

Is rising non-teacher pay to blame for falling teacher quality?

Lessons from the introduction of the birth control pill

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Abstract

The average quality of teachers declined precipitously between 1960 and 2000, coinciding with a decline in the ratio of pay in teaching as compared to pay in alternative professions: relative pay. The effect of relative pay on the quality of teachers is difficult to measure because teacher pay may be correlated with working conditions, such as the school's neighborhood quality, student behaviors, and other challenges associated with the work environment. To address this concern I exploit state-by-cohort variation in whether women had legal access to the birth control pill in young adulthood. Previous work has shown that young adult pill access improved early career investments by enabling better control of childbirth timing, producing landmark improvements in the alternative professional pay available to high-ability women. This lowered the effective relative teacher pay for high-ability women. I therefore use a measure of young adult pill access as an instrument for relative pay. This instrumental variables approach assumes that the pill rollout is random with respect to state-by-cohort variation in working conditions, thus producing unbiased estimates of the effect of relative pay on the propensity to teach among women who entered the labor market between 1960 and 1975. The primary results indicate that a 10 percent increase in relative pay increases the likelihood of choosing to teach by 5 percentage points. In addition, my results show no significant differences in the labor supply elasticity to teaching by ability suggesting that high-ability women are about equally responsive to relative pay as low-ability women. In culmination, the results reveal that the opportunity cost to teaching produces a significant effect on the average quality of U.S. teachers.

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Introduction

The teaching profession has been predominantly and increasingly female since the turn of the nineteenth century (Belfield, 2005; Strober and Lanford, 1986). However, the type of woman for whom teaching is a choice career has changed. While the profession was once comprised of mostly upper middle class and relatively well-educated women (Strober and Lanford, 1986), teaching has increasingly become a second tier option among women of the highest ability (Corcoran et al 2004).² One prominent explanation for twentieth century declines in average teacher quality is the expansion of women's labor market opportunities, which increased the diversity of careers and accompanying wage trajectories available to women (Goldin, 2006).³ Teacher wages, on the other hand, have been compressed by ability, mostly between 1960 and 1965, due to union pressure that reduced access to elevated pay trajectories for women of the highest ability (Flyer and Rosen, 1997; Hoxby and Leigh, 2004). As a result, women have experienced a decline in the ratio of teacher pay to alternative professional pay (hereafter referred to as relative pay) on the order of about 12 percent between 1960 and 1990 (Loeb and Page, 2000).

These trends provide prima facie evidence that relative pay may have contributed to the significant decline in average teacher quality over the past several decades. Further evidence for this hypothesis is found by disaggregating the changes in teacher quality and relative pay by race and gender. These results show the most substantial declines in average teacher quality for black women, the group who experienced the largest increases in alternative professional pay between 1960 and 2000. Following a consistent pattern, white men experienced the smallest changes in both relative pay and the propensity to teach since 1960.⁴

² Average teacher quality among women in the U.S., measured in one project by the high school test scores of new teachers, fell from 0.60 standard deviations above the mean female high school graduate in 1964 to 0.46 standard deviations above the mean in 2000. The research design used by Corcoran et al. (2004a) involved combining five longitudinal surveys for men and women in the high school classes of 1957, 1960-64, 1972, 1982 and 1992. As a result, the tests analyzed changed over time. However, the authors explain that the math and verbal scores used from each test are similar in content to the SAT and ACT.

³ Goldin (2006) argues that increases in the labor market participation, number of hours worked and the types of careers pursued by women in the last century have contributed the largest single influence on the changing U.S. labor market in the last century.

⁴ Figure 3 shows these trends using data from the Integrated Public Use Microdata Series (IPUMS), 1960-2000. The figure reveals declines in relative teacher wage for every subgroup between 1960 and 1990 with the most substantial declines shown for black women and the least for white men. The figure also shows black women experienced the largest decline in teaching propensity over the period while white men experienced the smallest changes in teaching propensity.

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To estimate the strength of the link between relative pay and teacher quality, previous research uses variation in wages by occupation to demonstrate that states and cohorts that experienced greater declines in relative pay have, in turn, been subject to larger declines in average teacher quality (Bacolod, 2007; Loeb and Page, 2000).⁵ One concern associated with estimating relative pay effects using state-by-cohort occupational wages is that teachers may be more sensitive to their working conditions than workers in alternative professions. The working conditions of teaching include the school's neighborhood quality, characteristics of the students attending the school, and the school's auxiliary resources. If teacher pay and working conditions are positively correlated, the relative influence of the school's working conditions will cause pay estimates to be overstated (Belfield, 2005; Hanushek, 1997; Hanushek and Rivkin, 2006).

The ideal means to circumvent such bias is to exploit exogenous variation in relative pay. This allows a researcher to identify the effect of relative pay on occupational choice, unrelated to working conditions. A natural experiment with such plausibly exogenous variation in lifetime average pay is provided by the low-cost means to delay pregnancy enabled by oral contraception that in turn transformed women's wage trajectories (Bailey, 2006; Bailey et al., 2012; Goldin and Katz, 2002; Hock, 2007).

Legal access to the birth control pill at ages 18-20, hereafter referred to as young adulthood, was rolled-out by state between 1960 and 1975. Differences in the date of legal access were largely due to variation in state legislative policies that lowered the legal age of majority.⁶ In particular, I present evidence that the pill rollout is unrelated to geographic differences in teacher working conditions.

The power of pill access in young adulthood on delaying childbearing is well established in the literature: among cohorts born before 1940, of whom none had young adult pill access, about 62 percent of women ever having children gave birth prior to age 22. On the other hand, for women born around 1955, nearly all of whom had legal pill access in young adulthood, only 25 percent of those ever having children gave birth prior to age 22 (Bailey, 2006). Moreover, previous research has demonstrated that young adult pill access transformed women's wage trajectories. The ability to postpone pregnancy beyond age 22 enabled women to invest in their early work careers and to strive for work with the potential for greater returns to ability (Bailey et al., 2012; Hock, 2007). Because the wage trajectory of teaching is less responsive to productivity, the quality of the teaching labor force may have been one of

⁵ Using this method, Bacolod (2007) shows a 10 percent increase in alternative professional pay, holding constant teaching pay, decreases teaching propensity for the highest ability women by 6.4 percent as compared to by only 3.7 percent for lower ability women.

⁶ The age of majority was lowered largely for reasons unrelated to variation in social acceptance of women's work or family planning (Bailey et al, 2012).

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the principal losers of the pill and women's work-liberation movements (Goldin, 2006; Goldin and Katz, 2002; Goldin et al., 1997; Lazear and Rosen, 1990; Steingrimsdottir, 2010).

This paper uses nationally-representative samples of women born between 1943 and 1954 from the National Longitudinal Survey (NLS) and Integrated Public Use Microdata Series (IPUMS), 1960-2000, to analyze labor market outcomes of cohorts that straddle the period of the pill rollout and the subsequent 30 years during which the average ability of women entering the teaching profession declined. The research design identifies the effect of relative lifetime wage in teaching as compared to alternative professional occupations on the propensity to choose a teaching career, by ability group, using changes in occupation-specific pay trajectories induced by young adult pill access. The model successfully identifies an unbiased estimate of the effect of relative pay on occupational choice under the assumption that the state-by-cohort pill rollout is uncorrelated with state-by-cohort differences in non-pay factors affecting occupational choice—namely working conditions. Therefore, this model assumes that because the rollout is limited to young adult women who were not engaged in professional work at the time of the rollout, and all of whom obtained legal access at age 20, that the state-by-cohort variation in pill access is unrelated to occupational choice except through changes in wage opportunities induced by supply-side forces.⁷

The first set of regressions examine the reduced-form effect of young adult pill access on teacher quality. State, birth cohort, and race fixed effects are included in all regressions to account for persistent differences between states, cohorts, and racial groups. These reduced-form estimates provide evidence on the effects of increasing the opportunity cost to teaching for higher ability women on the average quality of women in the teaching profession. In all models, I proxy for high-quality with an indicator identifying whether a woman is in the top third of the IQ distribution.⁸ The results show that young adult pill access increases the propensity of high-ability women to pursue alternative professions. The results also show

⁷ Crucial to this argument is the age specific nature of the rollout. Given empirical and theoretical evidence that young adult pill access did not affect a woman's demand for the number of children, and only their birth timing (Bailey, 2006), I argue that it is unlikely that childbirth timing influences the non-wage benefits of teaching in a manner that can be anticipated at 18-20. Biased estimates using this instrumental variables strategy would arise if one instead believes that women anticipated lifetime non-pay occupational benefits at ages 18-20, which seems less plausible. Moreover, the reduced-form findings, which do not require zero correlation between young adult pill access and work conditions, imply the same result as the instrumental variables findings: the increasing opportunity cost that high-ability female teachers experienced between 1960 and 2000 negatively influenced average teacher quality in the U.S..

⁸ While cognitive achievement yields the most salient connection to student gain scores overtime amongst observable teacher inputs (Rivkin, Hanushek and Kain 2005, Ehrenberg and Brewer 1994), an ideal research design would include non-cognitive teacher quality measures from teacher evaluations or non-cognitive testing opportunities to generate a more comprehensive assessment of a teacher's ability or quality. However, only a cognitive measure, particularly only IQ, is available for women in these data.

that although teaching is more common for high-ability women and for women with young adult pill access, high-ability women with young adult pill access are *less* likely to teach than lower ability women with young adult pill access. Together, these results suggest that increasing the opportunity cost of teaching through providing access to the pill in young adulthood reduces the average quality of women in the profession.

Next, I develop an instrumental variables framework to examine the effect of relative pay on teacher quality using the variation in relative pay induced by young adult pill access. Because teacher and alternative professional work cannot occur for the same individual simultaneously, I estimate this model in two separate stages.⁹ In the first-stage, I predict teaching and alternative professional pay in a woman's primary occupation, using young adult pill access as the key explanatory variable to uncover exogenous sources of variation in relative pay. I allow the effect of pill access to differ by ability group. To estimate pay, I average the wages earned in the primary occupation observed throughout a woman's work career to capture information on the typical wage trajectory. Under the assumption that women with and without young adult pill access could anticipate differences in their wage trajectory, due to their pill access status, this variation should provide supply-side wage effects driven by differences in lifetime pay expectations. The results show that pill access in young adulthood increased the alternative professional pay of high-ability women by about 19 percent.¹⁰ By comparison, I find that pill access had almost no effect on teaching and pink-collar pay.

To estimate the second-stage, I construct relative pay using the predicted teaching and alternative pay estimates from the first-stage. I then estimate the effects of log relative pay by ability on the choice to teach. These results show that a 10 percent increase in relative pay raised the propensity of women in the rollout cohorts (entering the labor market between 1960 and 1975) to choose teaching by 5 percentage points.¹¹ This is a sizable effect given that approximately 45 percent of college-educated women taught in 1960, and relative teacher pay declined by roughly 12 percent between 1960 and 1990. The results also show the supply elasticity to teaching is approximately equal for high and low-ability women in the rollout cohorts.¹² This effect is due to the opposite directional effect that young adult pill access had on

⁹ This method provides consistent estimates of relative pay effects on the choice to teach (Murphy and Topel, 2002).

¹⁰ The estimate of the pill on alternative profession pays is not statistically significant and the F statistic on the instrument is lower than the traditional threshold of 10. However, I am in the process of obtaining more data which should increase the precision of this estimate, which has a large magnitude, as well as increase the F statistic.

¹¹ These results are fairly consistent with the magnitudes estimated in the previous literature (Bacolod, 2007).

¹² This is slightly smaller than previous estimates which show a 10 percent increase in alternative professional pay, holding constant teaching pay, decreases teaching propensity for the highest ability women by 6.4 percent as compared to by only 3.7 percent for lower ability women (Bacolod, 2007). However, differences between this and Bacolod (2007) may be due to differences in the method used to calculate relative pay. In Bacolod (2007), pay is

relative pay for high and low-ability women: the natural experiment lowered relative teacher pay for high-ability women and slightly increased relative teacher pay for lower ability women, who were able to enter the teaching profession in greater proportion upon obtaining access to the pill after previously remaining out of the formal labor market.

Taken together, these findings suggest that increasing the opportunity cost to teaching by increasing the returns to choosing an alternative profession for high-ability women lowers the propensity that high-ability women will enter the teaching profession, even controlling for working conditions. To the extent that changes in the opportunity cost to teaching are driven by changes in the effects of relative pay, these results suggest that pay matters and, therefore, that raising teacher pay to match the pay available to high-ability women in alternative professions may have been a useful means to mitigate the average declines in teacher quality experienced between 1960 and 2000. Moreover, negligible differences in the supply elasticity to teaching suggest that while pay matters, it does not matter significantly more for high-ability women. Thus, increasing teacher pay alone may have been insufficient to mitigating falling average teacher quality.

Background on Teacher Quality

Although the teaching profession was filled in the early 1900s with mostly upper middle class and relatively well educated women, and before that with mostly men (Strober and Lanford, 1986), teaching has increasingly become a second tier option among men and women of the highest ability (Corcoran et al 2004). Prior studies implicate three causes for observed declines in average teacher quality: changes in women's work preferences (Goldin, 2006), pay compression in teaching due to unionization (Hoxby and Leigh, 2004), and expanding alternative career opportunities for women (Bacolod, 2007; Loeb and Page, 2000). Much of the most contentious debate surrounds the estimates of declining relative pay and the implication that elevating teacher pay, alone, could raise average teacher quality.

Relative pay declined by about 12 percent between 1960 and 1990, with the steepest declines occurring over the earliest 20 years of this period (Loeb and Page, 2000). Between 1964 and 2000, the mean female teacher's standardized test score fell from 0.60 standard deviations above the mean female high school graduate to 0.46 standard deviations above the mean—a decline of 23 percent (Corcoran et al., 2004a). The largest teacher quality declines also occurred among women entering the labor market

estimated by averaging wages within occupation, without accounting for differences in occupational wages by ability. In this paper, pay is estimated within occupation and ability.

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during the earlier years of this period (Bacolod, 2007). Research to assess the strength of the link between these variables has investigated differences in the response to state-by-cohort changes in relative pay on teacher quality. Bacolod (2007) averages teaching and alternative professional pay¹³ using IPUMS 1970-1990 samples by cohort, race, gender, state and SMSA. She then merges this information with all series of the National Longitudinal Survey (NLS) samples using observations with the same characteristics.¹⁴ Bacolod's results show a 10 percent increase in alternative professional pay, holding constant teaching pay, decreases teaching propensity more for women in the top quartile of the standardized testing distribution—by 6.4 percent as compared to only by 3.7 percent for women in the 25-50th percentile of the standardized testing distribution.

The significance of estimates produced using such non-experimental evidence remain contentious. One concern is that the relative magnitude of teacher pay effects may be dwarfed by other policy relevant variables like pay compression due to unionization. One study finding such a result by Hoxby and Leigh (2004) decomposes relative pay into components associated with mean teacher pay, pay parity between men and women outside of teaching, and pay dispersion within teaching. Because pay dispersion may be endogenous, the authors implement an instrumental variables strategy using state-by-cohort variation in unionization laws, which increased in prevalence between 1960 and 1965.¹⁵ Their findings indicate that unionization accounts for 80 percent of the variation in declining teacher aptitude. The results, identified from changes in pay compression due to union pressure, provide strong evidence that pay compression reduced average teacher quality.¹⁶

An additional, perhaps larger concern with relative pay estimates is that their magnitude may be overstated due to endogeneity (Hanushek and Rivkin, 2006). One major source of endogeneity stems from the fact that teacher pay is likely increasing in non-pay “working condition” benefits to teaching such as the desirability of working at a particular school, the local housing quality, or neighborhood crime rates (Belfield, 2005; Hanushek, 1997; Hanushek and Rivkin, 2006). Moreover, if teacher pay is but a limited component of the compensation package received by women who choose to teach and teachers are a selected sample of women who receive disproportionate pleasure from working with children and other related working conditions associated with teaching, then nominal relative salaries will provide only

¹³ The alternative professions considered in Bacolod's analysis are limited to occupations offering higher pay on average, than teaching: accountants, engineers, college professors, doctors, health technicians, managers, officials, and proprietors, excluding nurses and teachers.

¹⁴ The benefit of this method is that the NLS samples include IQ in the initial NLS, used also in this paper, and AFQT in the 1979 and 1997 NLS surveys.

¹⁵ Their data cover women in the college graduating classes of 1961-1997, thus born between 1940 and 1976.

¹⁶ Teacher aptitude is defined by the selectivity of the college attended.

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a muted estimate of the true effect of relative pay on outcomes (Hanushek, 1997; Hanushek and Rivkin, 2006). Therefore, research that does not control for non-pay components of compensation cannot claim that higher teacher pay is causing improved overall teacher quality.

Loeb and Page (2000) use an instrumental variables research design to circumvent variation in teacher pay that is collinear with work conditions. The data are from 1960-1990 IPUMS cross-sections. The instrument—non-teaching female wages—is the same as the denominator of the endogenous variable. If teacher working conditions and non-teacher wages are uncorrelated, this instrument provides estimates that are unbiased with respect to working conditions. One potential concern with using alternative professional pay to provide exogenous variation in relative teacher pay, however, is that states and cohorts with higher teacher pay, and better working conditions, may also have higher than average non-teacher pay. The authors find that raising teacher wages by 10 percent reduces high school dropout rates by 3 to 4 percent. Under the assumption that higher quality teachers are more effective at lowering dropout rates, this evidence is consistent with the non-instrumented research (Bacolod, 2007) that shows higher relative pay improves teacher quality.

In summary, the prior literature provides evidence that average teacher quality is falling, at least in part, because of falling relative pay. The implication of such a result is that raising teacher pay could reverse this decline. The major limitation of this literature is the inability to identify relative pay in a manner that is uncorrelated with working conditions. Loeb and Page (2000) suggest alternative professional pay may yield the solution to this dilemma under the assumption that alternative professional pay is uncorrelated with teacher working conditions. However, there is some reason to believe that state-by-cohort differences in alternative professional pay may mimic state-by-cohort differences in teacher pay and working conditions. The instrument of young adult pill access may provide an opportunity to build on the Loeb and Page (2000) research design if the alternative professional wage variation in the pill rollout is unrelated to state-by-cohort differences in working conditions.

Background on the Pill

The first birth control pill, Enovid, was approved by the Food and Drug Administration as a contraceptive in 1960 after 3 years on the market as a treatment for menstrual disorders. By 1962, about 1.2 million married women in the U.S. were on the pill and by 1965 this number grew to 6.5 million (Myers, 2012). Legal access for unmarried, young adult women between ages 18 and 20, was provided in different years by state, between 1960 and 1975 (Bailey, 2006; Bailey et al., 2012). This state variation proved binding due to the legacy of the federal Comstock Act, which previously prohibited the

distribution of contraceptives across states and was continued in many states with "little Comstock" policies during the young adult pill rollout (Myers, 2012). The binding nature of the pill rollout is part of its attractiveness as an instrument in a growing literature.

The other advantage of using the pill rollout framework is that access in young adulthood for unmarried women was largely unrelated to state variation in socio-political sentiments about females in the workforce. This is because legal pill access was informed both by federal and state policies on oral contraceptive for married and unmarried adults as well as state variation in mature minor legislation (Hock, 2007). Variation in mature minor legislation was particularly volatile between 1960 and 1975 related to popular discontent that young men who were eligible to be drafted into the Vietnam War could not vote (Bailey et al., 2012; Hock, 2007).

The effect of young adult pill access laws on the age of fertility has been well established in the literature: among cohorts born before 1940, of whom none had young adult pill access, about 62 percent of women ever having children gave birth prior to age 22. On the other hand, for women born around 1955, nearly all of whom had legal pill access in young adulthood, only 25 percent of those ever having children gave birth prior to age 22 (Bailey, 2006). This powerful and quasi-random change in young adult pill access by state and cohort has been exploited in research demonstrating an effect of pill access in young adulthood on marriage (Goldin and Katz, 2002), labor supply (Bailey, 2006), education (Hock, 2007), choice of college major (Steingrimsdottir, 2010), and earnings (Bailey et al., 2012) among other longer term outcomes (Bailey, 2013). The research designs generally posit causal effects of the pill on these outcomes under the framework that "treatment" states, offering earlier cohorts of women pill access in young adulthood, were on average no different from "control" states—that failed to offer these early cohorts young adult pill access—with regard to variables that might bias estimates.

This paper treats the evidence from prior literature as indications that young adult pill access affected a woman's expectations of her lifetime pay.¹⁷ The principal claim of this investigation, moreover, is that this wage change was anticipated by the women receiving the pill in young adulthood and that this opportunity provided women with the means to pursue a pay trajectory in young adulthood that was previously less attainable. Moreover, I claim that the variation in wage change provided by young adult pill access provides a unique opportunity to circumvent the correlation between teacher pay and working conditions. In Figure 7, I show that pill access rollout is equivalent by IQ tercile group, race,

¹⁷ Evidence in support of this claim includes prior empirical work showing that delayed childbearing increases wages by 3 percent per year of delay (Miller, 2011) and, that for the pill rollout cohorts specifically, pill access elevated women's earnings by as much as 11 percent in a woman's early forties (Bailey et al., 2012).

region of birth and quarter of birth. This consistent rollout with respect to these pre-existing characteristics suggests, therefore, that access to the pill in young adulthood is potentially as good as randomly assigned.¹⁸ In Figure 8, I depict the state variation graphically, confirming no noticeable regional trends in pill access exist. Most importantly, this graph shows that regions with little state variation in teacher pay, non-teacher pay and working conditions obtain pill access in different years. This component of the rollout is critical for circumventing working conditions biases due to student behaviors or neighborhood factors which are largely regional in nature.

An Empirical Framework for Avoiding Working Conditions Biases

The ideal means of estimating the effect of relative pay on teacher quality involves randomly assigning individuals of different ability to different relative pay regimes, holding constant all other determinants of occupational choice. This is infeasible. A second best means of estimating the effect of relative pay on teacher quality involves regressing the choice to teach on state, cohort and ability group averages of wages in teaching and alternative professional occupations, holding constant all non-pay amenities associated with the occupation. This is expressed in model (1), below. In the model, teaching is indexed by t and non-teaching by n . State, cohort, and ability are indexed by s , c , and q . Individual is indexed by i . Non-pay attributes to teaching are captured by X and allowed to vary across state and cohort. The return to non-pay attributes is identified by δ , and allowed to vary with ability.

$$P(t_{iscq} = 1) = \beta(w_{scq}^t - w_{scq}^n) + \delta_q X_{sc} + d_s + d_c + d_q + \varepsilon_{iscq} \quad (1)$$

The non-pay amenities or working conditions, X , are the most challenging component of this model to measure. Yet, accounting for non-pay amenities is particularly important in an occupation like teaching where the non-pay attributes are considered to be disproportionately attractive as compared to alternative occupations (Belfield, 2005; Hanushek, 1997; Hanushek and Rivkin, 2006). Effectively controlling for non-pay attributes is particularly important if these attributes are correlated with state and cohort wage averages, such that high-ability women disproportionately select teaching in states that pay more, at least in part, because the working conditions in these states are improved. Therefore, a model that excludes controls for non-pay amenities, such as model (2) below, would incorrectly identify wage effects as the source of teacher choice. This would cause relative wage effects to be overstated if places that pay better also tend to provide better amenities.

$$P(t_{iscq} = 1) = \beta(w_{scq}^t - w_{scq}^n) + d_s + d_c + d_q + \varepsilon_{iscq} \quad (2)$$

¹⁸ I reproduce the full list of legal access dates, from Bailey (2012) in appendix B.

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One innovative solution provided in the literature, predicts relative teacher wages with non-teaching wages (Loeb and Page, 2000). Under the assumption that non-teaching wages are unrelated to teaching amenities, this is effective. However, if states that offer better non-teacher wages, on average, also offer better teaching wages and amenities, this could remain a biased estimator.

Pill access in young adulthood, if randomly assigned by state and cohort with respect to women's productivity potential and teacher working conditions, provides an opportunity to circumvent this bias using average changes in non-teacher pay by state, cohort and ability. In other words, if early pill adopting states are similar in observable *and unobservable* ways to late pill adopting states, but early pill adopting states have women who are more likely to delay pregnancy and to pursue wage trajectories that offer increased returns to their newfound earning potential, then the state-by-cohort variation in wages predicted by the pill rollout should be unrelated to state-by-cohort differences in non-pay working conditions. A model detailing the estimation strategy discussed above is shown in equations (3) and (4), below.

$$P(t_{iscq} = 1) = \beta_q(w_{scq}^t - w_{scq}^n) + d_s + d_c + d_q + \varepsilon_{iscq} \quad (3)$$

$$E[w_{scq}^t - w_{scq}^n] = \theta_q YAPA_{sc} + d_s + d_c + d_q + \vartheta_{scq} \quad (4)$$

The implications, building from prior research (Bailey et al., 2012; Hoxby and Leigh, 2004; Lazear and Rosen, 1990; Mincer, 1974; Steingrimsdottir, 2010), are that alternative professional lifetime average wages of high-ability women should increase due to pill access in young adulthood disproportionately as compared to teaching wages. On the other hand, low-ability alternative professional pay will likely fall as well since lower ability women will now be able to enter teaching and this distributional shift will reduce the average wages of low-ability women remaining in alternative professions (who now reflect a relatively lower skill mix as compared to the pre-rollout low-skill alternative work labor force). Therefore, relative teacher wages should fall with pill access in young adulthood for high-ability women and rise with pill access in young adulthood for low-ability women. Further construction of the estimators used to identify the effects of the opportunity cost (broadly speaking) and relative pay (narrowly speaking), isolated with young adult pill access, on the choice to teach are discussed in the research design.

Assessing whether young adult pill access affects occupational choice directly: the exclusion restriction

One potential limitation of using variation in the timing of pill access as an instrument for pay is that it adjusts pay expectations by adjusting the risk of fertility. In particular, if the pill rollout affected women's pill access at all ages, there would be a concern that the benefits of career amenities that are complementary to rearing children would be directly affected by the rollout. This complication would suggest the greatest utility of the model is derived from the reduced-form estimation of pill access on the choice to teach. Indeed, the reduced-form results provide meaningful information. The reduced-form estimates, like the instrumental variables results, demonstrate the opportunity cost to teaching affects the choice to teach.

In addition, there is evidence to support the claim that the pill access rollout can be used to identify exogenous variation in relative wages. This follows from the age-specific nature of the rollout. Because women who were over the age of 20, married or who had already conceived were able to obtain legal birth control pill access in 1960, the pill access rollout affected the pill-taking behavior of only unmarried women in young adulthood (Bailey et al., 2012). Importantly, women in young adulthood were not yet engaged in professional work. Therefore, in order for the young adult pill rollout to provide non-pay benefits to a woman's occupational choice behavior in young adulthood, women would need to anticipate the non-pay benefits of their occupational work during their pre-work ages differently with the pill in young adulthood than they did without the pill in young adulthood. The principal reason this might occur is if access to the pill in young adulthood changed the number of children that women demanded.¹⁹ That is, if the pill encouraged women to have fewer children, then certainly one would expect women to reduce their anticipatory desire for child-friendly work amenities. However, life-cycle models of marriage and fertility behavior suggest that the power of the pill is derived from its ability to empower young women to disentangle sex and unplanned pregnancy (Bailey, 2006; Goldin and Katz, 2002), and yet, not change their lifetime demand for the number of children desired (Bailey, 2006). Therefore, a pregnancy in young adulthood that occurred without legal pill access during this period would just result in fewer pregnancies in later life, rather than a change in the number of children born. This line of

¹⁹ Empirical research on fertility and occupational choice bears out the weak relationship between fertility timing and occupational choice. Shin and Moon (2006) find the presence of a newborn child has insignificant effects on occupational choice, although negative effects on labor force participation using the same cohorts and data used in this analysis. Stinebrickner (2002), using more recent data, shows that while childbearing is the strongest predictor that teachers leave the occupation, it is also strongly predictive of women leaving the labor-force across occupations. As such, there is no evidence that a woman's taste for teaching changes subject to bearing children.

reasoning suggests the non-pay benefits to teaching should remain unaffected by access to the pill at ages 18-20.²⁰

Integrated Public Use Microdata Series (IPUMS) Data: Trends in declining relative pay, 1960-2000

The IPUMS, 1960-2000 samples, which cover nearly the full work career of women in the pill rollout cohorts, confirm the positive cross-sectional relationship between teacher quality and relative teacher wage established in the prior literature (Bacolod, 2007; Loeb and Page, 2000). Figure 2 shows that women's teaching wages fell relative to alternative professional wages²¹ for women with at least an Associate's degree between 1960 and 2000. This decline appears largely constant across the period, but accelerates between 1990 and 2000 at which point alternative professional wages actually exceed teaching wages for the first time in history. Figure 3 shows average wages across state, race, sex and year as compared to teaching work propensities by these same cells. The figure reveals declines in relative teacher wage for every subgroup between 1960 and 1990 with the most substantial declines shown for black women. The figure also shows black women experienced the largest decline in teaching propensity over the period as well. By comparison, white men experienced the smallest changes in relative pay and the propensity to teach.²²

These trends provide preliminary evidence that relative pay may have contributed to the significant decline in average teacher quality over the past several decades. More robust evidence for the effects of pay on teacher quality can be gleaned by exploring changes in the lifetime work behaviors due to exogenous changes in the opportunity cost to teaching, such as legal access to the pill in young adulthood provides. The pill rollout cohorts, turning 18-21 between 1960 and 1977 and entering retirement ages between 1999 and 2019, represent approximately a third of the workforce throughout the 1960-2000 teacher quality decline period.

²⁰ In Appendix F I examine of the effects of a negative relationship between young adult pill access and the benefit of teacher working conditions. The results show that this relationship should upwardly bias the estimator of relative pay on the choice to teach. In light of the results that show pay positively influences the choice to teach but not significantly more for higher ability women, the potential for the estimates to be overstated seems unlikely.

²¹ Alternative professional wages are defined as all wages for women with at least an Associate's degree who are not working in teaching or other pink-collar (high female ratio) professions.

²² Figure 3 shows the trends in non-teaching, pink-collar occupations with high female-ratios and alternative professional occupations, not including pink-collar occupations. These figures do not show similar declines in work propensity from 1960 to 2000. In fact, black women, the sub-group experiencing the greatest decrease in teaching propensity, show increases in their propensity to work in alternative and pink-collar occupations. Pink-collar occupations show the least change in work propensity for any race across the period.

National Longitudinal Survey of Young Women (NLS) Data: Estimating the effects of alternative professional pay on the choice to teach, cohorts entering the labor market from 1964-1975

To explore the extent to which the association between wage opportunities and work propensity is reflective of a causal relationship, I use the National Longitudinal Survey of Young Women (NLS) whose respondents were entering the workforce between 1964 and 1975, years fully encompassed by the 1960-1975 pill rollout. The sample includes 2,570 respondents, followed for nearly 30 years from 1968 until 1997 in 19 subsequent semi-regular interviews. The women were aged 14 - 24 during the initial survey and thus born between 1943 and 1954 and turning 21 between 1964 and 1975.

Each woman has at least some college education, defined as at least one year of postsecondary training. Although I have chosen a sample of only college-educated women, as women considering college are the most likely to be choosing between teaching and alternative professions, the results presented in this paper are entirely consistent when they are unrestricted to those with only some college. The sample is restricted to women who are either black or white because women identified otherwise were not available in large samples and the occupational behavior of women born between 1943 and 1954 was very different by race and ethnicity (Bacolod, 2007).

One of the primary benefits of this sample is information on IQ—a measure of ability—which is assessed once in 1968. While cognitive achievement yields the most salient connection to student gain scores overtime amongst teacher inputs available to the secondary researcher (Rivkin et al., 2005), an ideal research design would incorporate non-cognitive teacher quality measures to provide a more comprehensive assessment of a teacher's ability. Unfortunately, these data include only IQ as a means to identify ability. Nonetheless, cognitive achievement is weakly correlated with teacher ability and strongly correlated with a woman's productivity on the non-teaching market (Hunter, 1986; Rivkin et al., 2005). Moreover, this method of estimating ability using a cognitive measure is consistent with prior work (Bacolod, 2007) in part because other observable measures of teacher characteristics are noisy indicators of quality, at best (Rivkin et al., 2005).

High-ability women are defined as women who scored in the top tercile, nationally, on their respective IQ test. This represents a bit more than half of the college-educated NLS sample. Low-ability women, by comparison, are defined as women who scored in the bottom two terciles. One shortcoming of this data is that IQ is available for only about 70 percent of respondents, reducing the effective sample size to 1,847.

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Geographical information is not publically available in the NLS data but is implicitly derived using a crosswalk between the Census, 1969 and NLS, 1969. This implicit geographical information is available for about 85 percent of the respondents.²³ As a result, the main regression models use a sample that includes information on 1,623 respondents of which 1,328 were white and 295 were black.²⁴

Occupational choice is measured over time. I define a primary occupation as the occupation observed most often over an individual's career. I then construct four mutually exclusive categories: teacher, two non-teaching alternative professions, and one remaining category for women who remain out of the formal labor market for the majority of their career. The teacher designation is assigned to survey respondents who identified themselves as a prekindergarten, kindergarten, elementary or high school teacher in addition to school counselor, school administrator, teacher's aide or special education teacher.²⁵ The first non-teaching, alternative professional category includes college-educated women in occupations with high-female ratios, so-called pink-collar professions. This category includes clerical workers, librarians, nurses, health aides, cosmetologists, household workers and seamstresses. The professional alternatives to teaching and pink-collar work incorporate all other work, including accountants and bookkeepers, architects, engineers, doctors, lawyers and judges as well as photographers, managers and supervisors, legal assistants, decorators and designers, retail workers, inspectors, farmers, editors, bankers, and women working in sales.

The alternative occupations have greater pay variation with a more substantial right skewness than is available in teaching but also have a lower modal wage, as shown in Figure 5. Figure 6 shows that the return to ability is greater in alternative professional work than in teaching, consistent with the

²³ The geographical information is based on a public sampling algorithm that matches information between the NLS, 1969 survey questions relevant to state of residence and the Census, 1969. In the algorithm, three variables from the NLS are used to create the link: an index of the demand for female labor in the area of residence, the Census division of residence, and educational expenditures per pupil in a labor market. Implicit state is calculated by matching information from these variables to state level data from the 1969 Census. The crosswalk was acquired from Martha Bailey who received it from Alessandra Voena and Elizabeth Peters. These latter authors thank Jeff Gray in their respective articles, Voena (2012) and Peters (2008), for developing the algorithm to match state of residence between the NLS, 1969 survey and the Census, 1969. This implicit state information is provided for about 85 percent of the data. Observations are missing when the matching was unable to be performed, due to limitations of the crosswalk, or individuals were missing from the survey year. The implicit state therefore reduces the sample size and has the potential to introduce measurement error, if the crosswalk is not completely accurate. To rectify this I am in the process of obtaining full state of residence from the Census Bureau. The application is currently under review by the Federal Census Bureau and should be approved by May of 2014.

²⁴ This limited sample size is likely the greatest limitation of this paper. To mollify this concern, I am in the process of obtaining full state of residence from the Census Bureau. The application is currently under review by the Federal Census Bureau and should be approved by May of 2014.

²⁵ The results are robust to excluding school administrators and school counselors. They were included only because the classifications are sufficiently overlapping that it is occasionally challenging to determine in which school-related capacity an individual works.

aforementioned statistical framework. Low-IQ teachers, however, have a greater modal wage than low-IQ alternative professional workers. I also compile a category for primarily out of the labor force to present mutually exclusive and collectively exhaustive options.

Although nearly one-third of the sample were teaching at some point between 1968 and 1997, only 13 percent were primarily teachers. About 43 percent worked primarily as alternative professionals and about 36 percent were pink-collar. Only 7 percent were mostly out of the labor force. One-third of the sample had young adult pill access and the average date of state acquisition was 1971. The state-by-cohort estimates of date of legal pill access were adopted from data available in Bailey et al (2012) and are reprinted in Appendix B. More detailed information on the data construction is available in Appendix A.

Econometric Model

The identifying assumption of the research design is that state-by-cohort average changes in women's behavior due to young adult pill access are random with respect to working conditions. If women in states that offer young adult pill access to early cohorts are more likely to choose alternative professional occupations because their reduced risk of unwanted or ill-timed pregnancy in young adulthood increases their early career investment behaviors, then analyses of the work behaviors of these women should tell us about the effects of changing the opportunity cost of teaching, broadly speaking, and the relative pay, narrowly speaking, on occupational choice, unrelated to differences in teacher working conditions.

Reduced-Form Estimator

The first set of regressions examine the reduced-form effect of young adult pill access on teacher quality. The reduced-form estimator is defined in equation (5). State, birth cohort, and race fixed effects—indexed by s , c , and r —are included to account for persistent differences between states, cohorts, and racial groups. Ability is defined by whether a woman has a top-IQ and is indexed by q . These reduced-form estimates provide evidence on the effects of increasing the opportunity cost to teaching for higher ability women on the average quality of women in the teaching profession.

$$\Pr(\text{Teacher}_{qsc} = 1) = q_q * YAPA_{qcs}\theta + d_q + d_s + d_c + d_r + e_{scqr} \quad (5)$$

Instrumental Variables Estimator

The instrumental variables estimator builds on this reduced-form framework by isolating potentially exogenous relative wage measures and estimating their effect on a woman's propensity to teach, by ability. Because no women can be simultaneously observed in both teaching and alternative professions, I estimate these models separately by occupation. This effectively generates two first-stage models in which I predict teacher and non-teaching professional pay using young adult pill access as well as state, birth year, race and ability group fixed effects as shown below,

$$\ln(\text{teacher pay})_{iqcs} = q_q * YAPA_{qcs}\gamma_1 + d_q + d_s + d_c + d_r + \zeta_{iscqr} \quad (6)$$

$$\ln(\text{professional pay})_{iqcs} = q_q * YAPA_{qcs}\gamma_2 + d_q + d_s + d_c + d_r + \xi_{iscqr} \quad (7)$$

where i indexes the individual, q is an indicator for top IQ tercile or not, c is birth cohort, r is white or black racial group and s is state of residence in 1968. Consistent with the reduced-form estimator, the state and cohort fixed effects help address concerns that the estimates are driven by national legislation that influences persistent differences in pay between states or cohorts such as the 1965 Equal Pay Act or improved anti-discrimination enforcement that occurred throughout the 1970s, 80s and 90s. The standard errors are clustered at the state of residence.

Next, I construct relative pay by dividing the teacher pay predictions by alternative pay predictions. In the second-stage, I estimate the effect of occupational choice on this derived relative pay. This second-stage specification provides consistent relative pay effects (Murphy and Topel, 2002), enabling an instrumental variables estimate where the endogenous regressor—relative teacher wage or alternative professional wage—cannot be directly observed, and rather, is constructed in a first-stage prediction. The benefit of this model over a more traditional construction of pay that averages pay by state, cohort, ability, and race is that it allows a specification that fits the entire available data, thus not dropping observations without both a teaching and non-teaching observation of the same state, cohort, ability, and race. Given the paucity of observations in the NLS (or in other sources that combine information on ability and lifetime wage for women who enter the labor market between 1960 and 1975) and the requirement of estimating changes in behavior between 50 states, 10 birth years, 2 races and 2 ability groups, this two-stage estimation procedure is a requirement for performing the analysis.²⁶

²⁶ To increase the sample from 1,623 to approximately 2000 observations, I am currently in the process of obtaining the Census Bureau's restricted access data.

The wage information used to estimate the model is from a constructed measure of hourly wage, based on the typical weekly, monthly, annual income and hours worked. To mitigate imbalance in the panel and reduce the potential to conflate birth cohort and life-cycle effects, I restrict analyses of wage information for women during ages 24-43, as these are the ages for which all observations are observed. Further information on the construction of this variable is available in the data appendix and follows the usual labor market variable conventions for survey data.

Equation (8) is estimated using a linear probability model of the likelihood of choosing a teaching profession, where predicted relative pay is derived from equations (6) and (7).

$$\Pr(\text{Teacher}_{iqsc} = 1) = \beta_q^1 \ln\left(\frac{\widehat{\text{teacher pay}}}{\widehat{\text{professional pay}}}\right)_{iqcs} + d_q + d_s + d_c + d_r + v_{iscqr} \quad (8)$$

$$\Pr(\text{Teacher}_{iqsc} = 1) = \beta_q^2 \ln(\widehat{\text{professional pay}})_{iqcs} + d_q + d_s + d_c + d_r + u_{iscqr} \quad (9)$$

As above, i indexes the individual, q is an indicator for top IQ tercile or not, c is birth cohort, r is racial group and s is state of residence in 1968. To account for the two-step estimation, I bootstrap the standard errors for this second-stage. Both models again include the same fixed effects as the first-stage and reduced-form models to absorb persistent differences in pay between states, cohorts or racial groups that undoubtedly affected relative pay.

Reduced-Form Results

Graphical Results

Figures 9 and 10 graphically produce estimates for the reduced form estimates of young adult pill access on occupational choice described by model (5). These graphical results foreshadow the estimation findings.²⁷ Specifically, the left panel of Figure 9 shows that top-IQ women *without* young adult pill access are *more* likely to take up a career in teaching while top-IQ women *with* young adult pill access are *less* likely to choose a teaching career. The right panel of Figure 9 shows that women *without* young adult pill access are about equally likely to take up an alternative profession regardless of their IQ. Conversely, the right panel of Figure 9 shows that women *with* young adult pill access are *more* likely to

²⁷ Figures 9 through 12 also confirm that the behavior of women in the first and second terciles are fairly consistent and, conversely, that the behavior of women in the top tercile differ. When the results are broken down by quintile, women in the top two quintiles appear more similar than women in the bottom 3. This evidence suggests that the definition of high-ability as top tercile is adequate for understanding differences in occupational behavior and wage trajectory by IQ.

take up an alternative occupation if they are at the top of the IQ distribution than women without pill access in young adulthood or lower IQ women. Since low-IQ women *with* young adult pill access were no more likely to teach upon obtaining access to the pill, Figure 10 explains where the increase in low-IQ women with the pill entering the teaching profession came from: women *with* young adult pill access are *less* likely to spend the majority of their career out of the formal labor force at lower points in the IQ distribution. Women *with* and *without* the pill in young adulthood show no significant changes in their propensity to take up pink-collar work.

Regression Results

Table 2 presents the reduced-form estimates of the effects of young adult pill access on occupational choice by ability described by model (5). These results indicate that young adult pill access significantly reduced the probability that a high-ability woman chose to teach. Column 1 shows that pill access increases the propensity that women, regardless of IQ, will choose a career in teaching by 9 percentage points and that a high-IQ, regardless of pill access, increases the propensity that a woman will teach by 9 percentage points. However, the interaction effects of high-IQ and pill access lowers a woman's teaching propensity by 15 percentage points. For women without young adult pill access, high-IQ women are 7 percentage points more likely to choose a career in teaching. In a sample where 13 percent of women teach, this suggests that pill access has a noteworthy effect on women's occupational choice behavior.

Column 2 shows that young adult pill access also has a noteworthy effect on alternative professional work behavior. Table 1 shows that, more generally, 43 percent of the sample selected alternative professions. Without young adult pill access, high-IQ women and low-IQ women were approximately equally likely to choose alternative professions. However, with young adult pill access, high-IQ women were about 8 percentage points *more* likely to choose alternative professions. Although this result is not statistically significant, it represents the second largest change in magnitude (after the change in the propensity to teach) among high-IQ women with young adult pill access.

Pink-collar work and remaining mostly out of the formal labor market are the two other mutually exclusive and collectively exhaustive categories of lifetime occupational behavior in this framework. Column 3 shows that pink-collar work is largely unaffected by young adult pill access, regardless of IQ. Column 4 shows that low-IQ women with young adult pill access are now significantly less likely to

remain out of the labor force, presumably because they are now more likely to enter the teaching profession according to the empirical and graphical trends of Table 2 and Figure 10.²⁸

The reduced-form results stratified by race—presented in Appendix E—are useful for understanding the extent to which the fixed effects are effectively controlling for differences between races as the change in the opportunity cost to teaching induced by access to the pill should be effectively equal by race. That is, there is no reason to believe that the change in the opportunity cost to teaching induced by access to the pill, alone, should differ substantially by race. The results bear this out: black women display a statistically equal propensity at the top of the ability distribution to exit teaching as compared to white women.²⁹

In culmination, these reduced-form results show that a lack of control over fertility was a significant barrier for women's pursuit of work and that upon the removal of this constraint, women pursued work that was more pleasing to them. As such, the reduced-form results suggest that in order for the teaching profession to retain high-ability women, the occupation must compete for high-ability labor by providing the contemporary attributes of alternative professions.

To examine the mechanisms by which access to the pill in young adulthood influences occupational choice, I next describe results of the effects of the pill on age of first birth, age of first marriage and years of education. Each of these mechanisms suggests a link between access to the pill in young adulthood and pay in alternative professions for women of the highest ability (Card, 1999; Goldin, 2006; Lazear and Rosen, 1990; Miller, 2011; Mincer, 1974). In the final component of the results section, I examine the effects of relative pay, isolated by variation in pill access in young adulthood, on the choice to teach using an instrumental variables estimator.

Mechanisms

To assess the mechanisms by which young adult pill access influences work behavior, I examine the effect of legal access to the pill young adulthood on age of first birth, first marriage and years of education differently by occupation and ability. Prior research suggests that young adult pill access should increase wages by delaying the average age of childbearing and age of first marriage (Bailey,

²⁸ This effect is reflected in the point estimates by showing young adult pill access increases a high-IQ woman's propensity to remain out of the labor force by 6 percentage points as compared to a low-IQ woman.

²⁹ Although the magnitude of the estimate on the interaction of pill access and high-IQ for black women is greater than for white women, the standard error for black women is quite large, such that the confidence interval includes the magnitude of the estimate for white women.

2006; Goldin and Katz, 2002) which, in turn, should enable women to invest more in their careers during ages which otherwise may have been dedicated more exclusively to rearing children (Hock, 2007; Steingrimsdottir, 2010). Moreover, theory suggests that young adult pill access should differentially encourage high-ability women to enter alternative professions and low-ability women to enter teaching, following a traditional returns-to-ability framework (Mincer, 1974). Consistent with theory, the results show that young adult pill access increased age of first birth, first marriage, and years of education disproportionately for high-IQ women in alternate professions. Below I describe these results in greater detail.

Age of first birth

Figure C1, from Appendix C, shows that access to the pill in young adulthood increased the age of first birth for high-ability women working in alternative professions more than the age of first birth of teachers, with or without the pill. By contrast, the pill delayed childbearing disproportionately for middle and lower IQ tercile women in teaching. Table C1 displays this result empirically, showing that young adult pill access increased the age of first birth of high-IQ alternative professionals by about 2 years. Although this result is not statistically significant, it is opposite in direction and greater in magnitude than the negative one-half year that the pill effects the age of first childbearing for high-IQ teachers.

Age of first marriage

The graphical and regression estimates for age of first marriage largely mirror the results discussed above for age of first birth. Young adult pill access for high-IQ women increased the age of first marriage for women in alternate professions. It also increased the age of first marriage for middle-IQ women, as shown in Figure C2. The effect of young adult pill access on the age of first marriage of teachers is negligible.

Years of education

The average wage return to each additional year of education is about 10 percent (Card, 1999). Therefore, if the pill increased the ability of women to stay in school longer, this alone should have increased their lifetime wage accumulation. Figure C3 shows the effects of young adult pill access on the years of education of high-IQ alternative professionals. High-IQ women with young adult pill access obtained an average of about an additional year of education in alternative professions. The differential effect of the pill on the educational attainment of high-IQ women in alternative professions is consistent with the framework presented: high-ability women with young adult pill access differentially increased

their schooling as compared to low-ability alternative professional workers or teachers and pink-collar workers, without regard to ability. The pill had negligible effects on the schooling of teachers. This is probably due to wide spread mandates that teachers obtain post-baccalaureate schooling, which exceeds the average schooling of the population.

Instrumental Variables Results

Graphical Results

Figure 11 confirms that the wage effects of occupational choice are consistent with the statistical framework. Top-IQ women experience significant financial gains to their choice to pursue alternative occupations with the advent of the birth control pill in young adulthood. Low-IQ women experienced modest financial losses, on average, if they remained in alternative occupations with the advent of the birth control pill in young adulthood. Wages by IQ for teachers were relatively unaffected by pill access in young adulthood. Specifically, there were small declines in average wages among women with young adult pill access but they did not differ substantially by IQ. Because average wages for teachers in later birth cohorts should have been lower, due to the falling real wages of teachers, the fact that women with the pill have lower wages is potentially a product of more women with the pill being born in later cohorts. Also, Figure 12 shows that wages of pink-collar workers are effectively unchanged by access to the pill in young adulthood.

The first-stage: the effect of pill access on wages

The estimates of young adult pill access on pay, shown in table 3, estimate equations (6) and (7). The results in column 1 show that pill access increased alternative professional wages for high-ability women by an average 19 percent between ages 24 and 43. This result is not statistically significant but the magnitude of the point estimate remains noteworthy considering the wage effects of pill access in young adulthood are negligible for high-ability women who chose to teach or take up a pink-collar occupation, by comparison.

The F-statistic on the effect of young adult pill access on the average lifetime wages of women in alternative professions is 3.3, greater than the F-statistics on the effect of young adult pill access on the average lifetime wages of women in teaching or pink-collar occupations which were 0.18 and 1.12 respectively. The limited sample size is likely a contributing factor to the modest F-statistic on the

instrument for alternative professions as well as the lack of statistical significance for the effect of young adult pill access on top ability alternative professional wages.³⁰

The second-stage: the effect of relative pay on teaching

The main results of the paper are derived from the second-stage, defined in equations (8) and (9) as the effects of relative pay or simply alternative professional pay on the choice to teach. I also estimate differences in the supply elasticity to teaching by ability. The results are produced in table 4. Column 1 shows a 10 percent increase in relative pay increased the propensity of a woman to choose teaching by 5 percentage points. Column 3 shows a 10 percent increase in alternative professional pay decreases teaching propensity by nearly 7 percentage points. These estimates are largely consistent with the prior literature (Bacolod, 2007) and suggest that women, regardless of ability, are responsive to pay. That is, one's propensity to take up a career in teaching is reduced when her pay opportunities outside of teaching are greater. Under the assumption that the changes in pay, derived from the young adult pill rollout, are quasi-random with respect to state-by-cohort differences in teacher working conditions, this implies that women from the rollout cohorts were responsive to wage differences, even controlling for working conditions.

To ascertain the extent to which relative pay effects differ by ability, I estimate differences in the effect of relative pay for high-ability women, over and above the effects on low-ability women. These results are presented in columns (2) and (4) for relative teacher pay and alternative professional pay, respectively. The estimates show that a 10 percent increase in relative pay increased the propensity of a high-ability women to choose teaching by only 0.6 percentage points more than a lower ability woman. This effect is not statistically significant. Similarly, a 10 percent increase in alternative professional pay decreased the propensity of a high-ability woman to choose teaching by about 1.3 percentage points more than a lower ability woman. This effect is also not statistically distinguishable from zero. These supply elasticities by ability are smaller than the estimates in the previous literature.³¹ However, the research design used in this paper, different from the prior work, exploits changes in occupational pay by ability rather than changes in occupational pay, unrelated to ability. Moreover, the effect of young adult pill access on wages increased alternative professional pay for high-ability women and slightly lowered alternative professional pay for lower ability women. Therefore, while these results show that pay

³⁰ Access to the full unrestricted data sample is anticipated by May, 2014. This will boost the sample size by about 15 percent. This will likely increase the F-statistic on the instrument as well, considering the magnitude of the point estimates.

³¹ Although the estimates in this paper are not statistically significant, their standard errors do not suggest that their point estimates should be consistent with the prior work.

differences are almost equally attractive to high and low-ability women, in a framework where higher ability teachers are preferred and distinguishable from lower ability teachers, these results suggest better pay could have induced better quality women to remain in teaching.

Policy Implications

Teacher quality fell precipitously between 1960 and 2000 (Corcoran et al., 2004a; Corcoran et al., 2004b). Previous findings implicate three primary causes for this downturn. First, teacher unionization increased substantially between 1960 and 1965 and was relatively flat thereafter, compressing teacher pay by ability (Flyer and Rosen, 1997; Hoxby and Leigh, 2004). Second, expanding female labor market opportunities throughout the 1970s and 1980s lowered teacher pay relative to the increasing pool of alternative professional opportunities (Bacolod, 2007; Loeb and Page, 2000). Third, women's work preferences changed, first gradually in the 1960s and then more dramatically as women increasingly began to identify with their work as a career, rather than simply a family's secondary income source, and to arrange their family life in conjunction with their career aspirations (Goldin, 2006).

The relative roles of each of these causes is disputed in the current literature. Concern over confounding factors elicits debate over the magnitude of relative pay estimates, in particular (Hanushek, 1997; Hanushek and Rivkin, 2006), despite cross-sectional evidence of a positive correlation (Bacolod, 2007; Loeb and Page, 2000). One of the principal endogeneity concerns is produced by teaching's non-pay attributes which may overstate the effect of pay on teacher quality if not controlled.

In this paper, I use the young adult pill rollout which significantly elevated alternative professional lifetime pay for high-ability women entering the labor force between 1960 and 1975. This rollout provides a unique opportunity to explore changes in women's wage expectations due to a quasi-random state-by-cohort change in women's lifetime pay expectations by occupation and ability under the assumption that supply-side changes in women's pay expectations induced by access to the pill in young adulthood are uncorrelated with geographic and cohort differences in teacher working conditions.

The first estimates produced in this paper explore the reduced-form effects of the pill's change in the opportunity cost to teaching on the average quality of women in the teaching profession. The results find that raising the opportunity cost to teaching by offering access to the pill in young adulthood indeed reduced the average quality of U.S. female teachers. The implications of this suggest shrinking the opportunity gap between teaching and alternative professions for high-ability individuals as a means to mitigate ongoing declines in teacher quality.

One principal component of this opportunity gap between teaching and the alternative professions is due to differences in pay. College-educated alternative professionals have experienced greater real earnings growth than teachers since 1960, as shown in Figure 2, and incurred a greater return to ability, as shown in Figure 6. The instrumental variables strategy presented in this paper provides estimates of the effects of relative pay and alternative professional pay on teacher quality, unrelated to teacher working conditions.

The instrumental variables results indicate relative pay affects teacher quality, even controlling for working conditions. Additionally, given the argument by economic historian Claudia Goldin that "changing constraints, more so than changing tastes... served to alter the decisions of college women [over the past century] with regard to family and career (1997, pg 25)," the evidence presented in the current work suggests increasing teacher pay may be a useful lever to mitigate the ongoing declines in average teacher quality. Further, negligible differences in the relative pay effects of young adult pill access by ability and occupation reveal that both high and low-ability women are attracted to teaching profession about equally for each additional dollar. Therefore, to effectively apply the lessons of the young adult pill rollout to future policy, one must consider pay schemes that discriminate by ability. Debate over the efficacy of pay-for-performance, for example, continues in the literature (Dolton and Marcenaro-Gutierrez, 2011; Springer et al., 2011). However, under the assumption that principals are able to effectively distinguish between high and low-quality teachers (Jacob and Lefgren, 2008), the natural experiment presented in this analysis presents an optimistic lens for the capacity of such policies to both attract and retain able teachers.

Conclusions

Schools offer one of the most politically palatable means for equalizing the economic and social opportunity available to children (Oreopoulos, 2007). Moreover, the quality of school teachers provides the strongest school-related predictor of student achievement (Rivkin, Hanushek and Kain 2005; Clotfelter, Ladd and Vigdor 2007; Chetty et al 2011). Therefore, improving our understanding of the underlying factors that have led to declining teacher quality is of first order policy significance.

This paper uses nationally-representative samples of women born between 1943 and 1954 from the National Longitudinal Survey (NLS) to analyze labor market outcomes of cohorts that straddle the period of the pill rollout and the subsequent 30 years during which the average ability of women entering the teaching profession declined. The research design first explores the effect of young adult pill access on the choice to teach to examine the effects of increasing the opportunity cost to teaching for higher

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ability women on the average quality of women in the teaching profession. The results show that increasing the opportunity cost of teaching through providing access to the pill in young adulthood reduces the average quality of women in the profession.

In addition, the research design identifies the effect of relative lifetime wage in teaching as compared to an alternative professional occupation on the propensity to choose a teaching career, by ability group, using changes in occupation-specific pay trajectories induced by young adult pill access. This instrumental variables estimator successfully identifies an unbiased estimate of the effect of relative pay on occupational choice, by ability, under the assumption that the state-by-cohort pill rollout is uncorrelated with respect to state-by-cohort differences in working conditions. These results show that, for women entering the labor force between 1960 and 1975, a 10 percent increase in relative pay increased the teaching likelihood by 5 percentage points. This is a sizable effect given that approximately 45 percent of college-educated women taught in 1960, and relative teacher pay declined by roughly 12 percent between 1960 and 1990. The interaction effects of relative pay and ability show differences in the labor supply elasticity to teaching by ability are negligible. However, because pill access differentially elevated alternative professional pay for women of the highest ability, the results suggest the opportunity cost to teaching bears a significant effect on the choice to teach, and under the assumption that principals are able to effectively distinguish between high and low-quality teachers, elevating teacher pay may be an important consideration for improving average teacher quality in the future.

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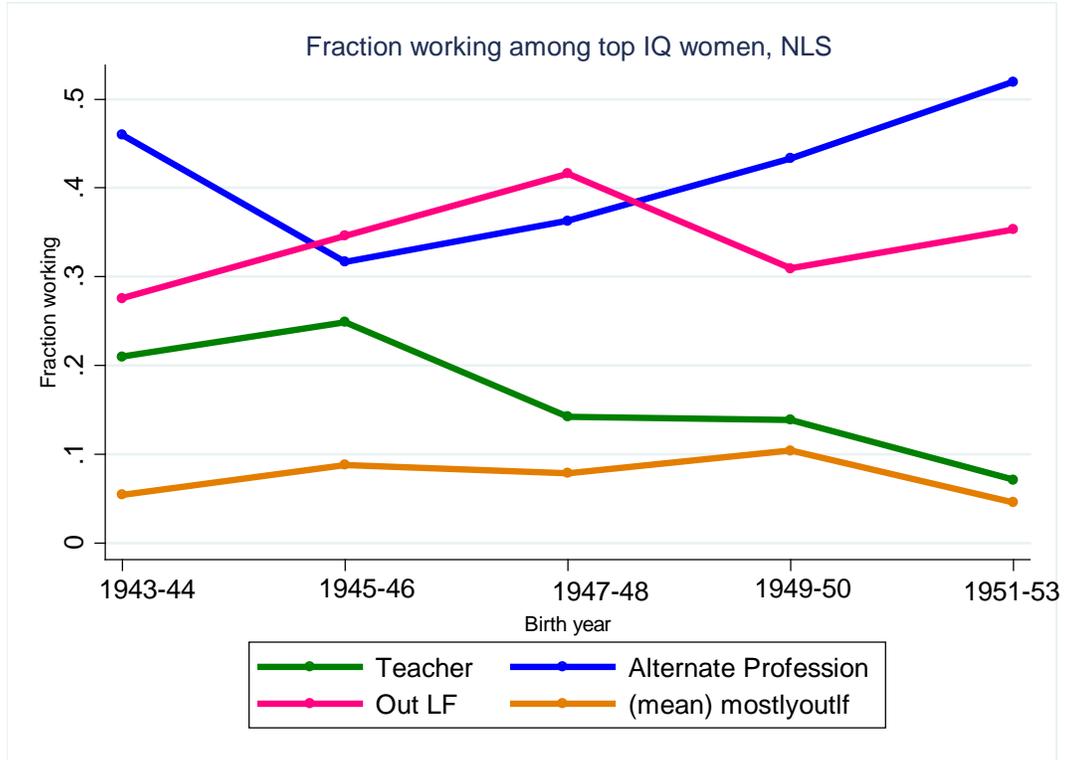
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Figure 1



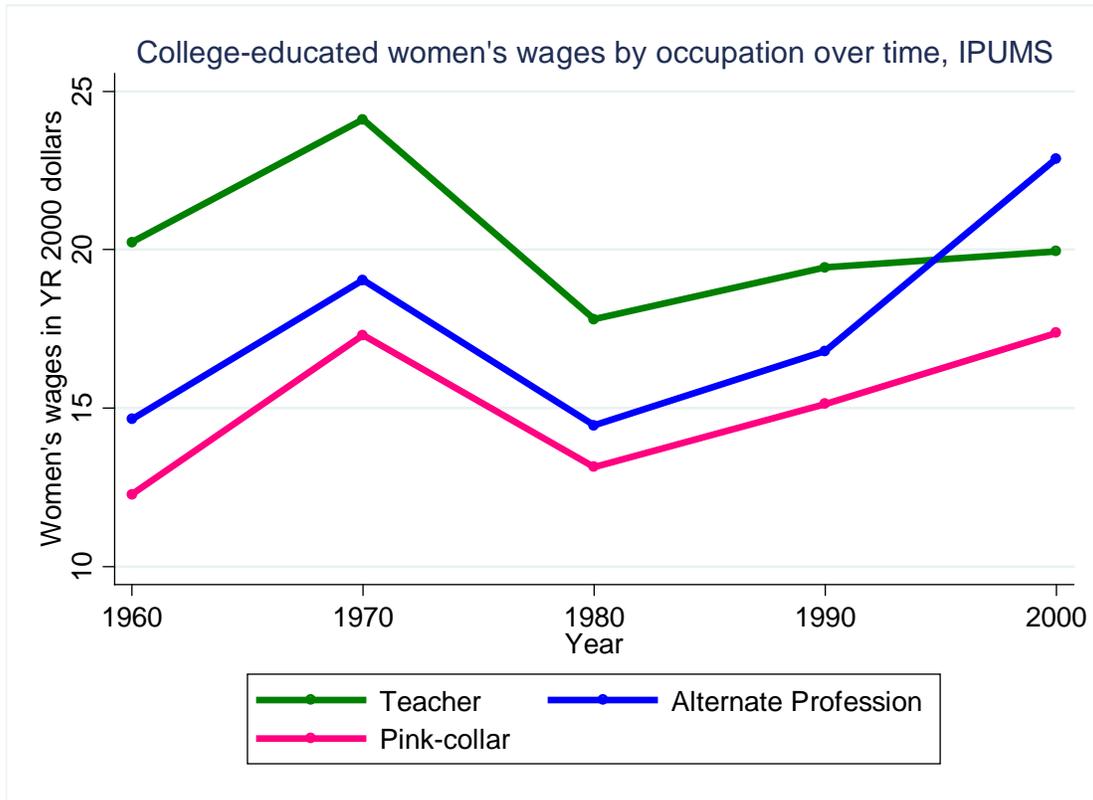
Source: National Longitudinal Survey, 1968-1997 samples. More detail provided in the data appendix on the construction of primary professional wages.

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Figure 2

Teacher wages fell relative to alternative professional wages, 1960 to 2000



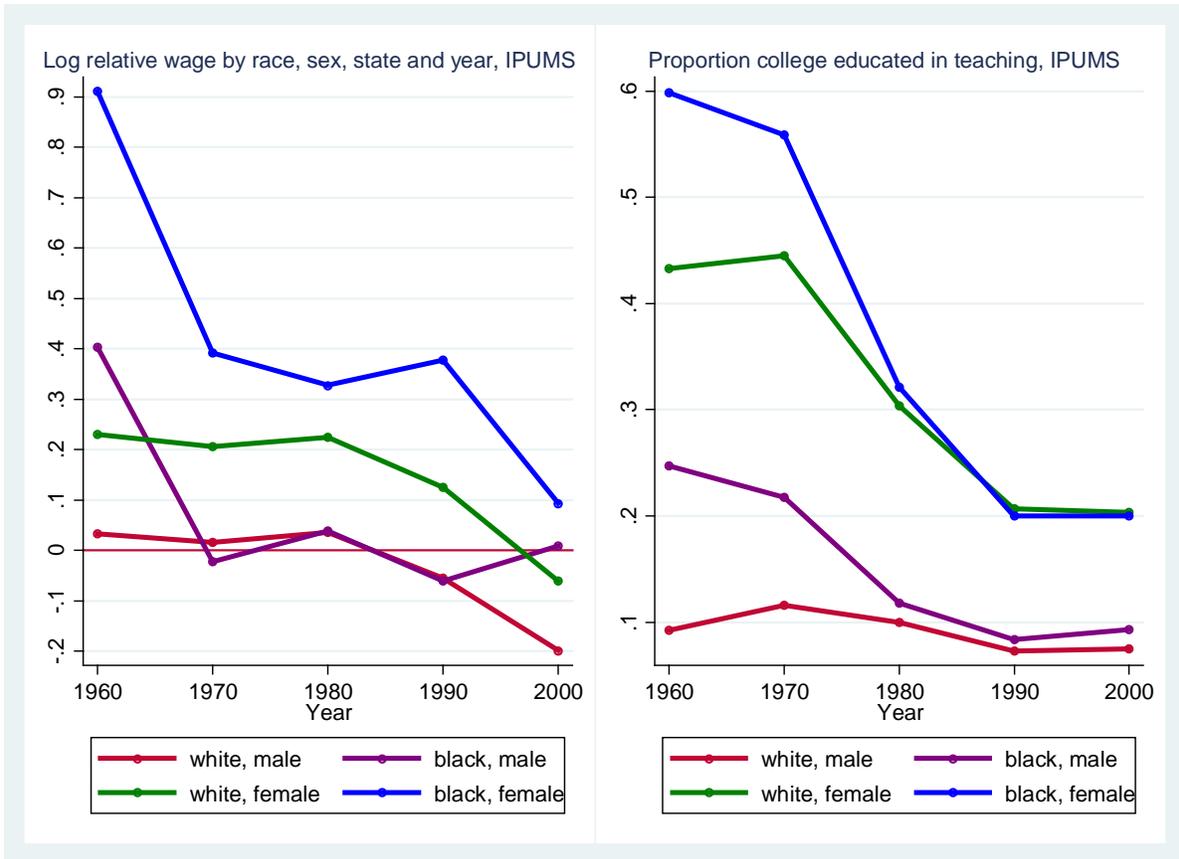
Source: IPUMS, 1960-2000 samples including women in the labor force, aged 21-60. Women are excluded who have less than an associate's degree, reside in group quarters or who are neither black nor white. Wages are CPI deflated to 2000 dollars.

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Figure 3

The proportion of college aged women choosing to teach fell over time and most precipitously for black women, the subgroup who experienced the largest decline in relative teacher pay.



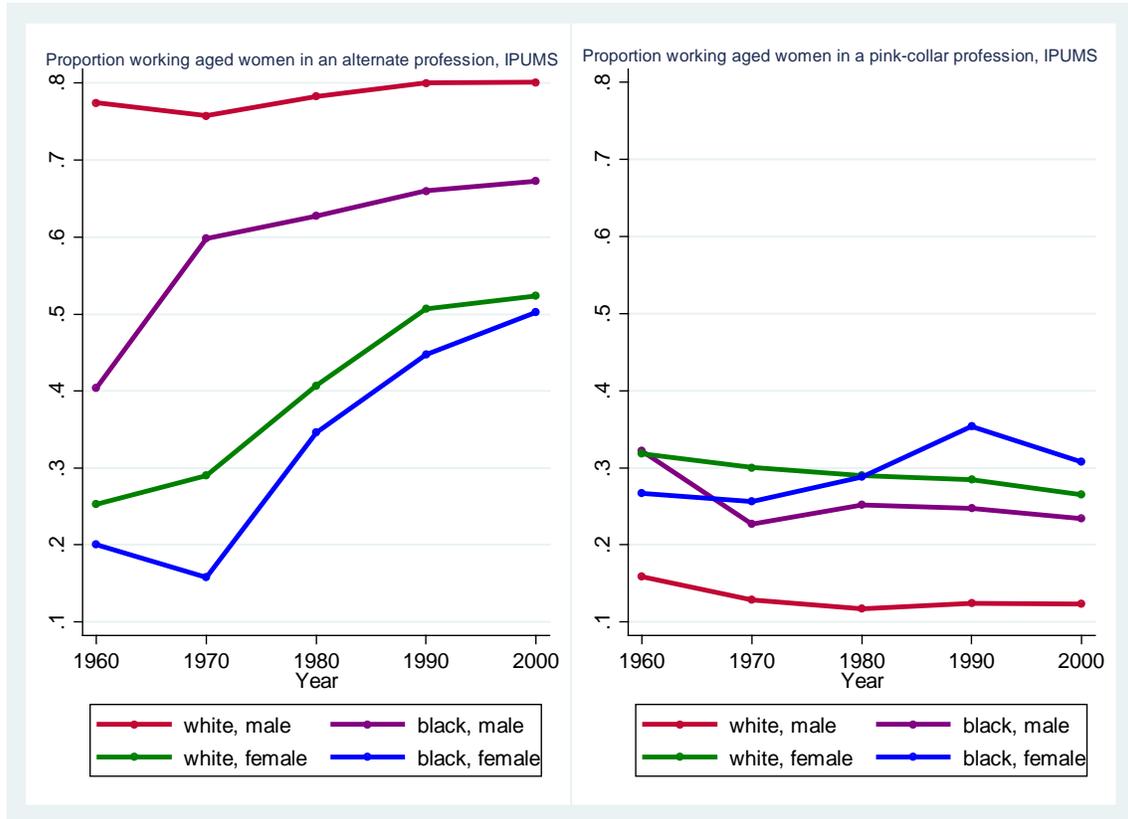
Source: IPUMS, 1960-2000 samples including women in the labor force, aged 21-60. Individuals are excluded who have less than 2 years of college education, reside in group quarters or who are neither black nor white. Wages are CPI deflated to 2000 dollars. Proportions of working are established by averaging work propensities across race, sex and year. Relative wages are calculated by averaging teaching and alternative professional wages by race, sex, state, birth year and year. Average teacher wages are then divided by average alternative professional wages and then put in natural log form.

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Figure 4

Alternative professions experienced only increases in the propensity of college educated men and women to enter. Pink-collar professions show much less volatility than teaching or alternative professions.



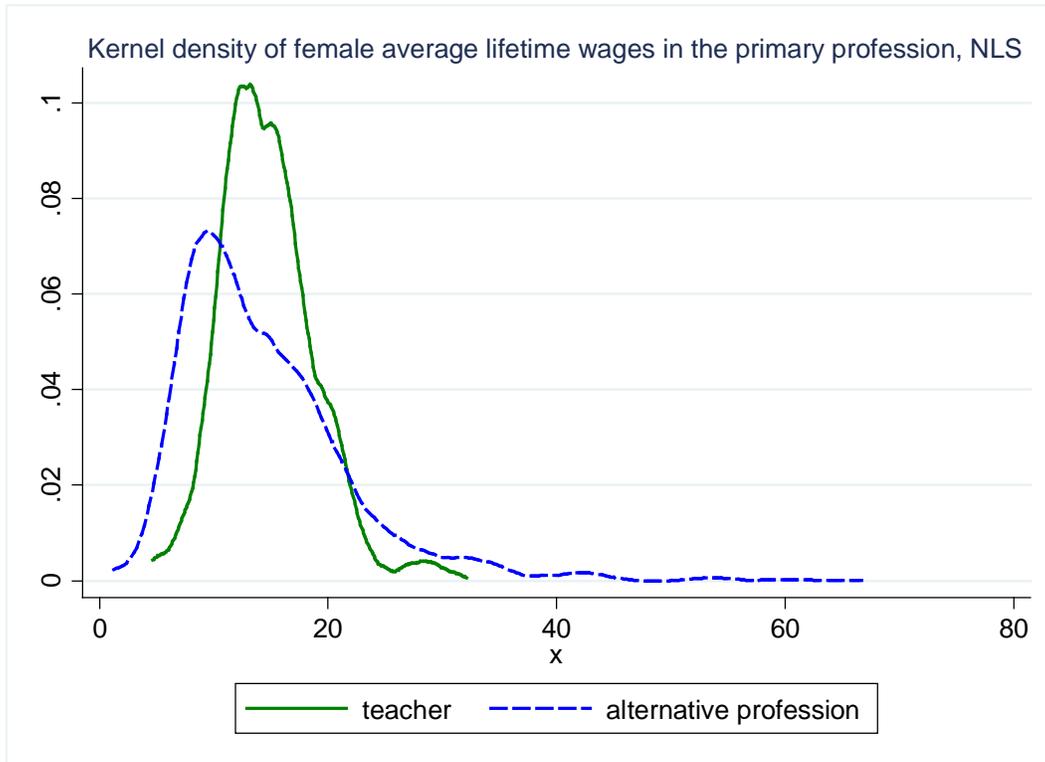
Source: IPUMS, 1960-2000 samples including women in the labor force, aged 21-60 Individuals are excluded who have less than an Associate's degree, reside in group quarters or who are neither black nor white. Wages are CPI deflated to 2000 dollars. Proportions of working are established by averaging work propensities across race, sex and year.

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Figure 5

There is less variation in teacher wage as compared with alternative professional earnings, NLS



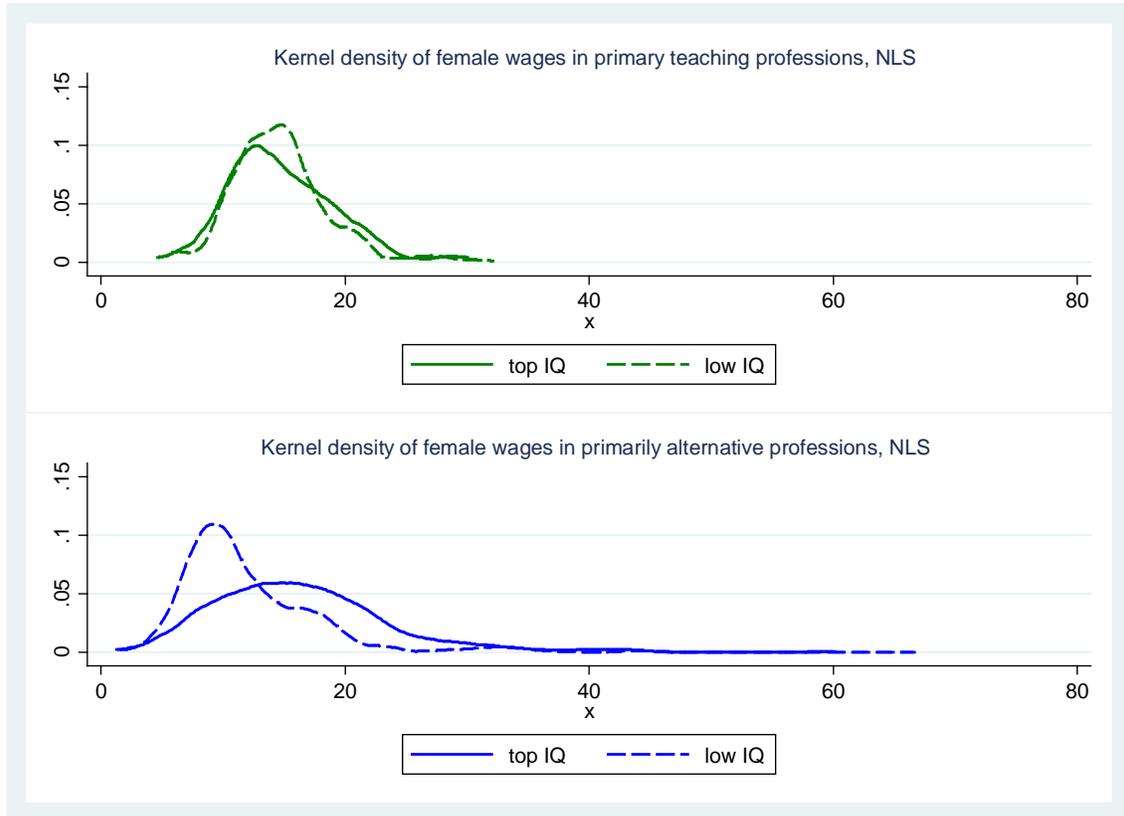
Source: National Longitudinal Survey, 1968-1997 samples. More detail provided in the data appendix on the construction of primary professional wages.

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Figure 6

Top IQ women benefit more from choosing alternative professions than teaching



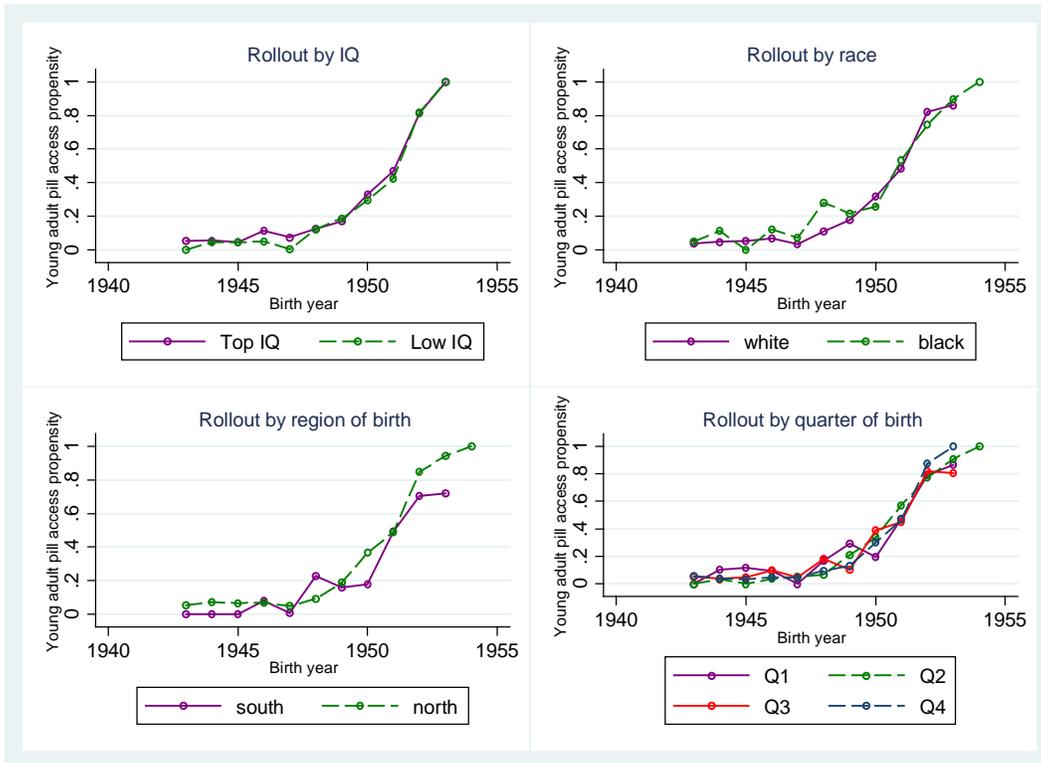
Source: National Longitudinal Survey, 1968-1997 samples. More detail provided in the data appendix on the construction of primary professional wages.

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Figure 7

The pill access rollout in young adulthood was highly consistent across cohorts by the available predetermined factors: IQ, race, region of birth and quarter of birth

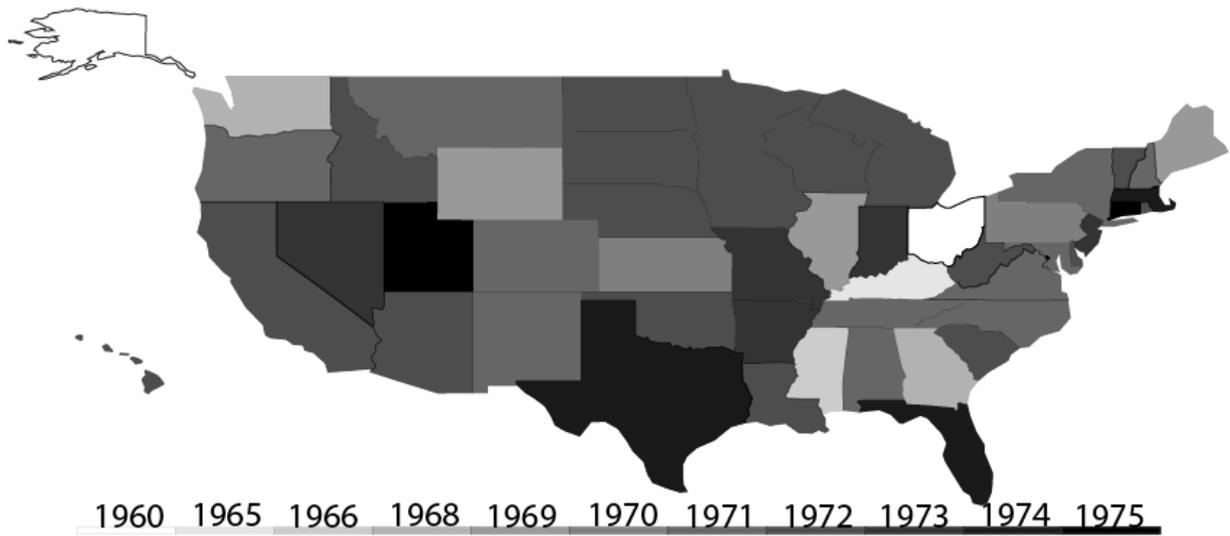


Source: National Longitudinal Survey, 1968-1997 samples. More detail provided in the data appendix on the construction of primary professional wages.

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Figure 8

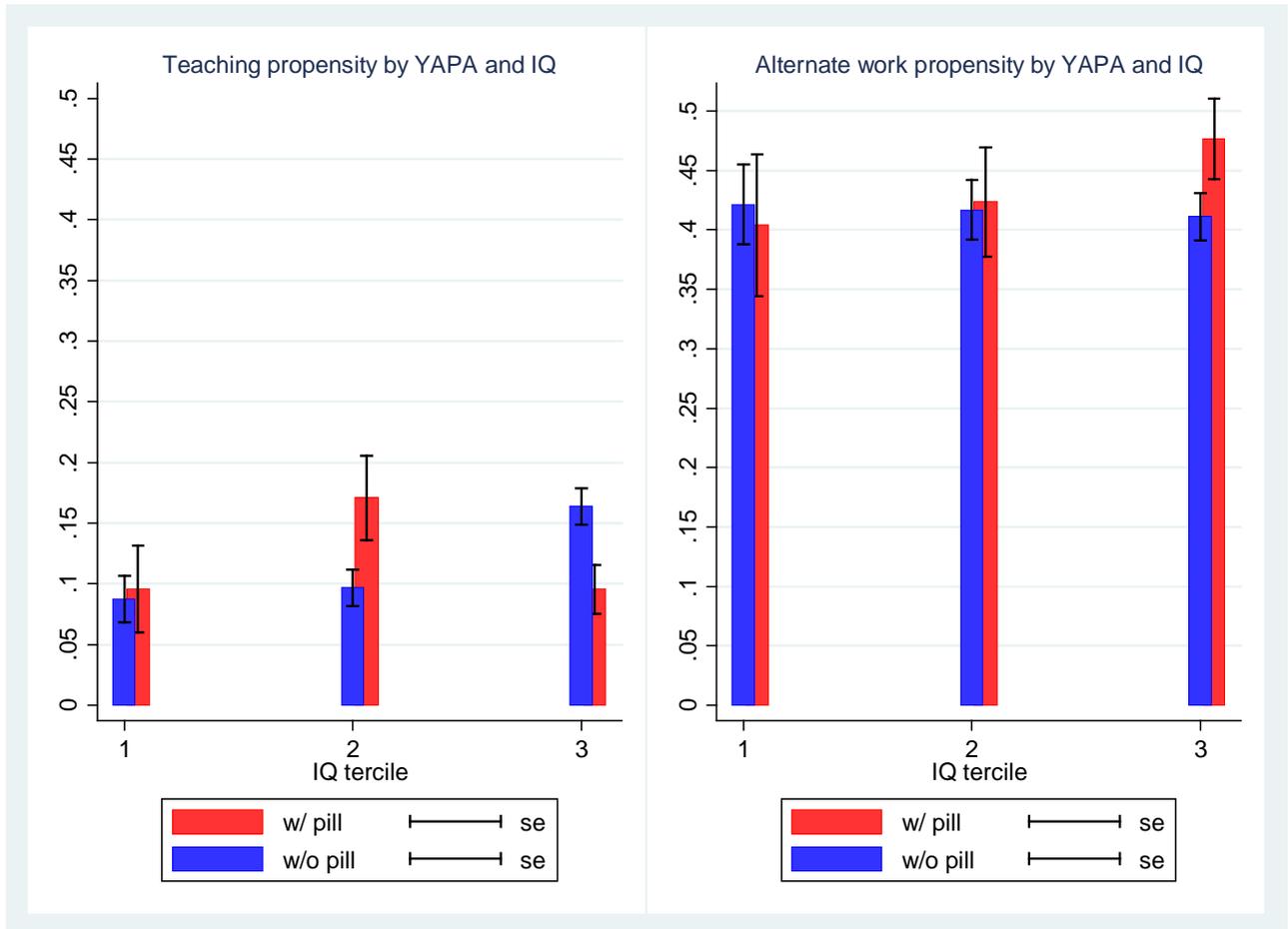


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Figure 9

The effect of young adult pill access on teaching and alternative work propensity



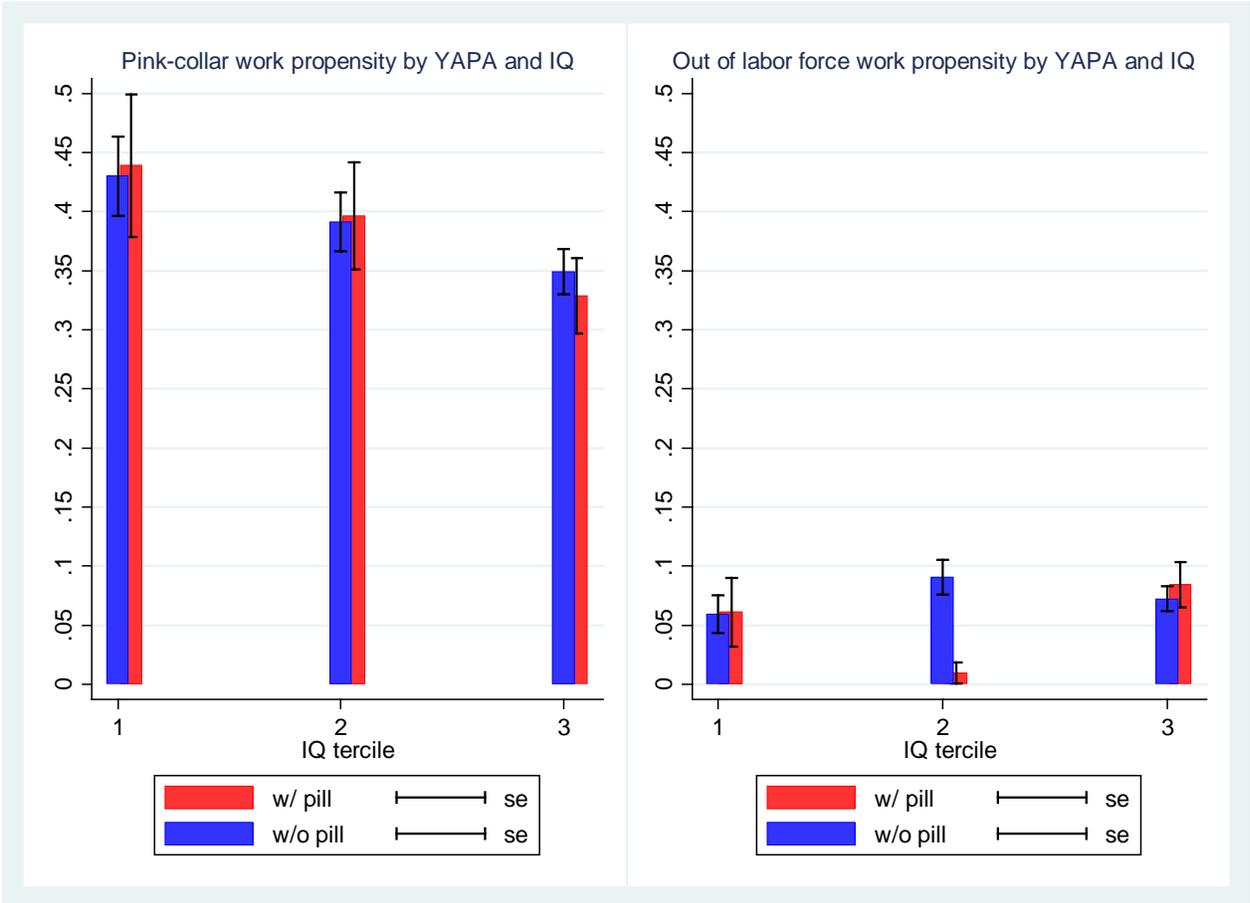
Source: National Longitudinal Survey, 1968-1997 samples. More detail provided in the data appendix on the construction of primary professional wages.

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Figure 10

The effect of young adult pill access on pink-collar and non-work propensity



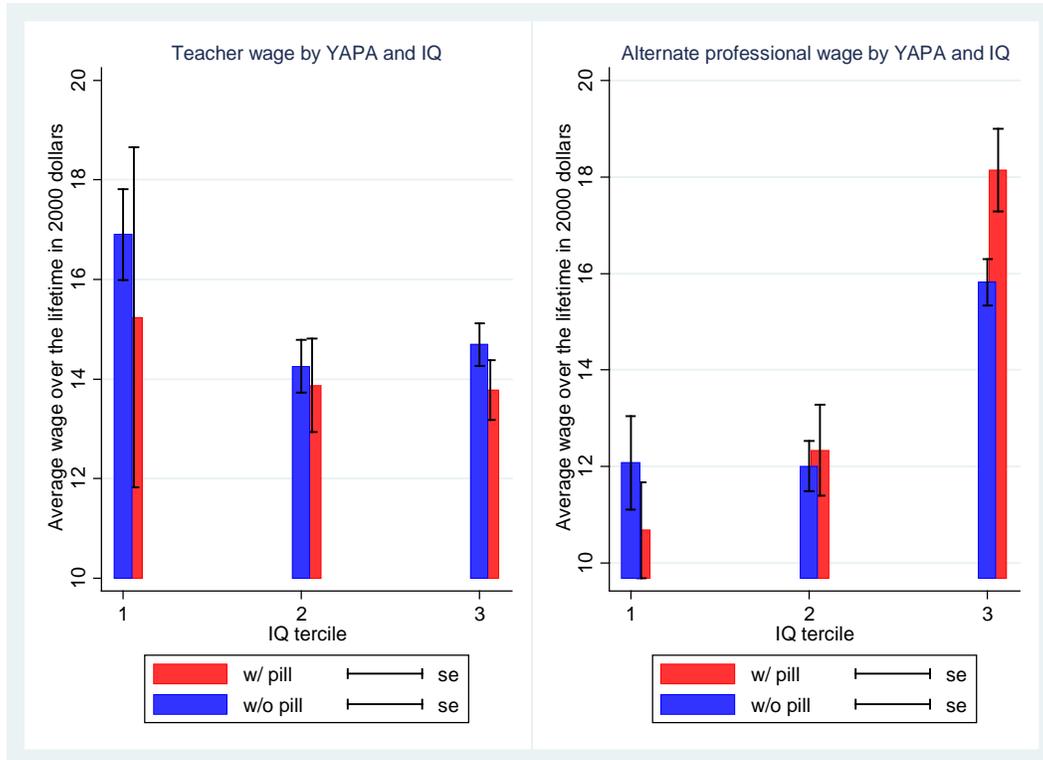
Source: National Longitudinal Survey, 1968-1997 samples. More detail provided in the data appendix on the construction of primary professional wages.

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Figure 11

The effect of young adult pill access on teacher and alternative professional pay



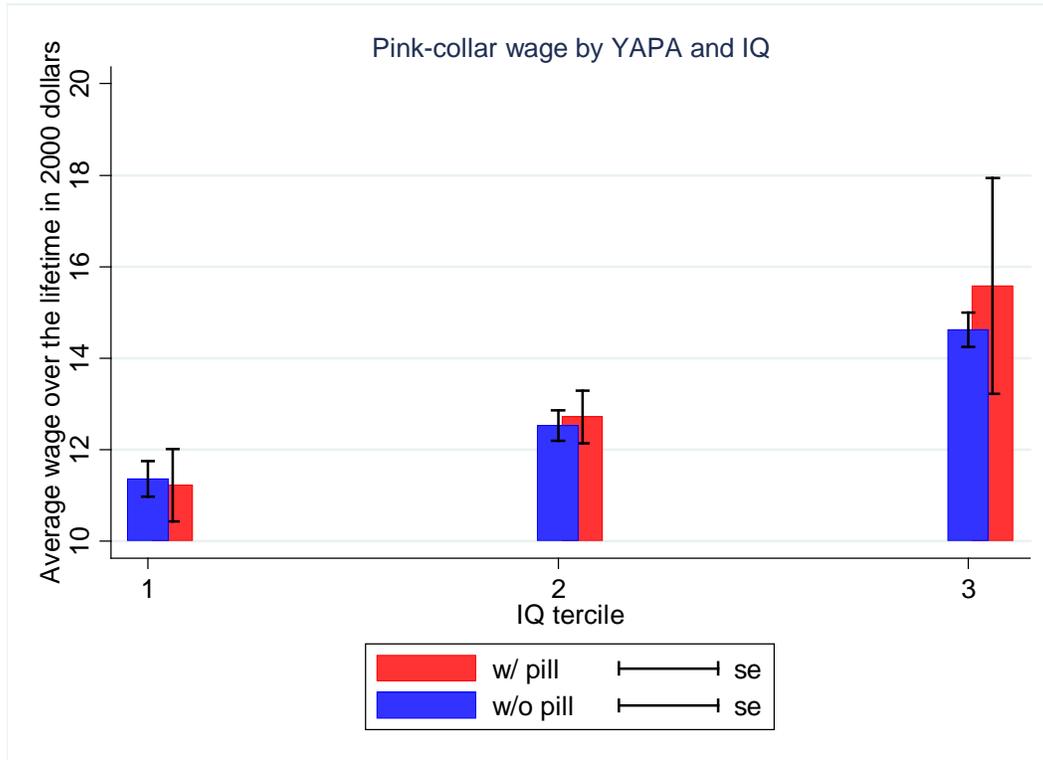
Source: National Longitudinal Survey, 1968-1997 samples. More detail provided in the data appendix on the construction of primary professional wages.

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Figure 12

The effect of young adult pill access on pink-collar pay



Source: National Longitudinal Survey, 1968-1997 samples. More detail provided in the data appendix on the construction of primary professional wages.

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Table 1

Summary statistics

| variable | N | mean | sd | min | max |
|---|----------|-------------|-----------|------------|------------|
| Demographics | | | | | |
| white | 2570 | 0.901465 | 0.298094 | 0 | 1 |
| black | 2570 | 0.098535 | 0.298094 | 0 | 1 |
| birth cohort | 2570 | 1948.529 | 2.976829 | 1943 | 1954 |
| topIQ | 1847 | 0.563717 | 0.496058 | 0 | 1 |
| lowIQ | 1847 | 0.436283 | 0.496058 | 0 | 1 |
| date of legal access (Bailey 2012) | 2211 | 1970.82 | 3.119553 | 1960 | 1975 |
| YAPA | 2211 | 0.320746 | 0.466869 | 0 | 1 |
| ever teach | 2570 | 0.279816 | 0.448996 | 0 | 1 |
| Primary profession | | | | | |
| mostly teach | 2570 | 0.133541 | 0.340225 | 0 | 1 |
| mostly altprof | 2570 | 0.427441 | 0.494804 | 0 | 1 |
| mostly pinkcollar | 2570 | 0.361234 | 0.480452 | 0 | 1 |
| mostly outlf | 2570 | 0.070394 | 0.255859 | 0 | 1 |
| Lifetime wages by primary occupation | | | | | |
| all | 2239 | 12.88968 | 8.003287 | 0 | 154.6975 |
| mostly teach | 296 | 14.71546 | 4.115265 | 4.655182 | 32.12741 |
| mostly altprof | 849 | 13.49869 | 7.552625 | 1.61381 | 154.6975 |
| mostly pinkcollar | 955 | 14.37205 | 7.433438 | 1.275556 | 66.68216 |

Source: National Longitudinal Survey, 1968-1997 samples. More detail provided in the data appendix on the construction of primary professional wages.

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Table 2

Reduced form results

| VARIABLES | (1) mostlyteach | (2) mostlyaltprof | (3) mostlypinkcollar | (4) mostlyoutlf |
|--------------|-----------------------|----------------------|-------------------------|----------------------|
| YAPA | 0.0946* (0.0489) | -0.0701 (0.0743) | -0.0196 (0.0865) | -0.00430 (0.0267) |
| topIQ | 0.0897*** (0.0203) | -0.0208 (0.0321) | -0.0589 (0.0378) | -0.0109 (0.0256) |
| YAPA*topIQ | -0.149*** (0.0338) | 0.0794 (0.0619) | -0.00826 (0.0538) | 0.0632** (0.0283) |
| Observations | 1,623 | 1,623 | 1,623 | 1,623 |
| R-squared | 0.065 | 0.054 | 0.049 | 0.062 |

regressions include race, state and cohort fixed effects

standard errors, clustered at the state in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Source: National Longitudinal Survey, 1968-1997 samples. More detail provided in the data appendix on the construction of primary professional wages.

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Table 3

First stage

| VARIABLES | (1) ln_mostlyaltprof_earn | (2) ln_mostlyteach_earn | (3) ln_mostlypinkcollar_earn |
|--------------------|------------------------------|----------------------------|---------------------------------|
| YAPA | -0.0623 (0.121) | -0.0137 (0.124) | -0.0645 (0.102) |
| topIQ | 0.260*** (0.0426) | 0.0169 (0.0635) | 0.162*** (0.0539) |
| YAPA*topIQ | 0.189 (0.115) | -0.0377 (0.0857) | -0.0909 (0.0992) |
| F stat on the inst | 3.32 | 0.18 | 1.12 |
| Observations | 590 | 210 | 550 |
| R-squared | 0.230 | 0.395 | 0.216 |

regressions include race, state and cohort fixed effects

all standard errors clustered at the state in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Source: National Longitudinal Survey, 1968-1997 samples. More detail provided in the data appendix on the construction of primary professional wages.

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Table 4

Second stage results

| VARIABLES | (1) mostlyteach | (2) mostlyteach | (3) mostlyteach | (4) mostlyteach |
|------------------------------|--------------------|--------------------|--------------------|--------------------|
| ln_relearn_ap_xb | 0.524* (0.314) | 0.465 (0.326) | | |
| ln_relearn_ap_top_xb | | 0.056 (0.068) | | |
| ln_mostlyaltprof_earn_xb | | | -0.694* (0.400) | -0.550 (0.388) |
| ln_mostlyaltprof_earn_top_xb | | | | -0.127 (0.099) |
| topIQ | 0.210** (0.095) | 0.193** (0.098) | 0.265** (0.132) | 0.564** (0.263) |
| Observations | 1,623 | 1,623 | 1,623 | 1,623 |

regressions include race, state and cohort fixed effects

bootstrapped standard errors

*** p<0.01, ** p<0.05, * p<0.1

Source: National Longitudinal Survey, 1968-1997 samples. More detail provided in the data appendix on the construction of primary professional wages.

Appendix A

Data details

This appendix provides a summary of the variables created for the analysis. Much of the formulation follows Bailey (2012). I use survey results from every year with uniformly non-missing entries. This includes: 1968, 1969, 1970, 1971, 1972, 1973, 1975, 1977, 1978, 1980, 1982, 1983, 1985, 1987, 1988, 1991, 1993, 1995, 1997.

Birth year

Birth year is surveyed in 1968, 1977, 1978, 1982, 1988, 1991, 1995, 1997, 1999, 2001 and 2003. I calculated the mode birth year from this sample and then removed individuals with a mode birth year before 1943 and have 1954.

State of Residence

The geographical information is based on a public sampling algorithm that matches information between the NLS, 1969 survey questions relevant to state of residence and the Census, 1969. In the algorithm, three variables from the NLS are used to create the link: an index of the demand for female labor in the area of residence, the Census division of residence, and educational expenditures per pupil in a labor market. Implicit state is calculated by matching information from these variables to state level data from the 1969 Census. The crosswalk was acquired from Martha Bailey who received it from Alessandra Voena and Elizabeth Peters. These latter authors thank Jeff Gray in their respective articles, Voena (2012) and Peters (2008), for developing the algorithm to match state of residence between the NLS, 1969 survey and the Census, 1969. This implicit state information is provided for about 85 percent of the data. Observations are missing when the matching was unable to be performed, due to limitations of the crosswalk, or individuals were missing from the survey year. The implicit state therefore reduces the sample size and has the potential to introduce measurement error, if the crosswalk is not completely accurate. To rectify this I am in the process of obtaining full state of residence from the Census Bureau. The application is currently under review by the Federal Census Bureau and should be approved by May of 2014.

Young adult pill access (YAPA)

Following the legal pill access coding in Appendix B from Bailey (2012), I identify the date at which a woman living in her state of residence should have received legal pill access. Then I indicate an individual had YAPA if her birth year + 20 was greater than or equal to the date of legal pill access.

IQ

A major advantage of these data are the information on IQ. IQ is surveyed once in 1968 by surveying IQ information available at the students most recent school attendance. Because the school's were sometimes not able to offer this information or nonresponsive, the IQ information is available for only about 70 percent of respondents. In this paper, I focus on IQ tercile although IQ quartile and quintile provide similar results.

Educational attainment

Educational attainment is surveyed in 1972, 1983, 1987, 1993 and 1997. I use the last available information on educational attainment, assuming that differences in reporting between 1987 and 1993, for example, were because the respondent acquired additional education. I then limit the sample to individuals with at least some college diploma.

Labor force participation and occupational choice

Employment status and occupational choice are surveyed every survey year. I generate 4 mutually exclusive and collectively exhaustive occupation categories for every survey year: teaching, non-teaching alternative professional work, non-teaching pink-collar work, and out of the labor force.

Teachers include art teachers and artists, dance teachers and dancers as well as musicians and music teachers because the pre-1980 job classifications don't allow a distinction between those employed only as teachers. For the post-1980 job classifications, the delineation includes the census codes 14, 154, 155, 156, 157, 158, 159, 163, 387 to incorporate all manner of teachers, school administrators, counselors, and teacher's aides. My goal was to provide as much power for the teacher classification as possible.

Pink-collar employees include clerical workers, waitresses, librarian, cosmetologists, household workers seamstresses, nurses, assistances, health aides and nursing aides. Alternative professionals include all other working individuals.

To generate primary occupation, I calculate the years an individual was a teacher, alternative professional, pink-collar professional or out of the labor force. Then I call the individual primarily a teacher if they spent more years teaching than in any of the other three designations. The individual is primarily a pink-collar professional if they spent more years in pink-collar work than in the other three designations. The individual is primarily an alternative professional if they spent more time as an alternative professional than any other work. And, similarly, the individual is primarily out of the labor force if the majority of their years are spent out of the labor force.

Wage

The wage was constructed using pay rate, pay unit, weekly hours and yearly weeks for 1968, 1969, 1970, 1971, 1972, 1973, 1975, 1977, 1978, 1980, 1982, 1983, 1985, 1987, 1988, 1991 and 1993. When the pay unit was given at the hourly level, then the wage was set equal to the pay rate. When the pay unit was given at the daily level, the pay rate was divided by 8. There was no information on the average number of hours a day worked. When the pay unit was reported at the weekly level, the pay rate was divided by the weekly hours worked in that same year. When the pay unit was reported at the bi-weekly level, meaning every other week, the pay rate was divided by twice the weekly hours worked to generate wage. When the pay unit was provided at the monthly level, the pay rate was divided by the weekly hours times 4, assuming the average month includes 4 weeks to calculate the hourly wage. When the pay unit was provided at the yearly level, the pay rate was divided by the weekly hours worked multiplied by the yearly weeks worked. There were also pay units provided at a piece rate or other unit that were excluded from the analysis. If part of the pay unit and pay rate was provided but no information on the weekly or yearly hours was provided, then the typical hours information was imputed—40 hours per week and 2000 hours per year. The wage for 1995 and 1997 was constructed with the hourly wage variable that was provided.

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The wages were all CPI deflated to 2000 dollars. Individuals reporting wages less than \$1/hour in 2000 dollars, thus reporting yearly wages less than \$1000/yr, had their wages removed.

Primary wage

The primary wage was an average of the wage earned while working in the primary occupation. For teachers who worked primarily in an occupation but without information on their wage, their wages were imputed first using individuals in the same state, year of birth, IQ and race and then, if unavailable, using only individuals of the same state, year of birth and IQ.

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Appendix B

Legal Coding (Bailey et al 2012)

| State | Access year, single women aged 18-19 |
|----------------------|---|
| Alabama | 1971 |
| Alaska | 1960 |
| Arizona | 1972 |
| Arkansas | 1973 |
| California | 1972 |
| Colorado | 1971 |
| Connecticut | 1972 |
| Delaware | 1972 |
| District of Columbia | 1971 |
| Florida | 1974 |
| Georgia | 1968 |
| Hawaii | 1972 |
| Idaho | 1972 |
| Illinois | 1969 |
| Indiana | 1973 |
| Iowa | 1972 |
| Kansas | 1970 |
| Kentucky | 1965/1968 (ambiguous coding) |
| Louisiana | 1972 |
| Maine | 1969 |
| Maryland | 1971 |
| Massachusetts | 1974 |
| Michigan | 1972 |
| Minnesota | 1972 |
| Mississippi | 1966 |
| Missouri | 1973 |
| Montana | 1971 |
| Nebraska | 1972 |
| Nevada | 1973 |
| New Hampshire | 1971 |
| New Jersey | 1973 |
| New Mexico | 1971 |
| New York | 1971 |
| North Carolina | 1971 |
| North Dakota | 1972 |
| Ohio | 1960 |
| Oklahoma | 1972 |
| Oregon | 1971 |
| Pennsylvania | 1970 |
| Rhode Island | 1972 |
| South Carolina | 1972 |
| South Dakota | 1972 |
| Tennessee | 1971 |
| Texas | 1974 |
| Utah | 1975 |
| Vermont | 1972 |
| Virginia | 1971 |
| Washington | 1968 |
| West Virginia | 1972 |
| Wisconsin | 1972 |
| Wyoming | 1969 |

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Appendix C
Robustness checks

Figure C1

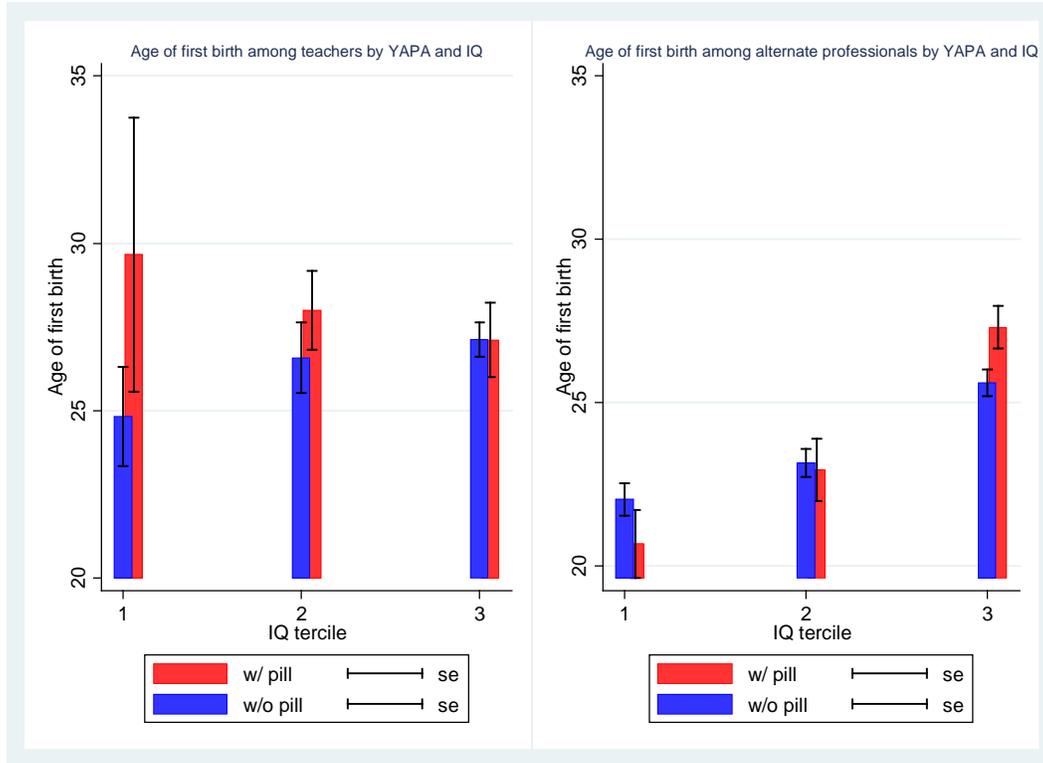


Table C1

Outcome: Age of first birth

| Primary Occupation | (1) all | (2) teach | (3) alternate | (4) pink-collar | (5) out of lf |
|--------------------|---------------------|-------------------|---------------------|--------------------|--------------------|
| YAPA | -0.407 (0.729) | -2.518 (4.006) | -1.570 (1.272) | 0.615 (1.433) | 0.626 (4.343) |
| topIQ | 1.719*** (0.336) | 1.132 (1.713) | 1.884*** (0.582) | 0.846 (0.624) | 3.462** (1.427) |
| YAPA*topIQ | 0.157 (0.730) | -0.539 (3.011) | 2.376 (1.530) | -1.199 (1.125) | 0.859 (2.438) |
| Observations | 1,107 | 145 | 475 | 387 | 96 |
| R-squared | 0.157 | 0.500 | 0.251 | 0.226 | 0.583 |

regressions include race, state and cohort fixed effects

standard errors, clustered at the state in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Source: National Longitudinal Survey, 1968-1997 samples. More detail provided in the data appendix on the construction of primary professional wages.

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Figure C2

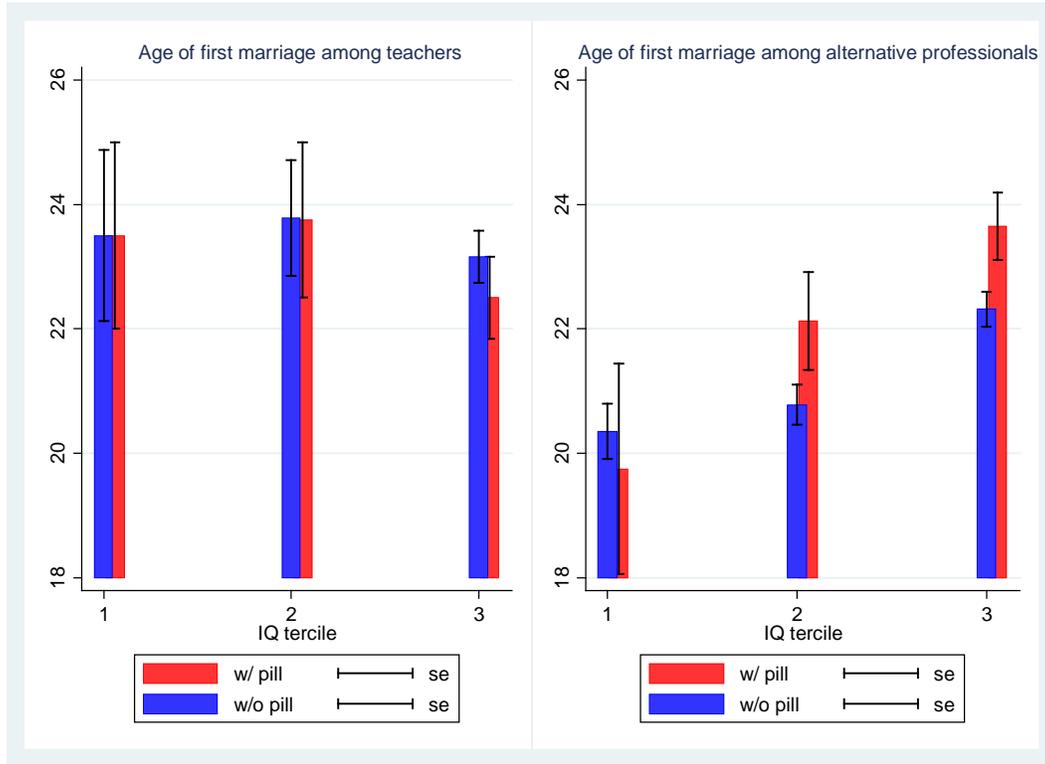


Table C2

Outcome: Age of first marriage

| Primary Occupation | (1) all | (2) teach | (3) alternate | (4) pink-collar | (5) out of If |
|--------------------|---------------------|-------------------|---------------------|--------------------|----------------------|
| YAPA | 0.696 (0.685) | -2.920 (2.527) | -0.158 (1.348) | 1.750 (1.448) | 3.708*** (0.674) |
| topIQ | 1.054*** (0.244) | -0.315 (1.458) | 1.489*** (0.471) | -0.0922 (0.534) | 3.106*** (0.872) |
| YAPA*topIQ | -1.270** (0.605) | -0.948 (2.098) | 0.637 (1.460) | -2.063 (1.461) | -6.833*** (1.994) |
| Observations | 978 | 138 | 413 | 337 | 88 |
| R-squared | 0.137 | 0.494 | 0.236 | 0.211 | 0.643 |

regressions include race, state and cohort fixed effects

standard errors, clustered at the state in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Source: National Longitudinal Survey, 1968-1997 samples. More detail provided in the data appendix on the construction of primary professional wages.

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Figure C3

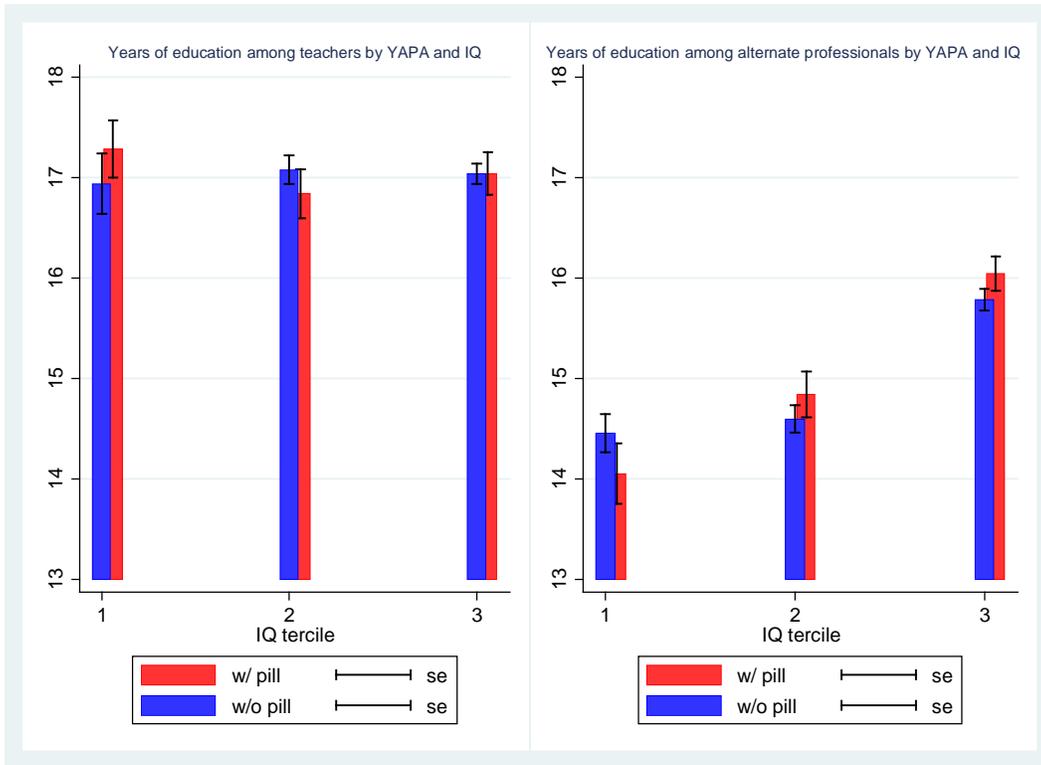


Table C3

Outcome: Years of education

| Primary Occupation | (1) all | (2) teach | (3) alternate | (4) pink-collar | (5) out of lf |
|--------------------|---------------------|---------------------|---------------------|---------------------|-------------------|
| YAPA | 0.336 (0.236) | -0.230 (0.461) | 0.0676 (0.349) | 0.454 (0.332) | -2.504 (2.743) |
| topIQ | 1.058*** (0.113) | -0.00470 (0.228) | 1.286*** (0.193) | 0.693*** (0.161) | 1.106 (0.927) |
| YAPA*topIQ | -0.342 (0.240) | 0.554 (0.519) | 0.0799 (0.275) | -0.393 (0.353) | 0.652 (2.358) |
| Observations | 1,363 | 206 | 582 | 479 | 88 |
| R-squared | 0.129 | 0.297 | 0.226 | 0.161 | 0.481 |

regressions include race, state and cohort fixed effects

standard errors, clustered at the state in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Source: National Longitudinal Survey, 1968-1997 samples. More detail provided in the data appendix on the construction of primary professional wages.

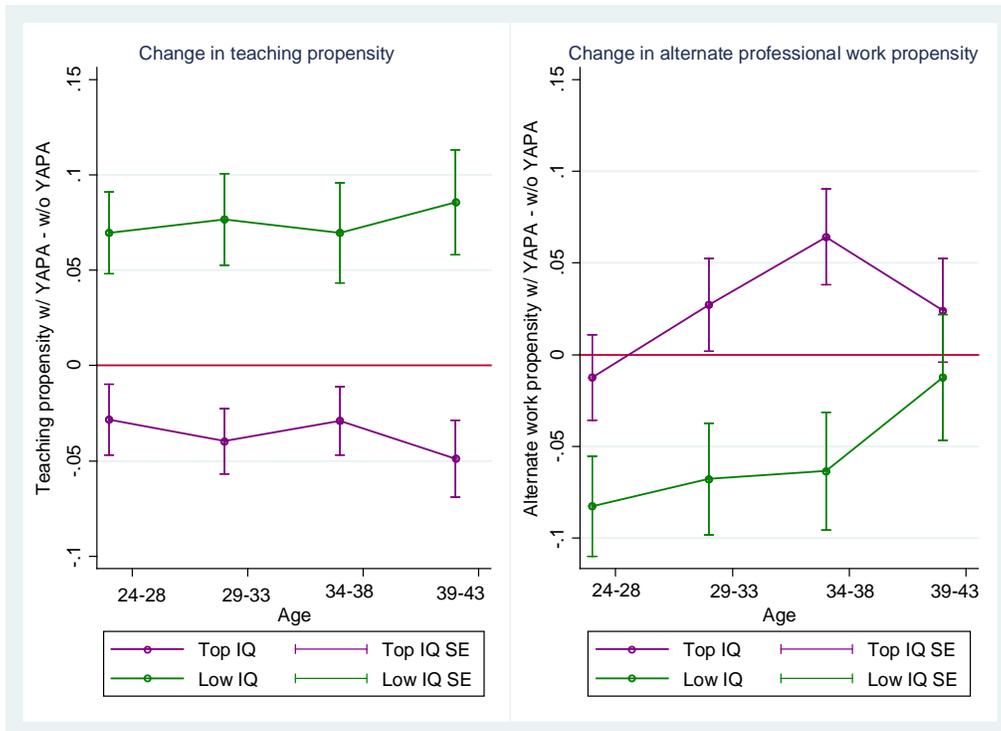
Appendix D

Wage trajectory by age

The framework presented in this paper in conjunction with the evidence presented in prior work (Lazear and Rosen, 1990; Steingrimsdottir, 2010; Miller, 2011; Bailey et al, 2012) suggests that delayed pregnancy should increase the returns to early career investments in a manner that sets women on a different career trajectory. For high-ability women, in particular, this career trajectory should produce a life-cycle of work behavior that disproportionately effects women in alternative professions, where the returns to work can change more significantly with age (Lazear and Rosen 1990). Figure D1 juxtaposes the changes in the career trajectory of women by IQ and career. It shows, in the left panel, that among teachers, high-IQ women are less likely to teach with young adult pill access, at all ages, and low-IQ women are more likely to teach with young adult pill access, at all ages. Therefore, teachers show no age dependent differences in their propensity to teach by IQ. On the other hand, in the right panel, where alternative professionals are displayed, considerable age variation in the life-cycle of work behavior is exhibited. High-IQ women with young adult pill access show a much greater increase in their work propensity in their mid-30s as compared to low-IQ women with young adult pill access. This suggests a life-cycle dynamic where high-IQ women in alternative professions try out a pink-collar or teaching occupation before selecting or being promoted into an alternate profession. Differences in the effect of young adult pill access on work behavior by IQ for pink-collar employees and those that are remain primarily out of the labor force, shown in Figure D2, are less striking.

Figure D1

Change in teaching and alternate professional work propensity over the life course due to young adult pill access



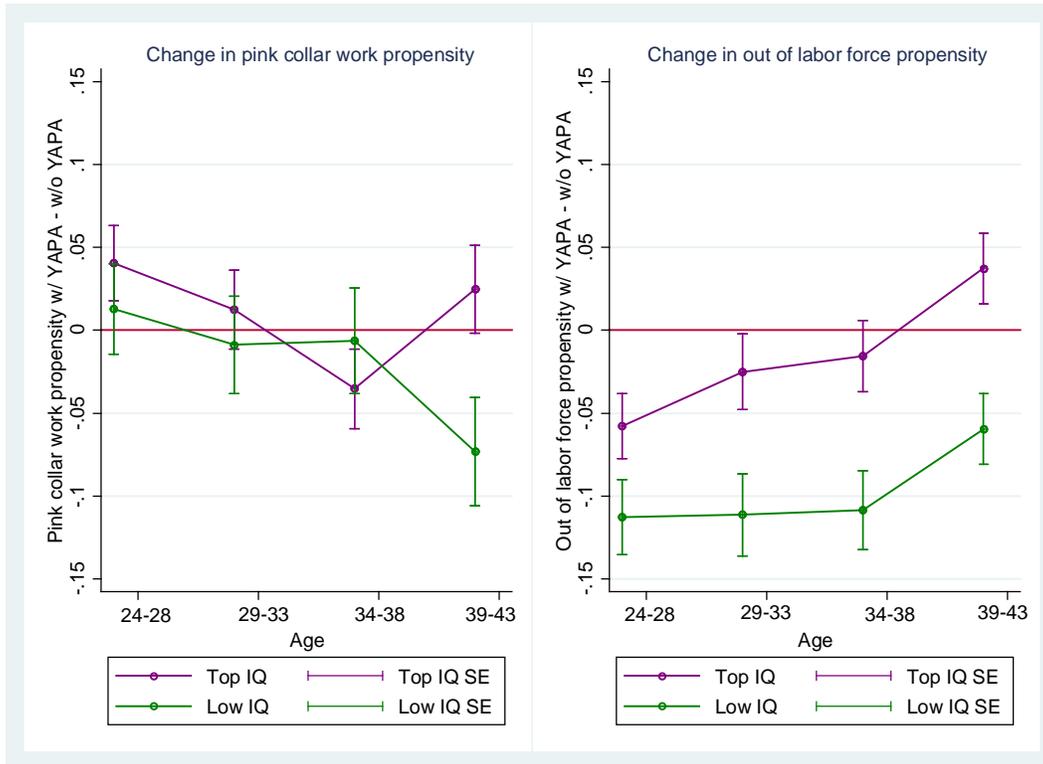
Source: National Longitudinal Survey, 1968-1997 samples. More detail provided in the data appendix on the construction of primary professional wages.

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Figure D2

Change in pink-collar work propensity and the propensity to be in the labor force over the life course due to young adult pill access



Source: National Longitudinal Survey, 1968-1997 samples. More detail provided in the data appendix on the construction of primary professional wages.

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Appendix E

Table E1

Reduced form, white

| VARIABLES | (1) mostlyteach | (2) mostlyaltprof | (3) mostlypinkcollar | (4) mostlyoutlf |
|--------------|-----------------------|----------------------|-------------------------|----------------------|
| YAPA | 0.0967* (0.0486) | -0.0447 (0.0750) | -0.0399 (0.0827) | -0.0122 (0.0269) |
| topIQ | 0.0845*** (0.0204) | -0.0141 (0.0345) | -0.0586 (0.0394) | -0.0128 (0.0270) |
| YAPA*topIQ | -0.155*** (0.0369) | 0.0557 (0.0665) | 0.00831 (0.0554) | 0.0760** (0.0293) |
| Observations | 1,328 | 1,328 | 1,328 | 1,328 |
| R-squared | 0.070 | 0.062 | 0.050 | 0.068 |

regressions include race, state and cohort fixed effects

standard errors, clustered at the state in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table E2

Reduced form, black

| VARIABLES | (1) mostlyteach | (2) mostlyaltprof | (3) mostlypinkcollar | (4) mostlyoutlf |
|--------------|---------------------|----------------------|-------------------------|----------------------|
| YAPA | 0.146 (0.0894) | -0.255 (0.161) | 0.0282 (0.203) | 0.0835* (0.0490) |
| topIQ | 0.253** (0.0967) | -0.0967 (0.0864) | -0.143* (0.0701) | -0.0137 (0.00882) |
| YAPA*topIQ | -0.230 (0.140) | 0.163 (0.209) | -0.00341 (0.133) | 0.0684 (0.0430) |
| Observations | 295 | 295 | 295 | 295 |
| R-squared | 0.201 | 0.210 | 0.250 | 0.219 |

regressions include race, state and cohort fixed effects

standard errors, clustered at the state in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Appendix F

Assessing the effect of bias in the instrument on the implications: A sensitivity analysis

One potential limitation of using the pill access rollout to young adult women as an instrument for pay is that it adjusts pay expectations by adjusting the risk of fertility. Particularly if the pill rollout affected women's pill access at all ages, there would be a concern that the benefits of career amenities that are complimentary to rearing children would be directly affected by the rollout. Such child-friendly work amenities may be paramount among teaching's non-pay attributes. Unfortunately, directly testing the degree of correlation between non-pay benefits of young adult pill access and the choice to teach is not possible. However, an alternative means of exploring the sensitivity of the results to the bias of the instrument is to examine the change in the implications of the result under the condition that working conditions confound the effect of young adult pill access on relative pay. This would imply that the negative effect of the pill on relative pay of high ability women is driven by the negative correlation between the pill and the change in the attractiveness of teaching's working conditions.

To explore the effect of a biased instrument mathematically, consider equations (10) through (17) below. The model presented in the statistical framework, and replicated in equations (10) and (11) below, suggests the choice to teach is given by y , which is indexed by the individual, i , and is a function of relative pay, $(w_{scq}^t - w_{scq}^n)$ which differs by state, s , birth cohort, c , and ability, q . The return to pay is also allowed to differ by ability and captured by β_q . To isolate exogenous variation in relative pay, I predict relative pay with access to the pill, under the assumption that pill access in young adulthood produces state-by-cohort variation in non-teacher pay that is random with respect to state-by-cohort differences in working conditions.

$$P(\text{teach}_{iscq} = 1) = y_i = \beta_q(w_{scq}^t - w_{scq}^n) + d_s + d_c + d_q + \varepsilon_{iscq} \quad (10)$$

$$E[w_{scq}^t - w_{scq}^n] = \theta_q YAPA_{sc} + d_s + d_c + d_q + \vartheta_{scq} \quad (11)$$

For credible identification of the effect of relative pay on the choice to teach, the correlation between young adult pill access and ε_{iscq} must be zero. To model the effect of a failure of this requirement, let the error, ε_{iscq} , include working conditions, wc , as shown in equation (12).

$$\varepsilon_{iscq} = wc_{sc} + \varphi_{iscq} \quad (12)$$

Using this framework, I note that covariance between young adult pill access and ε_{iscq} is given by the covariance between young adult pill access and working conditions, if we allow young adult pill access to be unrelated to φ_{iscq} .

$$\text{cov}(YAPA_{sc}, \varepsilon_{iscq}) = \text{cov}(YAPA_{sc}, (wc_{sc} + \varphi_{iscq})) = \text{cov}(YAPA_{sc}, wc_{sc}) \quad (13)$$

Moreover, if I model the effect of pill access in young adulthood on working conditions as shown in equation (14), I can allow the covariance between working conditions and young adult pill access to be determined by the parameter, π_q , that equates the pill and working conditions and variation in young adult pill access.

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$$wc_{cs} = \pi_q YAPA_{cs} + \vartheta_{qcs} \quad (14)$$

$$cov(YAPA_{sc}, wc_{sc}) = \pi_q * var(YAPA_{sc}) \quad (15)$$

Next, I recall that the IV estimator for β_q is given by the reduced form relationship between young adult pill access and the choice to teach scaled by the first stage effect of young adult pill access on relative wage.

$$\beta_q^{IV} = \frac{cov(y_i, YAPA_{sc})}{var(YAPA_{sc})} \bigg/ \frac{cov(w_{scq}^t - w_{scq}^n, YAPA_{sc})}{var(YAPA_{sc})} = \frac{cov(y_i, YAPA_{sc})}{cov(w_{scq}^t - w_{scq}^n, YAPA_{sc})} \quad (16)$$

Since $cov(y_i, YAPA_{sc}) = cov(w_{scq}^t - w_{scq}^n, YAPA_{sc})\beta_q + cov(YAPA_{sc}, \varepsilon_{iscq})$, the β_q^{IV} estimator is given by the true effect of relative pay on the choice to teach and the bias of the effect of young adult pill access on working conditions scaled by the first stage of young adult pill access on relative pay. This is shown in equation (17).

$$\widehat{\beta}_q^{IV} = \beta_q^{IV} + \frac{cov(YAPA_{sc}, \varepsilon_{iscq})}{cov(w_{scq}^t - w_{scq}^n, YAPA_{sc})} = \beta_q^{IV} + \frac{\pi_q}{\theta_q} \quad (17)$$

Under the assumption that young adult pill access reduces the non-pay working conditions of teaching, π_q in equation (14) is negative. Given that young adult pill access is negatively correlated with relative pay, θ_q in equation (10), is negative. As a result, the naive estimation of the system of equations in (10) and (11) would be upwardly biased by the effect of young adult pill access on working conditions. This would suggest that the results in this paper imply that working conditions are positively correlated with the choice to teach, and less so for high-ability women, not that relative pay is positively correlated with the choice to teach. However, in either case, the implications of the research remain, given the estimates of the paper *understate* the a priori assumption that the relative pay is increasingly relevant for higher ability women.