statistic for exogeneity test	1.543	-0.472	-0.961	0.682	0.064	1.149
	(0.123)	(0.637)	(0.337)	(0.496)	(0.949)	(0.251)
N	1625	2149	2149	1625	1625	1625

Notes: See notes to Table 5 regarding estimation, control variables, sample, etc. The t-statistics for testing exogeneity are from the Hausman test of the null hypothesis that school-to-work participation is exogenous. All standard errors (and therefore also the Hausman test) are robust against heteroscedasticity and clustering. "IV 1" is the "jackknife" mean of school-to-work participation within school, defined as

$$IV1_{i,j} = 1/(N_j - 1)\sum_{k \neq i} STW_{k,j},$$

where i indexes individuals and j indexes schools. "IV 2" is calculated similarly, but for the residual of the first-stage regression, which removes the influence of other characteristics on school-to-work participation, or

$$IV2_{i,j} = 1/(N_j - 1)\sum_{k \neq i} \hat{v}_{k,j},$$

where $\hat{v}$ is residual of the first-stage regression.