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Producing Adulthood:
Adolescent Employment, Fertility, and the Life Course

Emily Rauscher
emily.rauscher@nyu.edu
PhD Student

New York University
Sociology Department
295 Lafayette St. 4th Floor
New York, NY 10012
646 706-1804

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Abstract

Adolescent employment is typically framed as having either positive or negative effects. Yet cutting edge research yields apparently contradictory results; work lowers delinquency but also increases high school dropout. Both opportunity cost and life course development theories could explain these results. This study investigates effects of employment on fertility among adolescent women, which pits life course development against opportunity cost theory. Using 2006 and 2007 American Community Survey data, individual instrumental variable and state level difference-in-difference models (following the same cohort over time) control for self-selection into early employment. Both methods find a positive effect of employment on adolescent fertility. National Vital Statistics birth data confirm state-level results. Results for fertility (and some evidence for other early transitions) suggest early employment speeds the transition to adulthood, supporting life course theory. Findings suggest adolescent employment should be reconceived as promoting adult rather than positive or negative behavior.

Social scientists and politicians typically associate employment with positive attributes and outcomes – responsibility, self-reliance, contribution to society, and reduced crime are just a few. The 1996 welfare reform emerged from similar pro-work beliefs; encouraging employment through "welfare-to-work" policies was supposed to reduce adolescent fertility and dependence on the government (e.g., Murray 1984). Desistance research associates employment with reduced criminal behavior (Laub and Sampson 2003). Work is thought to generate opportunity costs that discourage antisocial behavior and teen fertility.

But employment is an adult role; how does it affect adolescents? Although many contemporary adolescents work (D'Amico and Baker 1984; Entwisle et al. 2005) and sociological concern about youth employment dates to at least the 1970s (Elder 1974), existing research provides an incomplete understanding of the effects of youth employment for both methodological and theoretical reasons.

Theoretically, research cannot distinguish between life course and opportunity cost explanations for the effects of youth employment. For example, cutting edge research finds that employment lowers test scores (Tyler 2003) and increases high school dropout among youth (Apel et al. 2008), but at the same time reduces delinquency (Apel et al. 2008, 2007). These outcomes support both opportunity cost and life course explanations. Early employment could increase the opportunity costs associated with school effort and delinquent behavior through foregone earnings or work experience. However, another potential explanation is that early employment speeds the transition to adulthood, encouraging adult behavior such as disengagement from school and reduced delinquency.

Methodologically, establishing a causal relationship between employment and other teenage behavior is a challenge and flaws haunt the field. Early work fails to control for self-selection and relies on local, non-representative samples. Recent contributions address self-

selection, but some use questionable instrumental variables and rigorous studies continue the narrow focus on education and delinquency, which fail to establish theoretical causality.

Furthermore, gender-biased measurements or heterogeneous employment effects by gender could drive some of the results of existing research. For example, commonly used outcome measures (e.g., delinquency) can miss female-specific behaviors, such as fertility, obscuring effects among young women.

In an effort to make sense of the apparently contradictory effects of employment (Apel et al. 2008, 2007) and address the neglect of female-specific outcomes, this paper explores the relationship between adolescent employment and fertility among young women. In doing so, it addresses both the methodological and theoretical challenges of causality. Methodologically, it improves on early research by using more recent data (2006 and 2007 American Community Surveys and 1990-2006 National Vital Statistics birth data), an instrumental variable less likely to violate the exclusion restriction (state youth employment certification laws), state-level data to confirm individual results, and a female-specific outcome overlooked in previous research. Theoretically, this paper advances understanding by investigating employment effects on a new outcome – fertility – which pits opportunity cost and life course development theories against each other.

The US adolescent fertility rate is more than twice the average rate among European Union and other developed countries (UN 2008). Youth employment is also 30% higher (ILO 2006: 15; BLS 2008: 6). Adolescent fertility has negative economic consequences for both society and individuals, even net of self-selection (Lee 2007; Fletcher and Wolfe 2009). Is there a causal relationship between adolescent employment and fertility?

Issues relating to the transition to adulthood are a matter of growing policy concern. The transition to adulthood is taking longer in the US and other developed countries (Furstenberg,

2008). Arnett (2004) and Kimmel (2008) even suggest this extension of adolescence represents a separate life stage. Adolescence is associated with crime, suicide, and accidents so extending it could have negative consequences. Does working as a teenager speed adulthood? Results suggest adolescent employment speeds the transition to adulthood, producing adulthood and explaining apparently contradictory effects found in recent literature.

Theorizing Effects of Adolescent Employment

The following section outlines three theories, which could explain results of this and other research. Figure 1 illustrates the conceptual framework and how the predicted effect of work differs depending on job type, income level, and family background.

(Figure 1 here)

Employment and the Life Course: Speeding Adulthood

Broadly, life course theory links individual biography to historical, geographical, and structural context in an effort to understand social change and individual development. A central tenet of life course theory, particularly relevant here, is that the timing of a transition conditions its developmental impact (Elder 1998).

In his historical account of American adolescence, Thomas Hine (1999) examines how adolescence was created and problematized as a separate stage of life. In the early 1900s, technological developments mechanized production, reduced demand for child labor, and increased labor market competition, surplus, and real income (Zelizer 1994; Hine 1999). Meanwhile, large-scale immigration increased competition for already decreasing low-skilled jobs (Osterman 1979). Compulsory education increased, further pulling youth out of the work force. In short, fundamental economic and political changes in the early 1900s made youth an unemployed, idle group, segregated from adults in schools; adolescence and youth culture were born. Employment became a (if not *the*) fundamental barrier between youth and adulthood.

Given its role in creating adolescence, youth employment should reduce differences between youth and adults (structural, social, psychological) and speed the transition to adulthood.

The life course lens sees a series of age-graded or age-normative transitions (e.g., employment, fertility, or marriage) coupled with patterned trajectories through those transitions (Sampson and Laub 1992). Age differences in transitions – being ahead or behind the normative transition age – are conceived as influencing life trajectories (e.g., Caspi et al. 1990). The timing of employment, for example, may change one's trajectory. Work could steer youth away from delinquency or increase the chances of other early transitions such as fertility.

What are the mechanisms involved? According to the developmental model, work develops skills and promotes adult traits (responsibility, maturity) that could both reduce crime and improve academic work (particularly good jobs, Holland and Andre 1987). However, partly due to the unskilled jobs available, work exposes adolescents to attitudes and values not considered age-normative. This adult exposure increases risk taking and precocious behaviors deemed problematic during adolescence (Bozick 2006; Bachman and Schulenberg 1993). In other words, exposure to adult attitudes, experiences, networks, and even responsibilities is the mechanism through which early life course transitions are expected to speed later transitions.

To summarize, the life course model hypothesizes that early employment will affect life trajectories by speeding other transitions to adulthood, such as fertility.[1] Through its influence on social context, adolescent employment should increase adult behaviors, including positive behavior such as reduced delinquency, but also negative behavior such as substance use and school disengagement. Recent findings are consistent with this explanation (Apel et al. 2008,

[1] Procyclical fertility theory also predicts employment will increase fertility by providing resources to support a child (Galbraith and Thomas 1941; Silver 1965; Thomas 1925). However, high paying, skilled jobs provide more resources and should increase fertility most according to procyclical fertility; in contrast, life course theory predicts they expose youth to more age-normative contexts and have the least effect. Interactions by job type can distinguish between these two explanations.

2007), but could also be explained by opportunity cost theory.

Employment and Opportunity Costs: Reducing Fertility

Work provides a disincentive to engage in school and crime, which interfere with job rewards (both monetary and social). William Julius Wilson (1987) suggests unemployment among low-income black men generates crime, welfare dependence, and nonmarital fertility. Similarly for women, opportunity cost (or countercyclical fertility) theory suggests employment should reduce fertility by increasing its costs through foregone earnings and human capital development (Butz and Ward 1979). Having a child could jeopardize the employment, income, and social status of working youth (Kraft and Coverdill 1994; Rich and Kim 2002). Opportunity costs should be strongest in skilled and high paying jobs and among low income youth.

Effects of employment may depend on race and ethnicity. Lack of job availability and discrimination by employers (Pager 2003) could limit work opportunities for nonwhite and Latino youth to unskilled and low paid jobs – where social influence should be the most precocious. On the other hand, employment may generate higher opportunity costs for non-white compared to white youth, given low employment opportunity for minorities. After controlling for job type, opportunity costs of employment should be greater for minority youth.

Both the life course model and opportunity costs could explain previous findings – reduced academic engagement and delinquency. This paper advances research on adolescent employment by studying its effect on youth fertility, a fundamental adult behavior that distinguishes between opportunity cost and life course theories.

Self-Selection into Employment: No Effect of Work on Fertility

Both of the theories outlined above suggest that adolescent employment affects fertility. Yet several recent studies suggest that any apparent effects of youth employment are due to self-selection, reflecting a spurious rather than a causal relationship (e.g., Paternoster et al. 2003;

Rothstein et al. 2007; Buscha et al. 2008). For example, some argue that a lack of self-control causes both early employment and behavior problems (Gottfredson and Hirschi 1990). From this perspective, youth who are more likely to have a child may also be more likely to work because they lack self-control or desire autonomy. Similarly, early adult role transitions may reflect an underlying precocity. Precocious development hypothesis (Newcomb and Bentler 1988) suggests that precocious youth are more likely to both work and have a child as a teenager. Thus, regardless of findings, critics could argue that employment is endogenous and any apparent effect simply reflects an underlying unobserved factor. To address these criticisms, this investigation controls for unobserved differences using state differences in work permit laws as an instrumental variable.

This study tests the following hypotheses (see Figure 1). Life course theory suggests adolescent employment (particularly low-skilled, adult, low-paying work) increases the chance of fertility. Opportunity cost arguments expect it (especially skilled, high-paying employment) to decrease the likelihood of fertility. The self-selection or precocious development hypothesis predicts no effect.

Previous Research on Adolescent Employment

This review addresses some prevalent methodological concerns. It then turns to the central concerns of this paper: 1) the limited connection between adolescent employment and fertility; and 2) studying outcomes which do not distinguish opportunity cost and life course development theories, even if endogeneity is adequately addressed.

Several studies rely on local samples, which do not allow generalization at the national level (Mortimer et al. 1996; Mortimer et al. 2002). Several relevant panel data sets are outdated. For example, the National Youth Survey includes youth who were teens 30 years ago (Ploeger 1997). Even several recent studies using an IV approach rely on older data (Sabia 2009; Tyler

2003; Buscha et al. 2008; Lee and Staff 2007).

Early research failed to rule out the self-selection argument (Elder 1974; D' Amico and Baker 1984; Greenberger and Steinberg 1986; Marsh 1991). Research controlling for unobserved heterogeneity finds that most (Ploeger 1997; Bachman and Schulenberg 1993; Steinberg et al. 1993; Mortimer et al. 1996) if not all (Sabia 2009; Paternoster et al. 2003) of the purported relationship between adolescent employment and various outcomes is due to self-selection. But even these studies have limitations. For example, a central assumption of the IV approach is that the IV has no direct effect on the outcome. Several studies use local labor market measures as an IV (Neumark 2002; Rothstein 2007; Lee and Orazem 2008), which may influence outcomes directly through parental stress, neighborhood characteristics, or educational opportunities and bias results.

Much of the research on adolescent employment does not investigate heterogeneous effects of work by background factors such as race, class, or gender. Even some research controlling for self-selection (Paternoster et al. 2003; Apel et al. 2008) fails to investigate potentially heterogeneous effects by gender and could misrepresent effects of work.

Research tends to focus on work intensity (hours per week) (Greenberger and Steinberg 1986; Marsh 1991; Bachman and Schulenberg 1993; Ruhm 1995), neglecting context or skill level. Staff and Mortimer (2008) stress the importance of studying effects of job quality, but existing studies are limited by small, non-representative, or older samples (Hansen and Jarvis 2000; Mortimer et al. 2002; Entwisle et al. 2005, 2000; Steinberg et al. 1993).

A fundamental problem with adolescent employment literature is the outcomes investigated. This has several related problems. First, the outcomes can often be explained by at least two theories. For example, employment effects on later wages and employment (Neumark 2002; Neumark and Rothstein 2003) could reflect increased maturity or responses to opportunity

costs. McNeal (1997), Lee and Staff (2007), Apel et al. (2008), and Yeung and Rauscher (2009) collectively find evidence that work increases school dropout or disengagement, but reduces delinquency. This could support opportunity cost and life course explanations.

Second, common outcomes, such as education (Tyler 2003; Warren and Lee 2003) or delinquency (Paternoster et al. 2003; Brame et al. 2004), encourage a subjective view. Literature tends to frame youth work as either positive – promoting skills and development – or negative – detracting from more positive pursuits and exposing youth to deviant influences. An exception is work by Apel et al. (2008:357), who attribute apparently contradictory effects (increased school dropout, but lower delinquent behavior) to identity theory; "work provides a positive identity for youth who are already detached from school or find little positive identity in school." Critically, however, their methodological correction for self-selection should control for attachment to school and other theories could explain these effects.

Finally, while adult fertility is frequently related to employment (Galbraith and Thomas 1941; Silver 1965; Becker 1960; Butz and Ward 1979; Sacerdote and Feyrer 2008), the relationship between adolescent employment and fertility has rarely been examined. Adolescent fertility is frequently explained by individual personality or characteristics (Schneider 1982; Schinke et al. 1979) or family planning access and use (Boonstra 2002). Psychologists, for example, portray adolescent fertility as due to individual mental deficits: lack of self-esteem, desire for adult status, or lack of self-control (all factors that may be related to the decision to work). These explanations typically assume adolescent irrationality or powerlessness. In short, adolescents are treated as "a tribe apart" (Hersch 1999). Yet adolescent employment may be related to fertility for a variety of reasons.

Exceptions that examine the relationship between adolescent work and fertility (Colen et al. 2006; Kraft and Coverdill 1994; Rich and Kim 2002) generally focus on young adults rather

than teens and often fail to address self-selection. Colen et al. (2006) and Olsen and Farkas (1990) are important exceptions, but study employment at the aggregate rather than the individual level so any relationship may reflect reactions to significant others' employment.

In short, research has tended to focus on outcomes which encourage interpretation of work as positive or negative rather than "adult" and do not rule out other explanations. Taking a broader perspective, this study addresses the above concerns by controlling for self-selection and assessing effects of work on fertility – an outcome which pits life course development against opportunity cost theory.

Data and Methods

The 2006 American Community Survey (ACS), conducted by the US Census Bureau, provides nationally representative, household level data with a rolling reference point to allow a more sensitive measure of employment status (particularly important for youth). The overall ACS 2006 sample size is 2,969,741, of which 20,740 are females age 17. (State level aggregate ACS 2007 data is used for difference-in-difference estimates but not individual analyses; 2007 details are similar.) Analysis is limited to young women because they ultimately determine and experience greater consequences of youth fertility. Fertility data on young men suffers from measurement error and is generally unavailable in large scale surveys. The ACS is particularly attractive for this study because it includes a large group of adolescent women who give birth. Results are supported with National Vital Statistics data, which provides highly reliable records of annual state fertility by mother's age from 1990 to 2006. This is population, not sample data, and makes results more convincing.

The instrumental variable is state requirement of a work permit until age 18 instead of 16, which affects the employment likelihood of 17 year olds. The sample is therefore limited to 17 year olds and excludes those in Alaska (the only state requiring a work permit until age 17).

Models predicting fertility exclude youth in precocious or distinct contexts – ever married, not in school, living on their own, noncitizens (and those with allocated values for key measures). This yields a total sample of 16,306 17 year old women. These individuals are included in models predicting other precocious outcomes. However, in models predicting fertility these exclusions reduce measurement error, prevent the need to control for endogenous factors (such as school dropout or marriage) and allow analysis of the "typical" non-precocious US teenager, which makes a significant effect of employment on fertility less likely. Note that these exclusions do not bias estimates, they only reduce generalizability to the typical teenager, often assumed in policy discussions. Furthermore, including all these groups does not change results (with very slight exceptions in two models – additional information about exclusions is available in Appendix A). (See www.census.gov/acs/www/ SBasics/ for further information about the ACS design and sampling.)

Dependent Variable

The following question measures fertility: "Has this person given birth to any children in the past 12 months?" Comparison to NVSS birth data indicates validity (Appendix A). Including all 17 year old women, an index of early transitions includes whether an adolescent: had a child in the last 12 months; has ever been married; or is living independently (as head of household, spouse, partner, boarder, or housemate).

Key Independent Variable

Youth employment includes those who worked within the past 12 months, even for a few days. This is a broad definition of work, but captures youth employment that would be excluded otherwise. Summer and informal employment, for example, are often neglected. To address concern about this broad definition, a narrower measure is constructed, including youth who worked at least 40 weeks in the past 12 months, which limits work to fairly consistent, long-term

employment. Hours individuals worked last week provides a measure of work intensity. Of those who worked the week before the survey, the average is 18 hours per week (std. dev. of 10) and 73% work 20 or fewer hours per week, generally considered non-intense work. Thus, most work is part-time.[2]

Jobs are categorized as skilled, service, and labor according to the standard occupational classification codes. Skilled jobs are defined broadly to help differentiate youth workers, who are overwhelmingly concentrated in service work; they include managers as well as occupations associated with high status or skill requirements. In terms of context, jobs with the highest concentrations of youth according to the 2006 ACS should expose adolescents to a more youthful and less adult environment than others. High youth jobs (occupation codes holding 4% or more of 17 year olds working in the last year) include: waitress; cashier; retail sales; food service or sales; food preparation; restaurant hostess; and childcare. Finally, work with children should expose youth to the least adult environments. An indicator of childcare includes childcare and education occupations. (See Appendix A for measure details. Relationships between job type and adolescent fertility should not be interpreted causally, because the IV only controls for self-selection into employment, not type of work.)

Instrumental Variable

Most states either do not require employment certification or only require it until age 16. Living in a state requiring employment certification ("work permit") until age 18 creates a small but additional barrier to employment for 17 year olds. It is an exogenous shock on adolescent

[2] The ACS does not include a good measure of work intensity (hours per week), which Greenberger and Steinberg (1986) have identified as an important factor. This intensity measure will have substantial measurement error for youth, who tend to have less stable employment. However, work hours are only used to investigate time at work as a potential mechanism and results are interpreted cautiously. In addition, explanations for a relationship between adolescent employment and fertility focus more on job type rather than time. While time at work may be important, this study does not fully address it.

employment, exploited here to control for self-selection. Work permits require extra steps in the employment process for both adolescents and potential employers. They involve an application process for potential employees and potential employers generally must sign and keep work permits on file, verifying that youth will not be exposed to inappropriate work duties, environments, or hours. (Appendix B provides an example of additional obstacles faced by 17 year olds in these states.) Statistical tests confirm instrument strength.

A review of state youth labor laws suggests they are not systematically related to region, industry, or urbanization. To check for potential correlation, Table 1 compares state-level sample averages by work permit requirement, weighted by size of the state 17 year old population. Most of the work measures are significantly different, providing evidence of the IV's strength. Other significant differences include: race; head of household status; and number of child labor officers. To address these differences, they are controlled in ACS analyses (youth who are head of household are excluded in individual models predicting fertility). Unweighted comparisons show significant differences only for work and race measures.

(Table 1 and Figure 2 about here)

Figure 2 depicts the IV visually. States requiring work permits until age 18 are spread throughout the US, in every region. Not all of the restrictive states are agricultural, rust belt, southern, urban, or coastal. While requiring a permit until 18 looks more common in urbanized states, it is not a perfect correlation; Florida, Illinois, and Massachusetts do not require it.

Figure 3 shows state fertility rates for 15-17 year olds, for comparison with Figure 2.

(Figure 3 about here)

Comparing Figures 2 and 3 suggests that adolescent fertility is not related to the IV except through employment. In short, employment certification laws appear to be an exogenous

influence on adolescent employment. If they are related, the results would mis-estimate the effect of employment on fertility, but models control for state level measures to address this.

Potential Confounders and Additional Control Variables

Adolescents have minimal control over their place of residence. The following controls are therefore not endogenous: living in a state with a high proportion of Catholics (31% or more in 2000, above the 75th percentile) or Protestants (13% or more in 2000, above the 75th percentile); living in a state with abortion restrictions (an index of indicators for whether a state requires parental consent or notification, has a mandatory waiting period, and limits abortion funding – Cronbach's alpha is .86); state male incarceration rate; female headed household; number of people in the household; and head of household education. (A measure of generation age gap is constructed but not included in models shown because of missing values. Including it does not change results). Additional controls include race, ethnicity, household size-adjusted income, number of state child labor enforcement officers (potentially related to state differences in enforcement), region, and proportion of the state below poverty. (See Appendix A for details.)

Although potentially endogenous variables are excluded from early models, individual educational attainment (measured as highest grade level completed) and income (measured in $10,000) are added in later regressions that include other potentially endogenous variables, such as job type. Those in the top 40% of individual incomes ($2,500 for those who worked in the last year; $5,000 for those who worked more than 39 weeks) are considered to have a high paying job.

Modeling Strategy

IV models estimate the local average treatment effect – the effect of work on youth who are borderline in their decision to work and are influenced by the state work permit policy. This

estimate is particularly useful from a life course perspective, which seeks to understand transition effects on later trajectories. Early employment is a true turning point among borderline workers.

Two-stage linear probability models are used to estimate the effect of employment on adolescent fertility, corrected for endogeneity. OLS and logit models are provided for comparison. Hellevik (2007) finds evidence supporting the use of linear models for a binary dependent variable and results are easier to interpret. In all models, the endogeneity test (similar to a Hausman test for clustered data) of adolescent employment suggests the IV approach is an improvement over OLS.

1st stage: $E[P\{Work_i^* = 1 \mid WorkPermit18_i, Controls_i, \varepsilon_i\}] = aWorkPermit18_i + bControls_i + \varepsilon_i$

2nd stage: $E[P\{Fertility_i = 1 \mid Work_i^*, WorkPermit18_i, Controls_i, \gamma_i\}] = cWork_i^* + dControls_i + \gamma_i$

The second stage regression uses the predicted employment probability from the first stage, where *WorkPermit* has controlled for some exogenous variance. Coefficients are expressed in probabilities and the key parameter is *c*. A significant effect of work on fertility using the IV approach would suggest the relationship is not due to self-selection. Due to correlation of observations at the state level, all regressions use Huber-White standard errors adjusted for state-level clustering. Standard errors are corrected for the two-stage approach using Stata's ivreg2. All models presented include sampling weights; omitting weights slightly reduces the estimated magnitude of the effect of work, but does not change the significance. Variance inflation factor tests yield averages less than 2.3, suggesting multicollinearity is not a concern. Sensitivity checks include regressing fertility on working at least 40 weeks in the last year and give similar results.

Concerns about any instrument include exogeneity (which cannot be tested directly), strength (the IV must substantially affect the endogenous variable), and monotonicity (it pushes individuals in only one direction). Applied to this study, the concerns are that work permit laws

are not directly related to adolescent fertility (addressed above), change the work decisions of many youth, and the additional requirements do not perversely encourage some to work. In this analysis, Wald F statistic tests of IV strength are nearly all above 15% of Stock and Yogo (2005) critical values. The model with a weak IV test investigates interaction effects and should not be interpreted causally anyway because job type and income are endogenous.

To check for direct effects of the IV, regressions of fertility on the IV for groups whose outcome should not be affected (e.g., 16 and 19 year old youth in this case) show no significant relationship, either in a bivariate or the full multivariate regression. Bivariate regressions of the IV on fertility and on potential intermediate effects (e.g., ever having married, living on one's own) for ages 16 to 19 also show no significant relationship. Finally, youth employment rates are lower, on average, in states requiring work permits until age 18. This supports the monotonicity assumption. These steps cannot rule out lack of monotonicity or direct effects, but they offer convincing evidence.

A problem with cross-sectional data such as the ACS is establishing time order – e.g., youth may become pregnant and then decide to work. The IV approach should address this by controlling for selection into employment based on pregnancy or fertility, but state-level models more convincingly address time order. State-level difference-in-difference models estimate the effect of requiring work permits until age 18 (*b*) on the change in fertility among the same cohort as they age from 17 to 18 (year j to j+1 in state i). Time-constant state differences drop out.

$$Age18FertilityRate_{ij+1} - Age17FertilityRate_{ij} = a + bWorkPermit18_i + cControls_i + \varepsilon_i$$

Combining state level aggregate ACS 2006 and 2007 data – following the same cohort of young women from age 17 in 2006 to 18 in 2007 – allows estimating the exogenous effect of a work permit requirement on fertility rate changes. The same approach is used in National Vital Statistics (NVSS) fertility data from 1990 to 2006.

This approach assumes: 1) the change in state fertility rates from age 17 to 18 would be similar except for the different work permit requirement; and 2) the work permit law is unrelated to any changes in the cohort population from 2006 to 2007. To address these assumptions, the full difference-in-difference model for ACS data includes state level 2006 measures of all the factors included in the full individual level IV model, plus measures of other early transitions (marriage, living independently, and not being in school) which are not endogenous at the state level. If employment increases the likelihood of fertility, the increase in fertility between ages 17 and 18 should be smaller in states that restrict employment among 17 year olds.

Results

Descriptive Statistics

Table 2 shows significant racial and ethnic differences, with black and Latino youth more likely to: experience fertility, have early transitions, and live in a lower income, less-educated, female headed household. Many of these descriptives echo previous racial inequality findings. Out of concern for the exogeneity of the IV, it is notable that black, Asian, and Latino youth are more likely to live in a state requiring a work permit until age 18. Employment, including working more than 39 weeks, is more common among white than black, Asian, or Latino young women and white youth have higher incomes.

(Table 2 about here)

There are also significant differences between workers and nonworkers. Young women who worked in the last year are more often white, non-Latina, and from higher income households with fewer people and two parents. Contrary to assumptions and findings in the literature that youth employment competes with educational attainment (e.g., Mihalic and Elliot 1997), 17 year old women who work have completed slightly more years of school than

nonworkers. These observable differences suggest there may be unobservable differences and confirm the need to address self-selection into employment.

Regression Analysis

Table 3 compares IV linear probability models with OLS and logit models. OLS and logit models show a negative relationship between work and fertility. In contrast, the IV models (which endogeneity tests indicate are an improvement) reveal that working slightly (but significantly) increases the likelihood of fertility. The change of sign of the IV compared to the OLS coefficient suggests many adolescent women who are unlikely to have a child self-select into employment, giving a highly biased OLS estimate. After correcting for the joint determination of early work and fertility, work increases teen fertility. This contradicts opportunity cost and self-selection explanations.

(Table 3 about here)

This positive effect is robust, but does not hold in bivariate models, which suggests the effect of employment is suppressed by other factors. According to Maassen and Bakker (2001), the likelihood of suppressor effects increases with the reliability of a variable. The reduced measurement error associated with an IV approach therefore makes suppressor effects likely. The effect of work becomes significant when controlling for race, ethnicity, state poverty rate, and number of state child labor officers, which Table 2 indicates are likely confounders.

Cultural and structural factors which may relate to both work and fertility are added in Model 4 – female headed household, people in the household, parental education, access to family planning, high rates of Catholicism and Protestantism, and male incarceration rate. All have significant associations with fertility (except Protestantism), but do not mediate the work-fertility relationship.

Table 4 shows significant interaction effects by race, ethnicity, individual income, and occupation. Compared to white youth, working has a significantly smaller effect for black, Asian, and Latino youth. This could be because minority youth more often live in low-income neighborhoods, which provide less shelter from adult contexts and could weaken work effects.

(Table 4 about here)

Model 2 shows that effects depend on occupation. Compared to service jobs, skilled and labor jobs dampen effects of work, contradicting opportunity cost and procyclical theories. Skilled occupations likely expose youth to adults, but also strong norms against precocious behavior. Similar effects for labor occupations suggest a slightly more complicated story. Labor jobs are generally perceived to promote prematurely adult or even deviant values. However, these jobs could enforce strong age differences, rules (e.g., the military), or anti-fertility norms for reasons this data cannot identify. For example, construction jobs may have rigid seniority rules, preventing youth from achieving structural or status equality with adults.

Like job type, individual income plays a role in the relationship between work and fertility. Teens in high paying jobs are less likely to have a child, regardless of job type. Figure 4 depicts these income and occupation interactions.

(Figure 4 about here)

Finally, theories differ in their expectations about effects of youth vs. adult job contexts. Model 3 in Table 4 shows youth jobs and childcare (more youthful contexts) are less related to fertility than more adult jobs. Consistent with life course theory, adult contexts promote fertility the most. (Apparent job type and income effects could also be due to selection.)

An interaction effect between household income and employment is significant, with higher household income reducing the effect of work on fertility. Work has a strong significant effect when analysis is limited to those from lower income households (less than $30,000), but

no effect among higher income youth (despite similar sample sizes). Results suggest employment has the strongest effect on young white women from low income backgrounds.

State level difference-in-difference models address time order concerns. If employment increases fertility, the state-level effect of work permit requirements should be seen at age 18, a year after the age 17 employment increases found in ACS data. Results confirm this. Among the cohort age 17 in 2006, those who lived in states restricting employment showed a smaller increase in fertility than those who did not (1% difference). The difference is significant when controlling for initial age 17 fertility rate (the bivariate difference is not). The full model controls for all factors included in the individual level analysis, plus proportion ever married, not in school, and living independently (all measured at the state level). Table 5 shows state level results, weighted by state age 17 population (excluding weights does not change the estimated effect but reduces its significance to $p<.10$ in Models 1 and 4).

(Table 5 about here)

NVSS results further address time order concerns and confirm individual and state-level ACS findings. Figure 5 shows local polynomial smoothing plots of changes in fertility rates as cohorts age from 14 to 19 (averaged from 1990 to 2006). The multi-year average corrects for measurement error, but individual years show similar results. Cohort analysis of the NVSS data using the intrinsic estimator (Yang et al. 2004) reveals only significant age effects, with null year and cohort effects, so averaging rate changes across years does not lose information. Importantly, at all ages except around the transition to age 18, Figure 5 shows insignificant differences in fertility rate changes by work permit requirement. States that require a work permit at age 17 show a significantly lower increase in fertility at age 18, but the difference becomes insignificant again by age 19.

(Figure 5 and Table 6 about here)

Table 6 shows NVSS results. The bivariate effect of requiring a work permit at age 17 significantly reduces the state-level increase in fertility from age 17 to 18 for all youth and white youth, but not for black youth (Models 1 and 2 in Table 6). Model 3 includes fertility rate changes from ages 14 through 19, with state fixed effects and age controls. Model 4 is similar, addressing first-order serial correlation for state changes in fertility rate over time, but also allowing autocorrelation of errors, the strength of which can vary over time. Limiting age 17 employment through work permit requirements significantly reduces fertility in all models.

Life course development theory suggests work should increase other adult behaviors as well. IV regressions of early transitions (fertility, ever having married, and being head of household) on work, with the same controls, indicate that employment increases adult transitions. The model in Table 7 is incomplete, has time-order concerns, and does not control for many factors potentially related to marriage or living independently, but it suggests a strong relationship between adolescent employment and other early transitions.

(Table 7 about here)

Effects of work on living independently and marriage are also investigated separately. In some models (e.g., Model 2 in Table 7), effects of employment significantly increase the chances of being head of household. However, results are sensitive to specification. Effects on marriage are not significant. State level difference-in-difference estimates also indicate a significant relationship between work and independent living, depending on specification. Overall, results suggest that employment speeds the time to adulthood – in the case of both fertility and independent living.

Sensitivity and Generalizability

Results are not sensitive to the measure of employment. Working at least 40 weeks in the last year, total number of weeks worked last year, hours worked last week, and hours worked last

week multiplied by total weeks worked all yield similar results. This is consistent with the view that work increases fertility through exposure time to adult contexts. Controlling for hours worked last week makes the effect of work insignificant, supporting exposure time as an important mechanism.

IV models identify the local average treatment effect – the effect of working for youth who are borderline in their decision to work – swayed by state employment certification requirements. It could be that employers are the determining factor and choose to hire fewer adolescents in states requiring work permits. If this is the case, results are more generalizable to typical 17 year old women. In general, the results do not apply to youth who would work regardless of state laws or those who would never work. This analysis is limited to adolescent women and there are likely heterogeneous effects of adolescent employment by gender (indeed, part of the rationale for this study).

A central limitation is lack of individual panel data. (The PSID-Child Development Supplement, for example, has too few cases of adolescent fertility.) However, individual and state-level results are remarkably similar (both ACS and NVSS), suggesting results do not reflect reverse causality.

Conclusion

Results of previous research could be explained by both opportunity cost and life course development theories. Findings from this study suggest adolescent employment increases adult behavior, supporting life course theory. Evidence shows that youth employment increases the likelihood of fertility (and probably independent living). As the theory predicts, the relationship depends on individual earnings, occupation type, and work context, as well as class and race. Thus, contrary to implicit assumptions in the 1996 welfare reform, encouraging employment is unlikely to reduce adolescent fertility.

The discrepancy between OLS and IV models illustrates the importance of addressing self-selection in youth employment research. Possibly due to higher aspirations, motivation, or some other unobserved factor, young women who are less likely to have a child are more likely to work.

Although this analysis cannot directly assess whether adult norms and contexts mediate the work-fertility relationship, effects of time at work, job type, and income are most consistent with this mechanism. Exposure time appears to encourage fertility. Jobs offering more youthful contexts and social controls show a smaller overall increase in the likelihood of fertility. Early employment has the strongest effects for low income white women, whose contexts may be most influenced by employment.

Broadly, evidence suggests a life course perspective is critical to understanding adolescent employment. Results of this analysis support it, but life course theory also helps account for previous apparently contradictory findings (e.g., Apel et al. 2008). Research frequently labels outcomes as "positive" or "negative," but this study suggests "adult" vs. "adolescent" is a better characterization.

Adolescent employment has changed in the decades since Elder's (1974) pioneering study. Youth are now concentrated in service jobs which, findings suggests, have the most precocious effects. Evidence indicates that delaying employment can further delay other transitions to adulthood, depending on one's background. The current economic recession could reduce adolescent employment and further slow the already lengthening transition identified by Furstenberg (2008), Arnett (2004), and Kimmel (2008). Whether this delay is positive or negative depends on the outcome in question and beliefs about adolescence.

Adolescent employment encourages adult behavior. The challenge is that society has contradictory expectations of adolescents – we want them to behave like adults in some respects,

but not others. Decisions about adolescent employment should consider whether we want more adult behavior (both the "good" and the "bad") among youth. At the same time, criticizing stereotypical adolescent behavior is futile; adolescents will "grow up" – with all the accompanying "good" and "bad" behavior – when exposed to adult contexts.

The heterogeneous effects of adolescent employment by class could help explain the different pathways to adulthood these youth experience. Arnett's (2004) extended adolescence resonates most with youth from higher class backgrounds. Findings suggest employment can have long-term effects, particularly among low-income white women. Rather than providing an opportunity for self-discovery and experimentation, which Arnett (2004) and Erikson (1950) suggest are so important for youth development, employment nudges young women from different backgrounds onto very different life trajectories with long-term consequences.

Appendix

A. Additional Details about the American Community Survey Sample and Measures

Exclusions

Young women who have ever married, are not in school, or are living on their own may be more likely to have a child. But controlling for these factors would introduce endogeneity into the model. To address this, the main sample of 17 year old women excludes those who: have ever married (310; 1%); are not in school (an additional 1107; 5%); are head of a household or living alone (a further 162; 1%); have allocated values for marital status (another 270, 1%), school enrollment (another 466, 2%), when worked (another 497, 2%), weeks worked (another 520, 2.5%), or fertility (another 108, 0.5%). Noncitizens (an additional 744; 4%) and those living in Alaska (an additional 43; 0.2%) are excluded to strengthen the IV. Noncitizens may be less affected by youth age employment laws and Alaska uniquely requires employment certification until age 17, which is the age group most sensitive to other state laws. Finally, the sample excludes those without head of household educational attainment data (an additional 207; 1%) to make results comparable across regressions and because parental or household education may play an important role in both adolescent employment and fertility.

Validity

ACS has a rolling reference point, so we cannot know for sure what age an individual was when giving birth or in exactly which year the birth occurred. Because surveys occur throughout the year, many of the births to 17 year olds in the 2006 ACS occurred in 2005 and while the teen was 16 years old. Therefore, in assessing validity, the fertility rates for 17 year olds in this sample should fall between the rates for 16 and 17 year olds (in 2005 and 2006).

The rate for all 17 year olds in the ACS (including those not in my sample) is 2.4%. This falls comfortably between the rates for 16 and 17 year olds in the NVS data for 2005 and 2006. The fertility rate of adolescents age 16 and 17 was 1.9% and 3.6%, respectively (www.cdc.gov/nchs/nvss.htm). Other comparisons suggest the ACS sample is valid, but may have a slightly lower adolescent fertility rate than the population as a whole.

According to the Bureau of Labor Statistics (www.bls.gov/data), 33% of 16-17 year old women were in the seasonally adjusted civilian labor force in 2005 and 2006. The unadjusted labor force participation rate was 34% in both 2005 and 2006. This compares to 43% (42% weighted) of women aged 16 to17 in the 2006 ACS. The higher ACS rates of employment could reflect sample bias, but are probably due to the broader definition of employment, which includes anyone who worked for a few days in the last 12 months rather than those currently in the labor force. This broader definition is appropriate for teenagers, who cycle in and out of work more frequently than adults and captures employment that other surveys may miss.

Measures

Skilled jobs include those in the following occupations: management; business and finance; legal; computer, mathematical, or engineering; science; counseling and therapy; acting, producing, or directing; healthcare practice and support; and managers in all other occupational categories. Service jobs include: social work and health education; education; arts and entertainment (aside from acting, producing, and directing); protective service; food preparation; cleaning and maintenance; personal care and service; sales; and office and administrative support (excluding managers in each category). Labor jobs include: farming, fishing, and forestry; construction; installation/repair; production; transportation; and the military.

States with a high proportion of Catholics (31% or more in 2000) or Protestants (13%) provides a cultural measure to control for potential normative differences. State-level religion data is gathered from the Association of Religion Data Archives (www.thearda.com). An index of factors that limit the availability of family planning is created based on state-level data gathered from the Guttmacher Institute (www.guttmacher.org). It is a sum of the following dummy variables indicating states that: require parental consent or notification for an abortion; have a mandatory waiting period after counseling before an abortion; and limit public funding of abortion. The factor loadings are all above 0.67 and Cronbach's alpha is 0.86.

Availability of male partners may explain any relationship between adolescent employment and fertility. If economically attractive male partners are unavailable, adolescent fertility may be more strongly related to employment of potential mothers (e.g., if they know they will need to rely on their own earnings whenever they start a family regardless of age). Therefore, the full model includes a measure of state male incarceration rates from the Bureau of Justice Statistics. It measures "the number of prisoners with a sentence of more than 1 year" per 100,000 state residents at the end of 2006 (divided by 1000), with populations based on January 1, 2007 census population estimates (Sabol et al. 2007:17-18).

Female headed household, number of people in the household, highest years of education of head of household or their spouse, and generation age gap (included in models not shown) may be related to fertility and work. All four variables are constructed from household (not equivalent to family) data and, therefore, may have substantial measurement error. Number of people in the household is the number reported, minus 1 if an adolescent in the household had a child in the last 12 months. Female headed household is an indicator of whether the reference person is a single female. More educated parents could encourage adolescent employment and experimentation in the adult world, but also teach girls about contraception or stress the

importance of going to college. More educated parents could keep better tabs on their teens, steer them into better jobs, or counteract peer influence at work. Models therefore control for the highest year of education attained by either the head of the household or their spouse.

Generation age gap is constructed by calculating the difference between the age of the female parental figure (female reference person or spouse) and the maximum age of the second generation (child or step/foster child of the reference person) in the household. This measure is problematic because it requires a female parental figure in the household and older children may have already left the household. This measure has the highest number of missing values (2042/20740 or 10%) and would limit the sample size to teens living with a female parental figure. Analyses shown do not include it, but controlling for it does not change the results.

Household size-adjusted income is measured by dividing total household income (minus any income earned by youth to prevent collinearity with youth employment) by the square root of the number of people in the household. This is used by the Congressional Budget Office and measured in $100,000s because of the tiny coefficient.

Child labor enforcement data is obtained from the 2004 Child Labor State Survey sent to all state labor departments (www.stopchildlabor.org/USchildlabor/2004_survey_results.htm) by the Child Labor Coalition. It asked how many labor compliance officers are exclusively responsible for inspecting workplaces for child labor compliance/violations. States with such officers include Alabama, Alaska, Arizona, Florida, New Mexico, Oklahoma, and Texas. The survey had an extremely low response rate (only 63% of states) and of the states that responded, few have child labor compliance officers. Because survey non-response indicates some lack of concern for child labor enforcement, non-responses are set to 0. Despite low response, controlling for this variable helps address the possibility that any relationship found between employment and fertility is only the result of lack of state child labor law enforcement. For example, employment may only increase fertility if child labor enforcement is extremely lax. States with strong enforcement may expose working youth to more positive experiences and peers. Results are the same when adjusting number of child labor officers for state population size or controlling for states missing this data.

Region is determined by the Census region categories. Proportion of the state below poverty is based on ACS data. Proportion of the state population that is black could affect the relationship, but it is correlated with state poverty rates and not included in models.

B. Example of Employment Certification Requirements: Additional Support for the IV

The North Carolina Department of Labor Youth Employment Certificate provides an example of the additional steps required. According to this document (www.nclabor.com/wh/yec.pdf), a youth under 18 seeking employment must: 1) download the form from the internet or, "as a last resort", call the Wage and Hour Bureau to get a copy; 2) complete the information; 3) have the employer provide a job description, company name and address, type of business, and indicate whether there is an alcohol permit on the premises (i.e., an individual must have been offered a job already, which delays employment start dates and could frustrate employers); 4) get the form signed by a parent or guardian; 5) take the form and a proof of age document to their local Department of Social Services office or a location of an approved designee; 6) give a copy of the form to her employer by at least her first day of work; and 7) the employer must keep the certificate on file. The does not apply to governmental (public), agricultural, or domestic employers, who do not need employment certificates to employ youth under age 18.

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Figures and Tables

Figure 1: Conceptual Framework of Competing Theories

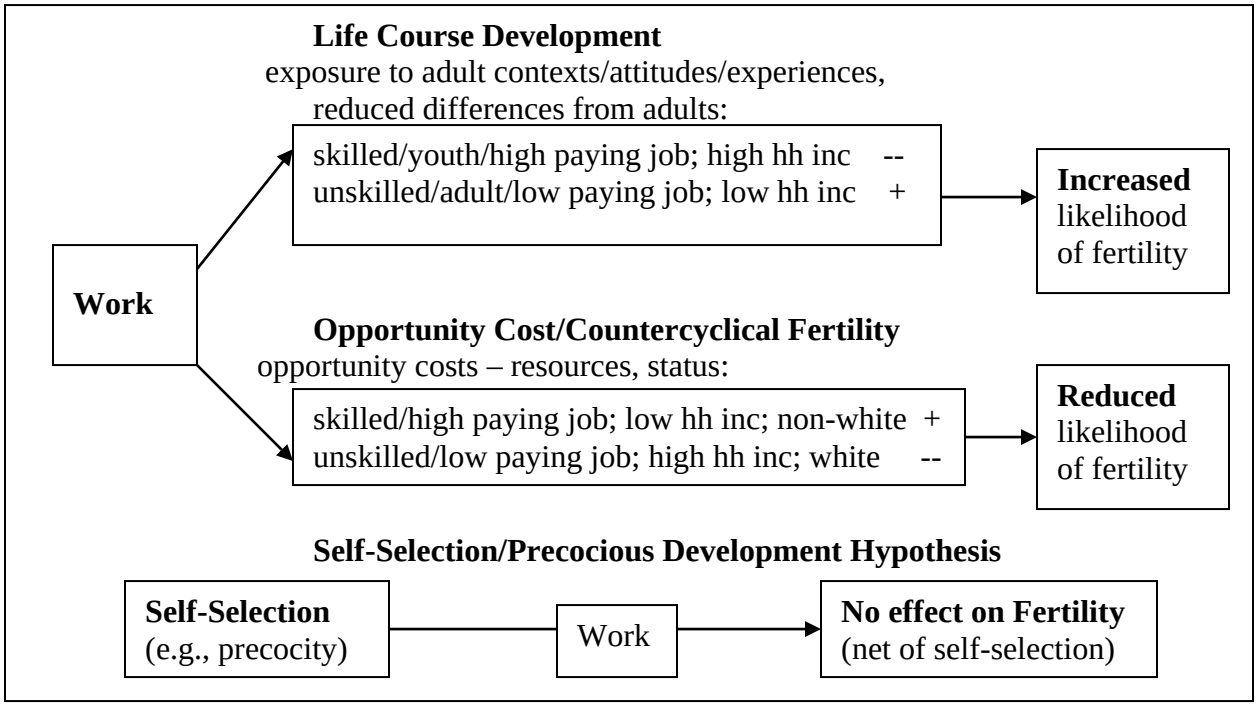
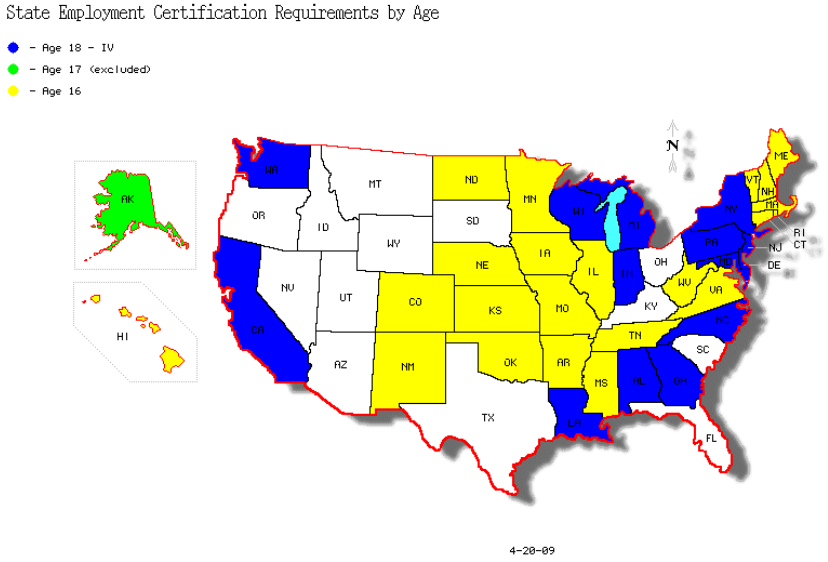
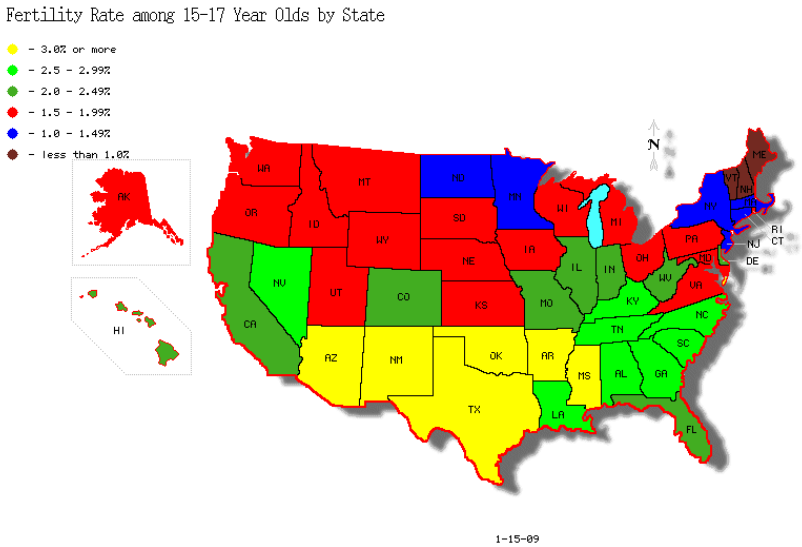


Figure 2: Instrumental Variable Map: Employment Certification Required until Age Specified



Note: Employment certification is not required if blank; Ohio requires certification until age 18 during school terms only – results shown in this paper treat Ohio as requiring certification until Age 16, but including it in the Age 18 category does not change results. IV includes states shaded the darkest color compared to all others.

Figure 3: Adolescent Fertility Rate by State in 2006



3% or more: AZ, AR, DC, MS, NM, OK, TX
2.5 – 2.99%: AL, GA, KY, LA, NC, NV, SC, TN
2.0 – 2.49%: CA, CO, DE, FL, HI, IL, IN, MO, WV
1.5 – 1.99%: AK, IA, ID, KS, MD, MI, MT, NE, OH, OR, PA, RI, SD, UT, VA, WA, WI, WY
1.0 – 1.49%: CT, MA, MN, ND, NJ, NY
< 1.0%: ME, NH, VT
Source: Martin et al. 2009:49.
Lighter colors indicate higher adolescent fertility rates.

Table 1: State Averages by IV Category: Whether Employment Certification Required at Age 17

State Averages	No Work Permit Required Age 17		Work Permit Required Age 17	
(based on 17 yo women)	N=35		N=15	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
Dependent Var:				
Fertility Age 17	0.023	0.012	0.019	0.009
Fertility Change Age 18-17	0.015	0.013	0.010	0.008
Race and Ethnicity:				
White *	0.759	0.105	0.681	0.119
Black	0.111	0.088	0.146	0.103
Asian *	0.024	0.031	0.056	0.042
Native Amer/HI	0.004	0.016	0.003	0.003
Other race	0.056	0.053	0.077	0.070
Multiracial	0.034	0.030	0.030	0.016
Latino	0.148	0.134	0.159	0.151
Household Chars:				
Hh size adjusted inc ($100k)	0.386	0.068	0.407	0.061
Female headed hh	0.248	0.042	0.248	0.032
People in household	4.132	0.183	4.212	0.173
Hh head educ max	10.246	0.428	10.164	0.412
Gen age gap(/100)	26.043	0.689	26.267	0.527
State Chars:				
Abortion restrictions	2.000	1.102	1.335	1.462
State high Catholic	0.168	0.380	0.360	0.497
State high Protestant	0.107	0.313	0.198	0.412
Male incarceration rate (/1k)	0.851	0.270	0.819	0.222
Child labor officers *	1.716	2.870	0.067	0.372
Poverty rate (/10)	1.355	0.290	1.309	0.229
Northeast	0.090	0.290	0.285	0.467
South	0.455	0.505	0.243	0.444
West	0.161	0.373	0.301	0.475
Midwest	0.294	0.462	0.170	0.389
Independent Variables:				
Worked last 12 months *	0.561	0.089	0.489	0.100
Worked over 39 weeks	0.160	0.055	0.131	0.051
Weeks worked last 12 mos *	13.652	3.469	11.494	3.563
Individual income (10k) *	0.207	0.033	0.182	0.039
Potential Confounders:				
Educational attainment	10.744	0.079	10.803	0.131
Ever married	0.016	0.009	0.013	0.005
Not in school	0.062	0.026	0.053	0.017
Head of household *	0.019	0.008	0.014	0.006

* Indicates significant difference in weighted averages.
Excludes Alaska, which requires employment certification until age 17.
State averages are based on 17 year old women in 2006 ACS sample, weighted by population.
Unweighted averages show significant differences only in work variables and proportion of the population that is white or black.

Table 2: Descriptive Statistics by Race and Ethnicity

	All N=16306		White N=12216		Black N=1933		Asian N=502		Latino N=2111	
Variable	Mean	St Dev	Mean	St Dev	Mean	St Dev	Mean	St Dev	Mean	St Dev
Dependent var:										
Adolescent fertility	0.014	(0.118)	0.010*	(0.098)	0.032*	(0.175)	0.011	(0.105)	0.021*	(0.143)
Head of household♣	0.017	(0.131)	0.015*	(0.122)	0.025*	(0.155)	0.012	(0.107)	0.028*	(0.164)
Ever married♣	0.015	(0.122)	0.015	(0.120)	0.010	(0.098)	0.031	(0.174)	0.031*	(0.174)
Early transitions♣	0.057	(0.274)	0.049*	(0.264)	0.074*	(0.281)	0.052	(0.228)	0.105*	(0.394)
Race and Ethnicity:										
White	0.706	(0.456)	1	(0)	0	(0)	0	(0)	0.479	(0.500)
Black	0.151	(0.358)	0	(0)	1	(0)	0	(0)	0.013	(0.114)
Asian	0.032	(0.176)	0	(0)	0	(0)	1	(0)	0.006	(0.078)
Native Amer/HI	0.003	(0.058)	0	(0)	0	(0)	0	(0)	0.004	(0.065)
Other race	0.064	(0.245)	0	(0)	0	(0)	0	(0)	0.410	(0.492)
Multiracial	0.033	(0.179)	0	(0)	0	(0)	0	(0)	0.078	(0.268)
Latino	0.144	(0.351)	0.097	(0.297)	0.012	(0.110)	0.025	(0.156)	1	(0)
Household chars:										
Hh SA inc ($100k)	0.412	(0.401)	0.466*	(0.435)	0.239*	(0.205)	0.424	(0.356)	0.304*	(0.298)
Female headed hh	0.262	(0.440)	0.197*	(0.397)	0.560*	(0.496)	0.152*	(0.360)	0.269*	(0.444)
People in household	4.193	(1.428)	4.122*	(1.331)	4.160	(1.585)	4.589*	(1.552)	4.660*	(1.656)
Hh head educ max	10.428	(2.742)	10.745*	(2.600)	9.890*	(2.381)	10.661*	(3.444)	8.648*	(3.520)
Gen age gap(/100)♦	0.265	(0.056)	0.270*	(0.055)	0.248*	(0.061)	0.274*	(0.055)	0.253*	(0.057)
State characteristics:										
Abortion restrictions	1.681	(1.304)	1.755*	(1.281)	1.931*	(1.228)	0.732*	(1.173)	1.175*	(1.330)
State high Catholic	0.263	(0.440)	0.270*	(0.444)	0.259*	(0.438)	0.231	(0.422)	0.194*	(0.395)
State high Protestant	0.146	(0.353)	0.167*	(0.373)	0.109*	(0.312)	0.074*	(0.263)	0.043*	(0.203)
Male incarc rate (/1k)	0.838	(0.246)	0.823*	(0.245)	0.907*	(0.257)	0.805	(0.203)	0.907*	(0.228)
Child labor officers	0.931	(2.230)	0.892*	(2.191)	0.998	(2.274)	0.610*	(1.881)	1.838*	(2.959)
Poverty rate (/10)	1.337	(0.260)	1.325*	(0.255)	1.402*	(0.296)	1.267*	(0.214)	1.366*	(0.235)
Northeast (omitted)	0.182	(0.386)	0.190*	(0.393)	0.169*	(0.374)	0.170	(0.376)	0.130*	(0.336)
South	0.353	(0.478)	0.334*	(0.472)	0.560*	(0.496)	0.167*	(0.374)	0.309*	(0.462)
Midwest	0.234	(0.423)	0.264*	(0.441)	0.189*	(0.392)	0.119*	(0.324)	0.091*	(0.288)
West	0.231	(0.421)	0.212*	(0.409)	0.082*	(0.275)	0.544*	(0.499)	0.470*	(0.499)
Empl cert 18 (IV)	0.471	(0.499)	0.440*	(0.496)	0.543*	(0.498)	0.678*	(0.468)	0.502*	(0.500)
Independent vars:										
Worked last 12 mos	0.510	(0.500)	0.570*	(0.495)	0.346*	(0.476)	0.345	(0.476)	0.380*	(0.486)
Worked >39 weeks	0.146	(0.353)	0.173*	(0.378)	0.063*	(0.244)	0.097	(0.296)	0.097*	(0.296)
Indiv inc ($10k)	0.178	(0.497)	0.194*	(0.556)	0.144*	(0.316)	0.123	(0.378)	0.147*	(0.314)
Educ attainment	10.767	(0.791)	10.783*	(0.742)	10.671*	(0.909)	10.977	(0.789)	10.769*	(0.842)
Of those who worked:	N=8844		N=7243		N=725		N=180		N=804	
Skilled job	0.025	(0.155)	0.022	(0.147)	0.029	(0.168)	0.030	(0.170)	0.029	(0.167)
Service job	0.866	(0.341)	0.863*	(0.344)	0.887*	(0.317)	0.901	(0.299)	0.874	(0.332)
Labor job	0.038	(0.190)	0.036	(0.187)	0.038	(0.192)	0.039	(0.193)	0.049*	(0.215)
High youth job	0.588	(0.492)	0.588	(0.492)	0.642*	(0.480)	0.480*	(0.501)	0.597	(0.491)
Childcare job	0.051	(0.219)	0.055*	(0.227)	0.033*	(0.180)	0.016	(0.127)	0.034	(0.180)

Standard deviations in parentheses.

♣ N=18379

♦ N=15058 (not included in regressions due to missing values, but results do not differ when included)

Table 3: Relationship between Employment and Fertility among Adolescent Women

	Adolescent Fertility					
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	IV	OLS	Logit	IV	OLS	Logit
Worked in last 12 mos	0.090*	-0.007**	-0.429*	0.155**	-0.005*	-0.344+
	(0.042)	(0.002)	(0.197)	(0.043)	(0.002)	(0.195)
Single female headed hh				0.020**	0.017**	0.655**
				(0.004)	(0.003)	(0.183)
People in household				0.012**	0.010**	0.386**
				(0.002)	(0.002)	(0.039)
Max educ hh head/spouse				-0.004**	-0.002**	-0.094**
				(0.001)	(0.001)	(0.034)
State abortion restrictions				-0.007**	-0.000	0.004
				(0.002)	(0.001)	(0.122)
State high Catholic				-0.014**	-0.005	-0.456
				(0.005)	(0.004)	(0.381)
State high Protestant				0.005	0.003	0.180
				(0.004)	(0.004)	(0.298)
State male incarceration rate				0.025**	-0.004	-0.165
				(0.010)	(0.007)	(0.551)
Constant	-0.076*	-0.002	-4.652**	-0.133**	-0.026*	-5.729**
	(0.034)	(0.006)	(0.433)	(0.032)	(0.012)	(0.651)
Observations	16306	16306	16306	16306	16306	16306
R-squared		0.011	0.082		0.027	0.143
F statistic [a]	10.25§			15.10§		
Endog test of Employment [b]	4.87*			7.13**		

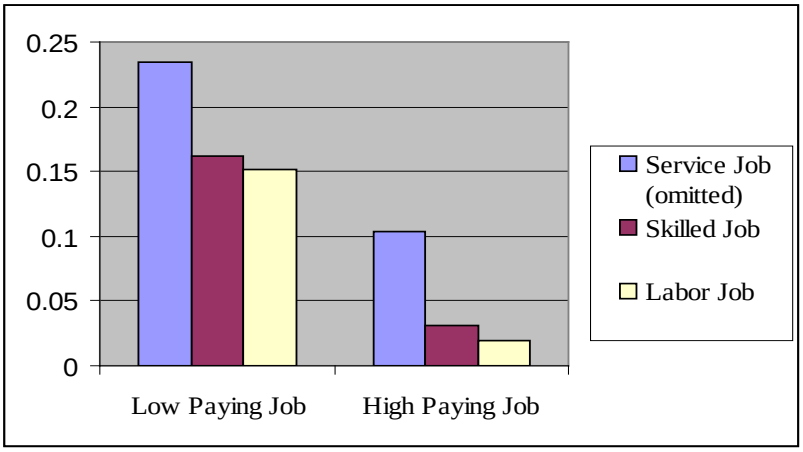
+ p<0.1, * p<0.05, ** p<0.01 Robust standard errors in parentheses

[a] Kleibergen-Paap rank Wald F statistic indicates test of IV strength is above Stock-Yogo (2005) critical values: § = 15%; ♦ = 20%; ¤ = 25%

[b] Endogeneity test indicates difference between 2 Sargan-Hansen statistics, robust to heteroskedasticity

All models include the following variables, not shown: Black** (in 1-4), Asian** (in 1 & 4), Native American/HI Islander; Other race; Multiracial; Latino** (in 1 & 4); Household income to needs* (except 5); State child labor officials**; State poverty rate*; South (* in 1-3); Midwest (* in 4); West.

Figure 4: Effect of Work on Likelihood of Adolescent Fertility by Job Type



(Predictions based on Model 2 in Table 4. Note that job type is endogenous.)
Predicted probability of fertility for an otherwise identical worker and non-worker by job type.

Table 4: Employment Effects by Race, Ethnicity, Individual Income, and Occupation

VARIABLES	Adolescent Fertility (1)	(2)	(3)	(4)
Worked in last 12 months	0.345*	0.234**	0.337**	0.242**
	(0.150)	(0.079)	(0.128)	(0.078)
Black * worked last 12 mos	-0.301*			
	(0.129)			
Asian * worked last 12 mos	-0.298*			
	(0.125)			
Latino * worked last 12 mos	-0.292*			
	(0.124)			
Skilled job		-0.072**		
		(0.025)		
Labor job		-0.083**		
		(0.027)		
High youth job			-0.203**	
			(0.077)	
Childcare job				-0.095**
				(0.031)
High indiv income*worked last 12 mos		-0.227**		
		(0.071)		
High individual income (60th percentile)	-0.115*	0.096**	-0.094**	-0.110**
	(0.051)	(0.028)	(0.036)	(0.036)
Educational attainment	-0.021**	-0.015**	-0.019**	-.018**
	(0.007)	(0.004)	(0.006)	(0.005)
Constant	0.027	0.027	0.079*	-0.134**
	(0.029)	(0.030)	(0.039)	(0.037)
Observations	16306	16306	16306	16306
F statistic [a]	4.94	10.85§	7.76♦	11.64§
Endogeneity test of Employment [b]	6.91**	6.90**	6.92**	6.90**

+ p<0.1, * p<0.05, ** p<0.01 Robust standard errors in parentheses

[a] Wald F statistic test of IV strength is above Stock-Yogo (2005) critical values: § = 15%; ♦ = 20%; ¤ = 25%

[b] Endogeneity test indicates difference between 2 Sargan-Hansen statistics, robust to heteroskedasticity

All models include: Black*; Asian*; Latino*; Native American/HI Islander; Other race; Multiracial; Single female headed household**; People in household**; Max education of household head/spouse**; State abortion restrictions*; State high Catholic (** in 3, 4); State high Protestant; State male incarceration rate (* in 3, 4); Household income to needs (* in 2-4); State child labor officers (* in 2-4); State poverty rate**; South; Midwest (* in 3, 4); West (* in 3, 4). Note that job type, individual income, and educational attainment are all endogenous.

Table 5: Difference-in-Difference: Employment Restriction Effects on Adolescent Fertility

VARIABLES	Change in State Fertility Rate (Age 17-18, 2006-2007) (1)	(2)	(3)	(4)
Employment Certification Required	-0.007*	-0.008*	-0.007*	-0.013*
	(0.003)	(0.003)	(0.003)	(0.006)
Constant	0.027**	0.007	0.013	0.478
	(0.003)	(0.010)	(0.016)	(0.283)
Observations	50	50	50	50
R-squared	0.239	0.337	0.358	0.741

+ p<0.1, * p<0.05, ** p<0.01 Robust standard errors in parentheses

Models include the following state-level measures (based on 17 year old average): **1)** 2006 fertility rate** **2)** 2006 fertility rate**; poverty rate. **3)** 2006 fertility rate**; poverty rate; child labor officers; proportion of the population that is white. **4)** 2006 fertility rate**; poverty rate; child labor officers; proportion of the population that is black, Asian, Native American or Hawaiian, other race, multiracial, and Latino; average household income to needs; average female headed household; average people in household; average max head/spouse of household education; average generation age gap; abortion restrictions*; state high Catholic*; state high Protestant; male incarceration rate**; South; West; Midwest; average educational attainment; average ever married; average not in school; average head of household.

Figure 5: NVSS Local Polynomial Smooth Plot – All Youth, Average 1990-2006

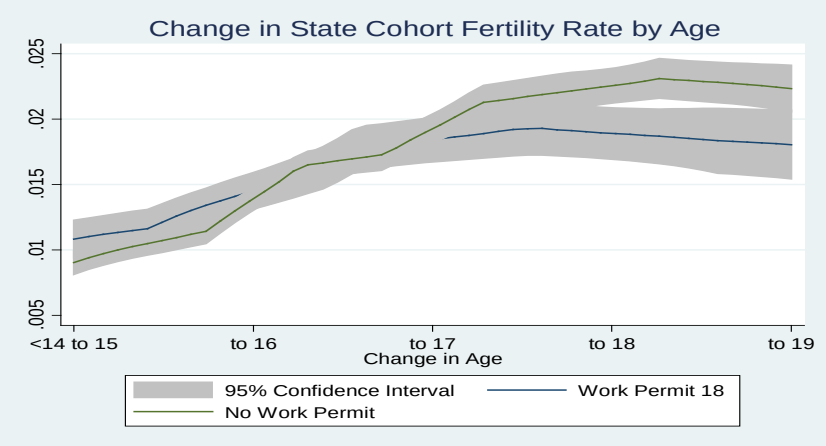


Table 6: NVSS – State Change in Cohort Fertility Rate by Employment Certification Law

VARIABLES	(1) Δ Fertility Rate	(2) Δ White Fertility Rate	(3) Δ Fertility Rate	(4) Δ Fertility Rate
	Age 17-18 only	Age 17-18 only	State Fixed Effects Age 14-19	State FE, AR1 Age 14-19
Empl cert18_Age 18	-0.004+	-0.005*	-0.004*	-0.002**
	(0.002)	(0.002)	(0.001)	(0.001)
Age 16			0.008**	0.008**
			(0.001)	(0.000)
Age 17			0.012**	0.012**
			(0.001)	(0.001)
Age 18			0.017**	0.016**
			(0.001)	(0.001)
Age 19			0.013**	0.013**
			(0.001)	(0.001)
Constant	0.024**	0.023**	0.007**	0.007**
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	51	51	255 (51 states*5)	255 (51 states*5)
R-squared	0.074	0.087	0.703	

** p<0.01, * p<0.05, + p<0.1 Standard errors in parentheses

Model 4 includes an indicator for Employment Certification until age 18, addresses first-order serial correlation for state changes in fertility rate over time, and allows autocorrelation, the strength of which can vary over time.

Table 7: Effects of Adolescent Employment on Other Transitions to Adulthood

VARIABLES	Early Transitions (1)	Head of Household (2)
Worked last 12 months	0.430**	0.124*
	(0.148)	(0.060)
Constant	-0.192+	-0.009
	(0.105)	(0.047)
Observations	18379	18379
F statistic [a]	9.47§	9.47§
Endogeneity test of Employment [b]	9.38**	3.92*

+ p<0.1, * p<0.05, ** p<0.01 Robust standard errors in parentheses

[a] Wald F statistic test of IV strength is above Stock-Yogo (2005) critical values: § = 15%; ♦ = 20%; ¤ = 25%

[b] Endogeneity test indicates difference between 2 Sargan-Hansen statistics, robust to heteroskedasticity

Models include: Black*; Asian (** in 1); Native Amer/HI Islander; Other race; Multiracial; Latino*; Female headed household**; People in household (* in 1); Max education of household head/spouse**; State abortion restrictions (* in 2); State high Catholic (* in 1); State high Protestant (* in 1); State male incarceration rate (* in 1); Household income to needs**; State child labor officials (* in 1); State poverty rate (** in 1); South; Midwest (** in 1); West.