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ABSTRACT

The Differential Impacts of Contingent Employment on Fertility: Evidence from Australia*

Many studies have reported evidence of negative associations between fixed-term contract employment and fertility. With few exceptions, these studies assume that employment status is exogenous and thus results are likely biased. Furthermore, previous research has mostly not considered whether the effects of employment status on fertility might vary with other worker characteristics. We draw on 19 years of data from the Household, Income and Labour Dynamics in Australia (HILDA) Survey to investigate the association between contingent forms of employment (including both fixed-term and casual employment) and first births, and how that association varies with selected worker characteristics. The issue of endogeneity is addressed through the use of instrumental variables estimation. Our main finding is that both fixed-term contracts and casual employment are associated with a significantly lower probability of first births among men. We also find that these negative fertility effects vary with workers' education, occupational status, country of origin, age, and relationship status. The results for women suggest fixed-term contracts are actually associated with more births. However, in this case one of the instruments fails to satisfy the exclusion restriction, suggesting endogeneity remains a concern when analyzing female fertility outcomes and hence this finding should be given little weight.

JEL Classification: J13, J41

Keywords: Australia, contingent employment, employment instability, fertility, HILDA Survey, instrumental variables

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Introduction

The resumption of the long-term decline in fertility rates in Western nations in the aftermath of the Global Financial Crisis of 2008/09 has seen renewed research interest in the sources of this decline. Perhaps the explanation most frequently posited is rising economic uncertainty (e.g., Kreyenfeld et al. 2012; Seltzer 2019; Vignoli et al. 2020a). One important contributor to this uncertainty is contingent employment, or jobs "in which an individual does not have an explicit or implicit contract for long-term employment or … in which the minimum hours worked can vary in a nonsystematic manner" (Polivka and Nardone 1989: 11). Indeed, many researchers have identified growth in fixed-term contracts, one of the most common types of contingent employment, as an important factor contributing to the postponement of family formation (e.g., Adsera 2011; Auer and Danzer 2016; de la Rica and Iza 2005; Kreyenfeld et al. 2012; Vignoli 2020a). A lack of certainty about job tenure, irregular and unpredictable working hours, relatively little control by workers over their working situation, and the inferior rights and entitlements often associated with contingent forms of employment, are all thought to act as impediments to family formation and lead to postponement of childbearing (e.g., Modena et al. 2014; Schmitt 2012; Vignoli et al. 2012).

Research in this space is, however, confronted by a serious methodological problem — the endogeneity of employment and labor supply decisions with respect to fertility intentions. Ongoing or permanent employment status (i.e., non-contingent employment) may thus be a function of preferences for children rather than a factor that directly influences those preferences. Relatedly, workers in permanent jobs may have characteristics, often not easily observed, that both systematically differ from those in non-permanent jobs and are correlated with preferences for children. If so, simple regression models that treat fertility outcomes as a function of recent employment status will result in biased estimates of the true causal impact.

Another weakness of previous research is that while it is common practice to allow the association between contingent employment and fertility behavior to vary with a worker's sex, it is far less common to examine whether the association might vary with other worker characteristics. This is surprising given "people differ in their vulnerability to precarious work, depending on their personality dynamics, levels and kinds of education, age, family responsibility, type of occupation and industry" (Kalleberg 2009: 10).

We address these limitations by leveraging 19 years of longitudinal data, covering the period 2001-2019, from the Household, Income and Labour Dynamics in Australia (HILDA) Survey. As in previous research, regression equations predicting first birth among a sample of childless workers are estimated, but, in contrast to other studies, an instrumental variables approach is used to address the endogeneity problem. More specifically, the share of workers employed in fixed-term contract and casual employment in each worker's industry is used to identify the causal effect of contingent employment on fertility. The idea is that structural variations in the share of contingent employment across industries are exogenous to the individual, and therefore generate variations in fertility that are unrelated to unobserved factors that may jointly influence the likelihood of engagement in contingent employment and fertility outcomes. Furthermore, not only are separate models for men and women estimated, but also models that distinguish between more and less educated, high and low income, high and low occupational status, Australian-born and immigrant, younger and older, and single and partnered workers.

Australia provides an especially interesting context for studying the impact of contingent employment on fertility. First, the total fertility rate in Australia has, after a brief reversal in the late-2000s, resumed its long-term trend decline, falling to an historical low of 1.66 in 2019 (Australian Bureau of Statistics 2020). And this rate is projected to only decline further in the decades ahead (McDonald 2020). Second, by international standards, the proportion of workers in contingent forms of employment is very high in Australia. The data from the HILDA Survey, for example, indicate that employees on a fixed-term contract accounted for 9.2% of the employed Australian workforce in 2019 and casual employees (who in theory are provided no advance commitment by their employer of either continuity of employment or the number of days or hours to be worked) a further 19.1%. This high level of casual employment is without parallel in any other Western country.

Theoretical Background

Theoretical approaches to understanding childbirth decisions and outcomes are extensive, with parallel literatures emanating from a wide range of disciplines (Balbo et al. 2013). Economic approaches (for a review, see Jones et al. 2011) have their roots in Becker (1960), which emphasizes the effects of income and the cost of children on the demand for children (i.e., number of children desired). Within this framework, parents desire children, derive utility from raising children, and jointly share the cost of rearing children. Raising children requires economic resources; in particular, the income to purchase the goods and services (such as food, housing, schools, and enriched learning environments) that are critical for children's development. Hence, when incomes fall, all other things equal, decisions to have children are delayed or foregone and fewer children are born (a negative income effect). Thus, if contingent employment is associated with lower wages, as has been found in many countries (OECD 2015), it follows that it will also be associated with fewer births. Especially relevant for our analysis is Ranjan's (1999) extension to Becker's model, in which uncertainty regarding future income will increase the income threshold beyond which parents decide to have children. Since contingent employment, by definition, is associated with more uncertain, and often less predictable, wage income, this will compound the income-level effect just described, further delaying decisions to have children.

Ranjan (1990) notwithstanding, and despite it long being recognised that fertility rates are responsive to general economic uncertainty and rising unemployment at the economy-wide level (Easterlin 1976), uncertainty has not played a major role in economic models of individual fertility behavior. In contrast, in most recent sociological studies uncertainty is central (for a review, see Balbo et al. 2013). This sociological tradition has its roots in Oppenheimer's theories (1988, 2003), which emphasize that long-term commitments, such as marriage and parenthood, require job stability or at least realistic prospects of stable employment. Labor market experiences characterized by nonemployment and irregular and temporary types of employment are thus frustrating experiences that negatively impact on family planning and transitions to parenthood (see also Bernardi et al. 2008). Additionally, other contributions (e.g., Mills and Blossfeld 2005) point to the importance of globalization trends, which when coupled with more relaxed labor market protections and regulations, are argued to have exposed youth to more employment uncertainty than in the past. Different from Ranjan's (1999) approach, Oppenheimer emphasizes that it is not just uncertainty about the future income that delays parenthood, but also the uncertainty attached to more precarious (i.e., contingent) forms of employment. However, both theoretical approaches arrive at the same conclusion; that increased uncertainty concerning income and / or employment reduces fertility.

As noted previously, people also likely differ in their vulnerability to contingent work. Among individual characteristics, education and income are perhaps the two most likely important sources of heterogeneous fertility responses. As discussed by Vignoli et al. (2020a), how the association between employment uncertainty (and thus contingent employment) and fertility varies with education is difficult to predict. On the one hand, the highly educated have the most to lose if employed in jobs where the likelihood of job loss or reduced hours is high. On the other hand, the better educated are both better equipped to deal with temporary job loss or declines in labor earnings (because they are better positioned to secure stable well-paid employment in the future) and more likely to have voluntarily selected into their current employment state than their less educated peers. Vignoli et al. (2020a) also suggest that this association between employment uncertainty, education and fertility might vary with earner status within the household. Among secondary earners (usually women) the cost of temporary withdrawal from the labor force is relatively low for the least educated, and thus fertility decisions of the less educated may not be greatly affected (see also Becker 1991). Conversely, for primary earners (usually men) the greater income uncertainty associated with contingent employment might have a greater negative impact on the fertility decisions of the least educated given their likelihood of securing more stable ongoing employment in the future is less than for the highly educated.

Associations between contingent employment and fertility might also be expected to vary with income in a similar pattern, given education and income are positively correlated. However, if the relevant construct is household income (rather than say individual labor earnings), then the role of earner status should not matter. It then follows that a priori we cannot predict how the association of interest will be moderated by income given there are forces operating in opposite directions.

It has also been argued, and found, that fertility is associated with a worker's occupation (Barakat and Durham 2013; Begall and Mills 2012; Cooney and Uhlenberg 1989). Studies in this space argue that occupations can influence fertility in two ways. First, high-status occupations are correlated with lengthy educational requirements, leading to delays in the timing of first births. Second, while high-status occupations may be associated with a higher degree of autonomy and flexibility (which may increase the compatibility between working life and parenthood), they are often coupled with longer working hours, higher earnings and lucrative career paths, all of which increase the cost of children. These arguments suggest that employment in high-status occupations will tend to be associated with reduced fertility, but will these effects be amplified or diminished in the presence of contingent employment? As with education, we expect the effects might differ between men and women (or more specifically, between primary earners and secondary earners), with negative effects on fertility likely amplified by contingent work for men in low-status jobs whereas among women it will be those in high-status occupations where effects will be largest.

Another dimension along which the fertility effects of contingent work may be heterogeneous is country of origin. Research undertaken across many Western countries has consistently found that immigrant groups, especially those with relatively poor skills in the language of their host country, are disadvantaged in the labor market, disadvantages that may decline only slowly over time (e.g., Borjas 2000, 2015; Clark and Lindley 2009; Kifle et al. 2019; Strøm et al. 2018). Further, in some countries, and with some specific immigrant cohorts, there may be a trade-off between the probability of employment and job quality (Panichella 2018). Immigrant groups may thus have a tendency to be over-represented in more precarious forms of work (though the evidence in support of this claim is mostly restricted to recent immigrant arrivals; e.g., Anderson 2010; OECD 2020). If so, would this over-representation have more significant consequences for immigrant fertility outcomes? We argue that this is not necessarily so, and that relative to native-born populations, contingent employment will have weaker impacts on the fertility choices of many immigrant groups. First, for some immigrant groups, contingent employment in their new country may compare favourably with labor market options in their former home country. Second, if the self-selected nature of migration means immigrant groups on average are relatively more motivated and entrepreneurial, as is often argued (e.g., Boneva al. 1998; Chiswick 1999; Vandor 2021), then it seems likely that they may be less affected by the potential income uncertainty associated with contingent employment.

Finally, differences in age and relationship status among workers are also expected to influence fertility outcomes. Sociologists have long argued that the negative fertility effects stemming from rising economic and employment uncertainty will be typically greater among young people, and especially young men (e.g., Mills and Blossfeld 2005; Oppenheimer 2003). Additionally, older females face the constraint that the remaining period during which children can be conceived is much shorter. We therefore expect to find stronger negative effects of contingent employment on fertility among younger than older workers. We also expect the negative effects of contingent employment to be stronger for persons living in relationships than for single persons. The intuition here simply relies on the empirical fact that for the majority of people a stable coupled relationship is a precondition for having children, and hence all influences on fertility will matter more to couples.

Past Research

A growing number of empirical studies, mostly undertaken with data from European countries, have identified negative associations between contingent forms of employment, and especially fixed-term (or time-limited) contracts, and fertility behaviour. For example, de la Rica and Iza (2005) used data in Spain and showed that women employed on fixed-term contracts were more likely to delay entry into motherhood than women holding permanent contracts. Similar findings have been reported for other European countries, including Finland (Sutela 2012), France (Dupray and Pailhé 2018; Pailhé and Solaz 2012), Germany (Auer and Danzer 2016; Schmitt 2012; Tölke and Diewald 2003), Italy (Barbieri 2011; Conti and Sette 2013; Modena et al. 2014; Vignoli et al. 2020a), Netherlands (van Wijk et al. 2021), and Sweden (Lundström and Andersson 2012), as well as in multi-country studies (e.g., Adsera 2011; González and Jurado-Guerrero 2006; Vignoli et al. 2020b). It thus should not be surprising that a meta-analysis of the effect of employment instability on fertility (based on estimates from 22 European studies) found time-limited contracts to be associated with a lesser likelihood of childbearing (Alderotti et al. 2021). Specifically, women employed on time-limited contracts were estimated to have, on average, 14% lower odds of having a child compared with women employed on unlimited contracts, and men 6% lower odds.

Despite the emergence of this Western continental European consensus, confirmatory evidence from other parts of the developed world is both sparse and less supportive. Schmitt (2012), for example, analyzed household panel survey data from both Germany and the UK, and in the case of the UK could find no evidence that fixed-term contracts were associated with a significantly lower likelihood of first birth. Similarly, studies undertaken in both Russia (Sinyavskaya and Billingsley 2015) and Japan (Raymo and Shibata 2017) have not found statistically significant relationship between contingent forms of employment and subsequent fertility outcomes. In Australia, the evidence is also not strongly supportive. Using data from the HILDA Survey, Laß (2020) examined how the transition to first birth within couples was affected by different forms of non-standard employment. This study reported a large negative association between first births and temporary forms of employment, but this association was entirely driven by temporary agency jobs, which account for a very small share of the Australian workforce. Neither casual nor fixed-term contract employment, which are far more widespread, were significantly associated with first births. Relatedly, Bassford and Fisher (2020) used the same data set to examine the determinants of fertility intentions (as distinct from actual births). They included a control for casual employment status and found no evidence of any significant relationship.

A notable feature of the existing literature is that rarely has any serious attempt been made to address the endogeneity between contingent employment and fertility. Indeed, most research does not even recognize the issue exists. An exception is Vignoli et al. (2020a) who employed propensity score matching methods, which they argued could overcome the endogeneity problem. This approach involves finding two groups that differ with respect to the characteristic of interest, or 'treatment' (in our case, whether employed in a contingent form of employment or not), but on all other characteristics are perfectly matched. The obvious weakness is that treatment and control groups can only be matched on observables, and thus this method requires the strong assumption that there are no systematic differences in the unobserved characteristics of the two groups.

Arguably the preferred approach would involve the identification of a natural experiment where one group of workers is exposed to some type of exogenous shock or change that affects their employment status but does not directly affect fertility decisions. Examples of such types of non-random shocks, however, are rare. Indeed, we could only identify one study of the effects of employment status on fertility that comes close to meeting quasi-experimental conditions. In that study, by Lopes (2020), the 'experiment' was a policy change in Portugal in 2003 that increased the maximum duration for which a firm could retain a fixed-term contract worker before they had to be converted to permanent status from three years to six. The treatment here was thus not a change in employment status per se, but a change in the conditions of employment for fixed-term contract workers. Comparing women's decisions before and after the policy change, Lopes (2020) found that women commencing work in a fixed-term contract job that started after the reform had a lower average probability of giving birth than women commencing employment on fixed-term contract prior to the reform.

In the absence of convincing natural experiments, the most common method for dealing with endogeneity, especially within economics, is instrumental variable (IV) estimation. However, this technique has also been rarely applied in studies of the impact of employment status on fertility. Again we could only identify one example – Modena et al. (2014) – and that was a study not of birth outcomes, but of fertility intentions. In this study educational cohort is used as an instrument for the job insecurity associated with employment contract type, with women who left education in the period 1981-1985 or 1995-2008 argued to be more likely to be employed on fixed-term contracts or in atypical jobs. They conclude that the hypothesis of no endogeneity cannot be rejected. In contrast, we are highly skeptical about the power of the instrument selected.

Another feature of the literature is that relatively little consideration has been given to the possibility that the effects of contingent employment might vary with worker characteristics (other than sex). The notable exception here is education, with at least four different studies (Baizán 2007; Pailhé and Solaz 2012; Auer and Danzer 2016; Vignoli et al. 2020a) explicitly testing whether there is an interactive effect of employment status and education level on fertility outcomes. Results are mixed and not always significant; this is especially the case for Baizán (2007), who used data from the European Community Household Panel to examine second- and higher-order births in four countries and did not uncover any consistent pattern across the countries considered. Nevertheless, the better studies in this group (Pailhé and Solaz 2012; Vignoli et al. 2020a) suggest that for women at least, and when considering first birth conception, precarious forms of employment are more likely to be associated with postponement among the most highly educated.

With respect to interactions with other variables, Lundström and Andersson (2012) examined whether the effects of temporary employment might differ between the foreign and the native-born (in Sweden), Dupray and Pailhé (2018) whether effects were different for the children of immigrants (in their French sample), and Modena et al. (2014) whether impacts might vary with household income (in their Italian sample). But beyond that, we are unaware of any other research that has allowed the effects of contingent employment to vary with worker characteristics.

Data and Methods

Sample

The data used in this analysis are drawn from Release 19 of the HILDA Survey (Department of Social Services / Melbourne Institute of Applied Economic and Social Research 2020), a longitudinal survey that commenced in 2001 with members of a nationally representative sample of Australian households (see Watson and Wooden 2021). Each year the survey collects extensive information about household and family formation, employment, income, and various socioeconomic outcomes and life events. Interviews are sought with every member of the household aged 15 years and over. Additionally, interviewees are asked to fill in a self-completion questionnaire (SCQ). In wave one, interviews were obtained from 13,969 individuals from 7,682 households. A further 2,153 households were added in wave 11 (providing an additional 4,009 respondents). Rates of sample loss and attrition are relatively low, with the annual re-interview rate rising from 87% in wave 2 to over 96% by wave 9 and remaining above that level in subsequent waves (Summerfield et al. 2020).

The analytical sample is restricted to childless individuals aged between 18 and 45 years at time of interview. While the dataset contains 19 consecutive waves (i.e., years) of observations, the nature of the outcome variable (described in more detail below), which requires observations be observed for up to five consecutive waves, means observations from waves 16 through 19 are only used in the construction of the outcome variable. Conversely, observations from waves 1 through 4 are only used in the construction of explanatory variables. Further observations in consecutive waves also results in a loss of observations because of wave-specific non-response and attrition. The final sample for estimation comprised 36,790 observations on 7,670 individuals (3,943 men and 3,727 women).

Variables

The outcome variable is a binary variable equal to one if an individual has a first birth that is recorded for the first time in either the second, third or fourth survey wave after the collection of employment data, and zero if he/she remains childless.[1] This timing ensures employment status is always observed at a time prior to conception. We also check whether results are sensitive to alternative time windows (two or three survey waves and two to five survey waves later).

The key explanatory variables are derived from information describing employment and labor force status. Following the labor force framework used by the Australian Bureau of Statistics, which in turn is based on conventions adopted by the International Labour Organization, respondents at each survey wave are divided into three labor market states based on their activity during the week preceding interview — employed, unemployed, or not in the labor force. Among the employed we then distinguish between employees, the self-employed, and unpaid family workers. The HILDA Survey then goes further and asks employees to choose one among four categories that best describes their current employment contract in their main job. This enables the construction of variables that distinguish between seven mutually exclusive states: (i) permanent employee; (ii) fixed-term contract employee; (iii) casual employee; (iv) self-employed; (v) other employed; (vi) unemployed; and (vii) not in the labor force. The focus of this analysis is on groups (ii) and (iii), fixed-term contract and casual employees, and how their fertility outcomes differ from that of comparable employees in permanent or ongoing positions in group (i).[2]

A great advantage of the HILDA Survey is the variety of information provided about the respondent, enabling the inclusion of numerous control variables for respondent's personal

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[1] Following Drago et al. (2011), multiple births are treated as single births.
[2] The group "other employed", which includes unpaid family workers and employees whose contract status could not be determined (such as piece rate workers), might also be considered contingent workers. This, however, is a very small group, representing less than 0.5% of all Australians aged 15 years or older.

and household characteristics. These are age, education, partnership status, if partnered the employment status of that partner, country of birth, presence of a long-term health condition, all of which are specified as categorical variables, and household income. The latter is measured with the log of equivalized real disposable household income and covers the financial year (the year ended 30 June) prior to interview. Sex differences are dealt with by estimating separate models for men and women. More detailed definitions of each variable (along with summary statistics) are provided in Table 1.

In additional specifications where we check the robustness of our results, measures of fertility intentions, workplace entitlements, the industry unionization rate, and egalitarian views are included. Fertility intentions is represented by a subjective measure of desire for children scored on an ordinal 0 to 10 scale. Workplace entitlements is a count of the availability of six different types of work-friendly benefits at the respondent's workplace. Examples include employer-funded paid maternity leave, formal arrangements for working from home, and flexible start and finish times. The unionization rate is the number of members of trade unions and employee associations divided by total employment within one of 19 major industry divisions, as defined in the Australian and New Zealand Standard Industrial Classification (ANZSIC), and weighted using annual cross-section population weights. Finally, following Baxter et al. (2015), an indicator of egalitarian views is constructed by summing responses to seven questions about respondents' attitudes toward mothering and the division of labor within the household. The scores on this index range between 7 (egalitarian views) and 49 (traditional views). These questions, however, have not been included in every wave of the survey, and hence we assume that these views are stable over the sample period and use the average of the available scores. The sample is then divided into two groups — those holding egalitarian views and those holding traditional views — using the population median as the threshold.

Finally, note that all control variables are either measured in the year when employment status is recorded or (in the case of country of birth and the egalitarian views indicator) are time-invariant.

Analytical Strategy

We are interested in estimating the causal effect of contingent forms of employment on fertility decisions, where contingent employment is represented by fixed-term contract employment and casual employment. We first estimate ordinary least squares (OLS) regressions of the probability of a first birth using the following equation:

$$Y_{i,t2-t4} = \alpha_0 + \alpha_1 F_{i,t} + \alpha_2 C_{i,t} + \alpha_3 X_{i,t} + \mu_s + \lambda_t + \varepsilon_{i,t} \quad (1)$$

$Y_{i,t2-t4}$ is a dummy variable equal to one if individual *i* has a first birth any time in the second, third or fourth years after the interview date (year *t*). The coefficients of interest are α_1 and α_2, which measure the impacts of fixed-term contract employment ($F_{i,t}$) and casual employment ($C_{i,t}$). The vector X_{it} comprises the various individual characteristics discussed earlier, noting that this also includes controls for the other employment and labour force states. We also include state fixed effects, μ_s, to control for unobservable differences in fertility behaviours across the different states of Australia, and year fixed effects, λ_t, to account for changes in fertility trends over time that are common to all states.

Estimation of equation (1) using OLS, however, may produce biased results because of endogenous selection and unobserved heterogeneous characteristics of respondents. To address such concerns, we implement an IV method. Specifically, we use variation in the rates of fixed-term contract and casual employment by industry as instruments for individual employment status. The first stage of IV approach is specified as follows:

$$F_{i,t} = \beta_0 + \beta_1 FR_{i,t} + \beta_2 CR_{i,t} + \beta_3 X_{i,t} + \mu_s + \lambda_t + \epsilon_{i,t} \quad (2)$$

$$C_{i,t} = \gamma_0 + \gamma_1 FR_{i,t} + \gamma_2 CR_{i,t} + \gamma_3 X_{i,t} + \mu_s + \lambda_t + \nu_{i,t} \quad (3)$$

where the instrumental variables are the rates of fixed-term contracts ($FR_{i,t}$) and casual employment ($CR_{i,t}$) in the industry division where individual *i* is working at time *t*. For those currently unemployed or not in the labor force, we employ information on the industry of their last job.[3]

For an instrumental variable to be appropriate, however, it must satisfy three conditions: (i) the instrument is correlated with the endogenous variable (the relevance condition); (ii) the instrument affects the outcome variable only through the endogenous variable (i.e., the rates of fixed-term contract and casual employment do not have a direct influence on fertility), also referred to as the exclusion restriction; and (iii) there is no confounding for the effect of the instruments on fertility.

The relevance condition requires that the probability of being a fixed-term (or casual) employee is influenced by the share of fixed-term contract (or casual employment) within an industry, and can be easily verified by looking at the correlation between the instruments and the endogenous regressors.

Establishing whether the remaining two conditions are satisfied is more difficult. Our instrument is motivated by a literature using aggregated labor market conditions as the instrument for employment status (e.g., Gathergood 2013; Gregg 2001; Neumark 2002). A clear advantage of our instrument is that, as an aggregated measure, it cannot be endogenous to individual decisions. This, however, does not guarantee exogeneity. Even if our instrument does not affect the dependent variable directly, it may do so indirectly through other channels that are not controlled for in our regression. To address this concern, we devised a placebo test that involves regressing the fertility outcome on our two instruments (the industry-specific rates of fixed-term contract and casual employment) using the sample of permanent

[3] Consequently, those who never had a job were excluded from the analysis (approximately 10% of the sample).

employees. If the exclusion restriction holds, the coefficients on the instruments should be statistically insignificant in this specification.

If confounding is an issue we should also find our results sensitive to the inclusion of other potentially endogenous variables, such as fertility intentions, or other influences correlated with the instruments. We thus re-estimate our models after including indicators of fertility intentions, egalitarian views about parenting, workplace entitlements, and the industry-specific unionization rate (described earlier), and check whether our estimates are much affected by their inclusion.

Finally, we emphasise that all equations are estimated using a linear probability model rather than models specifically designed for binary outcomes (i.e., probit or logit models). The linear probability model has the advantages of being generally more tractable for assessing causation and, most importantly, is well suited to situations involving not only a binary outcome variable but where the regressors of interest are endogenous dummy variables (Angrist 2001), which is likely to be the case with $F_{i,t}$ and $C_{i,t}$.

Results

Main Findings

A summary of the key coefficients of interest from both OLS and IV estimation are reported in Table 2. More complete results, including estimated coefficients for all control variables, are reported in an Appendix (Table A1). Beginning with the results from OLS estimation, which assumes fixed-term contract and casual employment are exogenous, we find, in line with expectations, that casual workers have a lower probability of first births within the first two to four years after the survey date. The magnitude of this association for men, however, is quite small — just 1.9 percentage points less than for comparable permanent employees. Among women, the estimated magnitude is larger – 5.5 percentage points lower. In contrast, and contrary to expectations, no association between fixed-term contract employment and the probability of first birth is found.

The results from IV estimation, however, suggest very different conclusions. First, among men the negative association with casual employment is much larger – a 9.7 percentage points smaller probability of first birth (compared with the 1.9 percentage point difference suggested by the OLS estimates). In contrast, for women there was little difference in the point estimate attached to casual employment, but the slightly smaller estimate together with greater inefficiency causes the IV estimate to be statistically insignificant. Second, large and highly significant effects of fixed-term contracts, but which operate in opposite directions for men and women, are now found. For men a large negative effect of 18.4 percentage points is found, whereas for women the IV estimates suggest an even larger effect – 22.6 percentage points – operating in the positive direction.

Clearly choice of estimator matters. OLS estimation suggests casual employment has only a modest restraining effect on births while fixed-term contracts have no effect. In contrast, the IV estimates suggest large negative impacts of both types of employment on fertility outcomes of men but not of women: Indeed, fixed-term contract employment among women is associated with far more births.

While not the focus of this analysis, we also note that results on most controls, but not all, are in line with expectations. Thus we find the probability of first birth exhibits an inverse U-shaped relationship with age (which is in line with previous studies; e.g., González and Jurado-Guerrero 2006), is higher for coupled than single persons, is lower for those with long-term health conditions, and among men (but not women) rises with educational attainment. Surprisingly, however, we found no association with household income. Also unexpected, birth rates were lower among immigrants from non-English-speaking backgrounds (though this might reflect the under-representation of recent immigrant cohorts in the HILDA Survey sample).

Instrument Validity

As discussed earlier, ex ante we favour the IV results given both the likelihood that contingent employment and fertility are jointly determined by variables that we cannot control for in the analysis and employment status is a function of fertility preferences. However, before we can be confident that our IV estimates are delivering consistent estimates, we need to be satisfied that the instruments meet the three pre-requisite conditions of relevance, exclusion, and the absence of confounders.

Relevance is established by inspecting the results from the first-stage of the two-step IV estimation, where the outcomes are the two endogenous variables representing the two contingent employment states and the explanatory variables of interest are the two instruments. These results are presented in summary form in Table 3 and reveal large positive and highly significant associations between each instrument and its relevant endogenous counterpart. Further, a formal test (the Kleibergen-Paap test) delivers F-statistics that are very large, and much larger than the critical value of 16.38 suggested by Stock and Yogo (2002), leading us to reject the null hypothesis that the instruments are weak.

Even more importantly, we also report results from the Durbin–Wu–Hausman test of endogeneity. This is a test of the hypothesis that OLS estimates are consistent. For both men and women the test statistics are large and highly significant, meaning the null is rejected, which in turn implies that concerns surrounding the endogeneity of contingent employment are warranted.

We next need to verify that our instruments only affect fertility through individual employment status (and more specifically, fixed-term contract and casual employment status). While no formal test for exclusion exists, we argue that a crude check is provided by regressing birth outcome on our instruments using a sample of respondents restricted to permanent employees. If birth outcomes among permanent employees are significantly associated with the industry shares of fixed-term contract and casual workers, then it would be difficult to argue that the instruments only affect fertility through contingent employment status. Conversely, the absence of significant associations would be consistent with the claim that the exclusion restriction condition holds. The results of this estimation are reported in Table 4. As can be seen, both of the relevant coefficient estimates are statistically insignificant for men. However, this is not the case for women, with the fixed-term contract industry share a highly significant predictor of fertility outcomes. We thus cannot be satisfied that our instruments are valid in the case of women, and hence that the problem of endogeneity has been appropriately dealt with. For the remainder of this analysis, we therefore focus on men (but the comparable results for women are reported in the Appendix).

Finally, we checked whether the results are robust to the inclusion of potential confounders. If controlling for other factors changes the estimates in a significant way, the validity of the instruments would be questionable, and the estimates might be biased. We first add a measure of fertility intentions. The idea is that controlling for fertility intentions will capture any unobservables that determine fertility outcomes that we were unable to control for in the baseline specification. The criteria used to determine who is asked the fertility intentions question, however, is different in waves 5, 8, 11, 15, and 19, thus forcing us to exclude data from these waves in regressions including fertility intentions. Summary results, reported in column (2) of Table 5, show strong evidence that fertility intentions are a predictor of birth outcomes. At the same time, however, the effects of contingent employment on fertility remain comparable to the baseline results reported in column (1).

Second, we conducted a similar exercise using family-friendly workplace entitlements as an additional control variable. Information on receiving entitlements is taken from the SCQ, and thus is subject to additional non-response.[4] We use the total number of different types of workplace entitlements received and present the results in column (3) of Table 5. Somewhat surprisingly, we find no evidence of a relationship between workplace entitlements and fertility outcomes. This might be explained by the focus on men whose employment decision is less likely to be affected by workplace entitlements.[5] Again, inclusion of this variable had relatively little impact on the estimated coefficients for contingent employment, although the coefficient for casual employment is somewhat smaller.

Next, in column (4), we add the industry unionization rate. If unions provide greater employment protection for workers in firms and industries where unions have greater coverage, then we might expect this to be associated with pro-birth outcomes. And indeed, we find a strong correlation between unionization rate and fertility. Nevertheless, our estimate on casual employment remains little affected. The negative coefficient estimate on fixed-term contract employment, however, does become much larger in absolute terms, possibly suggesting that if anything our estimates of fixed-term contract might be conservative.

Fourth, we examine gender egalitarianism as a potential confounder. It is possible that men with more traditional attitudes are more likely to have children, and at the same time may also be more likely to found in jobs providing permanent employment. The results shown in column (5) of Table 5 confirm that those with egalitarian views are associated with

[4] Despite this, inclusion of a variable identifying SCQ non-respondents in our model of births resulted in a small and insignificant coefficient, suggesting little difference between SCQ respondents and non-respondents in terms of fertility outcomes.

[5] Indeed, the effect of workplace entitlement on fertility is statistically significant when we use the female sample (see Appendix Table A2).

lower fertility. But again, the inclusion of this variable had no substantial effect on the magnitudes of the main estimates.

Overall, this evidence provides us with confidence that the assumption that our instruments are not strongly correlated with potential confounding factors is justified.

Robustness Checks

We also subjected our results to several additional robustness tests. First, and as noted earlier, we checked whether our findings were affected by measuring fertility outcomes over periods of time of different length — namely two to three and two to five years from the survey year. The results presented in columns (2) and (3) of Table 6, show a consistently negative impact of contingent employment on fertility outcomes for men. That said, the absolute magnitude of these effects does rise with the length of the observation window.

We next checked whether our results for first births for childless individuals would also apply to higher-parity birth outcomes. The results, presented in column (4) of Table 6, are inconclusive. Both of the coefficients of interest are statistically insignificant but in the case of casual employment, the estimated negative coefficient is larger in absolute size than that reported in the baseline specification. Large standard errors, however, prevent us from reaching any definitive conclusion.

We also followed previous studies on fertility and checked whether our results are affected by differential time trends across the different Australian states and territories (Ananat et al. 2013; Schaller 2016). We thus augmented the baseline specification with variables that interact the state dummies with a linear time trend. The results, presented in column (5) of Table 6, are little different from the results of the baseline specification.

Heterogeneity

The final phase of our analysis is to determine whether these impacts of contingent employment are uniform or vary with worker characteristics. Again we focus on the male sample, but for completeness the results using the female sample are presented in the Appendix (see Table A5). In line with our theoretical discussion, we examined whether the association between contingent employment and subsequent fertility outcomes varies with education, income, occupational status, country (or region) of birth, age, and relationship status. The key results of these heterogeneity analyses are reported in Table 7. For all regressions, we also report the *p*-value from a Wald test of equality of coefficients between subgroups.

First, we examine the differential impact of contingent employment on workers of different educational level based on whether or not respondents have a bachelor's degree or higher-level qualification. We hypothesized that for primary earners (and hence most men) the effects of contingent employment would be greatest among the least educated. The results, reported in columns (1) and (2) of Table 7, provide strong support for this hypothesis with respect to casual employment but not fixed-term contract employment.

Next, we analyzed the differential impact of contingent employment across high and low-income groups, using median equivalised disposable household income as the threshold. The results, reported in columns (3) and (4) of Table 7, reveal a modest but significant difference in the impact of fixed-term contract employment: Fixed-term contracts have a slightly larger negative impact on the birth outcomes for men from high-income households. In contrast, the impact of casual employment on fertility differs very little between men from low- and high-income households.

We next classified individuals into low- and high-occupation status groups using the Australian Socioeconomic Index 2006 (McMillan et al. 2009). The index is a continuous measure ranging from 0 (low status) to 100 (high status) and can be applied to all types of employment, with a low-status occupation defined here as any occupation with an index lower than 50. Persons not currently employed were assigned to an occupation based on their most recent previous job. As shown in columns (5) and (6) of Table 7, results are mixed. Casual employment has a much larger negative impact on the fertility of men in low-occupation status jobs whereas the reverse is true of fixed-term contracts.

Results for the final three dimensions considered – country of birth, age and relationship status (i.e., partnered versus single) – are presented in the lower half of Table 7, in columns (7) through (12). As expected, we find that contingent employment matters much for the fertility choices of Australia-born, of younger people, and of those living in a relationship. In the latter case, however, the difference is only significant for those employed on a fixed-term contract.

Conclusions

At first glance, the results presented here, at least for men, seem to be entirely consistent with most previous research (as reflected in the meta-analysis of Alderotti et al. 2021), with both fixed-term and casual employment found to be strongly correlated in a negative direction with the probability of first birth. At the same time, however, our findings also suggest that most previous research findings are likely biased.

There are very good reasons to think that employment status is endogenous with respect to fertility intentions and subsequent outcomes. Despite this, past research investigating the associations between contingent forms of employment has usually proceeded as if employment status is exogenous. The analysis presented here explicitly rejects this assumption and attempts to deal with endogeneity through the use of instrumental variables. The key finding is that conventional OLS estimates are inconsistent and that the magnitude of the ‘true’ negative impact of contingent employment on fertility is typically much larger. This was most obvious with respect to fertility outcomes for men. Estimation of a simple linear probability model with OLS resulted in a coefficient on fixed-term contract employment that was insignificant and very small (effectively zero) and a coefficient on casual employment that, while statistically significant, was still quite small. In contrast, IV estimation generated large negative coefficients of -0.184 and -0.097 respectively.

That the effect of fixed-term contract employment was found to be almost as twice as large as the effect of casual employment, however, was surprising. If the main channel through which contingent employment influences fertility is increased job insecurity, then casual employment, where working hours can be varied at any time without notice, should have had much larger effects. One possible explanation lies in the difference between regulation and practice. While under Australian industrial regulations any casual employee could find themselves not being required to attend work the following day, the reality is that many workers hired on a casual basis work the same pattern of hours over very long periods, leading to the presence of what some have labeled the ‘permanent casual’ (Campbell et al. 2019). It thus may be that the lived experience for many casual workers is not as insecure as the contractual terms of employment suggest. Furthermore, it may be that for many workers the open-ended nature of casual employment relative to the finite length of fixed-term contracts may have less of a delaying effect on fertility, and this is especially so given fixed-term contracts in Australia tend to be quite long (two years is common).

IV estimates, however, depend critically on the validity of the instruments selected. We subjected our chosen instruments to a range of checks and are comfortable that the instruments satisfy the necessary criteria for men. Especially critical is the requirement that the instruments only affect the outcome variable through the endogenous variables. While there is no formal test for this exclusion assumption, we argue that for it to hold the instruments should only be correlated with birth outcomes of the contingent employees and not of the permanent employees. We found this condition to hold for men but not women, suggesting that the instruments may not have adequately dealt with endogeneity in the case of women. For this reason we argue that little weight should be accorded to our results for women – as for men, the OLS results for women are found to be inconsistent, but we cannot be confident that the IV estimates are superior. This is especially so for fixed-term contract employment, with its instrument found to be strongly associated with the fertility of females in permanent jobs.

Another key finding of this analysis is that the magnitude of the association between our two types of contingent employment and fertility is not uniform and varies with education, occupational status, country of birth, age, and relationship status. Choice of discriminators was driven by theoretical considerations, but we speculate that estimated effect sizes would likely differ on many other worker characteristics that were not considered.

While we argue that the longitudinal data source used is well suited to analyzing fertility outcomes and the role that contingent forms of employment play in influencing those outcomes, the analysis is not without limitations. First, like all longitudinal surveys, and despite the relatively high annual re-interview rates, there is the potential for non-random attrition. That said, as a crude check of how important this might be for our analysis we correlated fertility intentions at time t with a dummy for survey participation at $t+1$. The correlation was close to zero (r=.007), which suggests that sample attrition is not associated with births. Second, while the study began with a sample that was broadly representative of the Australian population living in private dwellings, over time it becomes less representative due to the absence of any automatic mechanism for recruiting immigrants. This was redressed in 2011 through the addition of a population-wide refreshment sample, but the problem immediately re-emerges – unless they join a sample household, immigrants arriving in Australia after 2011 have no chance of being included in the sample. Third, while all analyses distinguish between men and women, theoretical considerations suggest we should have gone further and, in the case of persons living in couple relationships, distinguished between primary and secondary earners. Sample size considerations, however, likely mean that the power of such analysis would be poor, but this will become more feasible in the future as more waves are conducted and hence more observations made.

From a policy perspective perhaps the most important finding is the large negative impact of fixed-term contracts for men, especially given this type of employment has received far less attention from both policy-makers and trade union activists than casual employment. Thus while regulations surrounding the hiring of casual work were substantially tightened in 2021 as part of amendments to the *Fair Work Act* (the major government statute regulating employment of workers in Australia), no significant changes have been made to the conditions governing the employment of workers on fixed-term contract. Furthermore, whereas the incidence of casual employment has changed little in the last two decades in Australia, the share of fixed-term contracts has been steadily rising (Laß and Wooden 2020).

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Table 1 Variable definitions and summary statistics

Variable	Definition	Men		Women	
		Mean	SD	Mean	SD
Outcome variable					
First birth	Birth of child observed between t+1 and t+4	0.185	0.388	0.218	0.413
Employment status					
Permanent	Employee employed on a permanent or ongoing basis [reference group]	0.557	0.497	0.542	0.498
Fixed-term	Employee employed on a fixed-term contract	0.088	0.283	0.102	0.302
Casual	Employee employed on a casual basis	0.204	0.403	0.255	0.436
Self-employed	Works in own business	0.084	0.278	0.042	0.200
Other employed	Works without pay in a family business or an employee whose contractual status could not be determined	0.005	0.074	0.004	0.061
Unemployed	Not employed but actively looked for work in last four weeks and was available to start work	0.035	0.183	0.022	0.147
Not in the labor force	Not employed and either not actively looking for work or not available to start work	0.027	0.162	0.034	0.181
Instrumental variables					
Fixed-term contract rate	Fixed-term contract rate by industry	0.079	0.034	0.087	0.040
Casual employment rate	Casual employment rate by industry	0.218	0.137	0.235	0.151
Control variables					
Employment status of partner					
Partner permanent	Has partner who is an employee employed on a permanent or ongoing basis [reference group]	0.232	0.422	0.284	0.451
Partner fixed-term	Has partner who is an employee employed on a fixed-term contract	0.042	0.201	0.040	0.195
Partner casual	Has partner who is an employee employed on a casual basis	0.051	0.220	0.042	0.200
Partner self-employed	Has partner who works in own business	0.018	0.133	0.056	0.230
Partner other employed	Has partner who works without pay in a family business or is an employee whose contractual status could not be determined	0.002	0.041	0.002	0.043
Partner unemployed	Has partner who is not employed but actively looked for work in last four weeks and was available to start work	0.014	0.119	0.011	0.104
Partner not in the labor force	Has partner who is tot employed and either not actively looking for work or not available to start work	0.033	0.179	0.016	0.126
Age					
18-22	Equals 1 if aged 18 to 22 years and zero if otherwise [reference group]	0.301	0.459	0.329	0.470
23-27	Equals 1 if aged 23 to 27 years and zero if otherwise	0.264	0.441	0.280	0.449
28-32	Equals 1 if aged 28 to 32 years and zero if otherwise	0.193	0.394	0.190	0.392
33-37	Equals 1 if aged 33 to 37 years and zero if otherwise	0.113	0.316	0.097	0.296
38-42	Equals 1 if aged 38 to 42 years and zero if otherwise	0.085	0.279	0.067	0.250
43-45	Equals 1 if aged 43 to 45 years and zero if otherwise	0.044	0.206	0.036	0.187
Education					
Year 11 and below	Equals 1 if highest educational attainment is Year 11 or lower [reference group]	0.182	0.386	0.104	0.305
Year 12	Equals 1 if highest educational qualification is completing high school (i.e., Year 12)	0.305	0.460	0.312	0.463

Diploma / Certificate	Equals 1 if highest educational qualification is a diploma or Level III or IV certificate	0.285	0.451	0.244	0.429
Degree	Equals 1 if highest educational qualification is bachelor's degree or higher-level qualification	0.228	0.420	0.340	0.474
Relationship status					
Single	Equals 1 if not married or living with someone in a relationship [reference group]	0.608	0.488	0.549	0.498
Cohabiting	Equals 1 if not married or living with someone in a relationship	0.167	0.373	0.190	0.392
Married	Equals 1 if married	0.226	0.418	0.260	0.439
Country of birth					
Australia	Equals 1 if born in Australia [reference group]	0.852	0.355	0.866	0.341
Overseas: English-speaking	Equals 1 if born overseas in one of the main English-speaking countries (Canada, Ireland, New Zealand, South Africa, UK and USA)	0.063	0.242	0.048	0.213
Overseas: Other	Equals 1 if born overseas in a country other than one of the main English-speaking countries	0.085	0.279	0.087	0.281
Long-term health condition	Equals 1 if has health condition or disability that restricts everyday activity and has lasted or is likely to last, for 6 months or more	0.117	0.321	0.140	0.347
Ln household income	Log of equivalized real disposable household income during the previous financial year (year ending 30 June)[a]	9.042	0.872	9.070	0.874
Non-positive income	Equals 1 if household income is zero or negative	0.001	0.029	0.001	0.030
Controls used in supplementary specifications					
Fertility intentions	Score on 0 to 10 scale indicating how strongly respondents feel about having a child in the future	6.750	3.210	7.023	3.255
Workplace entitlements	Count of the number of six different types of family-friendly workplace entitlements respondent has access to [employed only]	1.523	1.266	1.663	1.269
Unionization rate	Weighted number of members of trade unions and employee associations as proportion of total industry employment (where industry is represented by the 19 major ANZSIC divisions)	0.195	0.111	0.227	0.137
Egalitarian views	Equals 1 if score on a 7-item index measuring attitudes to mothering and the household division of labor is below the population median	0.506	0.500	0.713	0.452

Note: [a] The equivalence scale used is the OECD modified scale, which assigns a weight of 1.0 for the first adult in the household, 0.5 for every other adult, and 0.3 for every child

Table 2 Contingent employment and first birth – Main results

	Men		Women	
Variable	OLS (1)	IV (1)	OLS (1)	IV (1)
Fixed-term contract	-0.003	-0.184**	0.004	0.226**
	(0.009)	(0.065)	(0.010)	(0.067)
Casual	-0.019**	-0.097**	-0.055**	-0.046
	(0.006)	(0.031)	(0.007)	(0.030)
R-squared	0.239	0.221	0.241	0.216
F-statistic	287.6	252.3	287.1	245.8
Observations	19,370	19,370	17,420	17,420

Notes: Numbers in parentheses are standard errors. All equations include the full set of controls listed in Table 1, as well as state and wave (year) dummies. Full results are presented in Appendix Table A1.
** $p < .01$

Table 3 Contingent employment and first birth – First stage IV results

	Men		Women	
Variable	Fixed-term contract	Casual	Fixed-term contract	Casual
Fixed-term contract rate by industry	1.366**	0.024†	1.337**	0.459**
	(0.090)	(0.065)	(0.090)	(0.092)
Casual employment rate by industry	0.023	0.721**	-0.051*	0.789**
	(0.018)	(0.026)	(0.019)	(0.026)
Observations	19,370	19,370	17,420	17,420
Weak identification test (Kleibergen-Paap Wald rk F statistic)[a]	135.15		134.57	
Endogeneity test (Durbin–Wu–Hausman F test: prob > F)	.004		.002	

Notes: Numbers in parentheses are standard errors. All equations include the full set of controls listed in Table 1, as well as state and wave (year) dummies.
[a] The critical value of the F-test, from Stock and Yogo (2002), is 16.38.
† $p < .10$; * $p < .05$; ** $p < .01$

Table 4 Placebo test of exclusion restriction (outcome variable is firth birth; sample restricted to permanent employees)

Variable	Men	Women
Fixed-term contract rate by industry	-0.188	0.635**
	(0.138)	(0.147)
Casual employment rate by industry	-0.014	0.062
	(0.035)	(0.038)
R-squared	0.136	0.149
F-statistic	344.9	423.2
Observations	11,080	9,881

Notes: Results from OLS estimation. Numbers in parentheses are standard errors. All equations include the full set of controls listed in Table 1, as well as state and wave (year) dummies.
** $p < .01$

Table 5 Contingent employment and fertility consequence (Men) – Impact of additional control variables

Variable	(1)	(2)	(3)	(4)	(5)
Fixed-term contract	-0.184**	-0.200**	-0.191**	-0.341**	-0.177**
	(0.065)	(0.074)	(0.071)	(0.095)	(0.065)
Casual	-0.097**	-0.096**	-0.074*	-0.078*	-0.093**
	(0.031)	(0.034)	(0.037)	(0.031)	(0.031)
Fertility intentions		0.026**			
		(0.001)			
Workplace entitlements			-0.004		
			(0.003)		
Unionization rate				0.112**	
				(0.036)	
Egalitarian views					-0.014**
					(0.005)
R-squared	0.221	0.265	0.236	0.184	0.223
F-statistic	252.3	250.1	225.5	197.5	247.7
Observations	19,370	15,354	14,356	19,370	19,020

Notes: Results from IV estimation. Numbers in parentheses are standard errors. All equations include the full set of controls listed in Table 1, as well as state and wave (year) dummies.
* $p < .05$; ** $p < .01$

Table 6 Robustness test results (Men)

Variable	Baseline (1)	Outcome window: Two to three years (2)	Outcome window: Two to five years (3)	Higher parity births (4)	State and time interaction (5)
Fixed-term contract	-0.184**	-0.101*	-0.279**	0.042	-0.180**
	(0.065)	(0.051)	(0.078)	(0.124)	(0.064)
Casual	-0.097**	-0.062**	-0.159**	-0.130	-0.095**
	(0.031)	(0.023)	(0.036)	(0.113)	(0.029)
R-squared	0.221	0.162	0.255	0.187	0.223
F-statistic	252.3	161.7	285	405.9	227.1
Observations	19,370	21,453	16,854	18,570	19,370

Notes: Results from IV estimation. Numbers in parentheses are standard errors. All equations include a full set of controls. In column (4), we additionally control for number of children.
* $p < .05$; ** $p < .01$

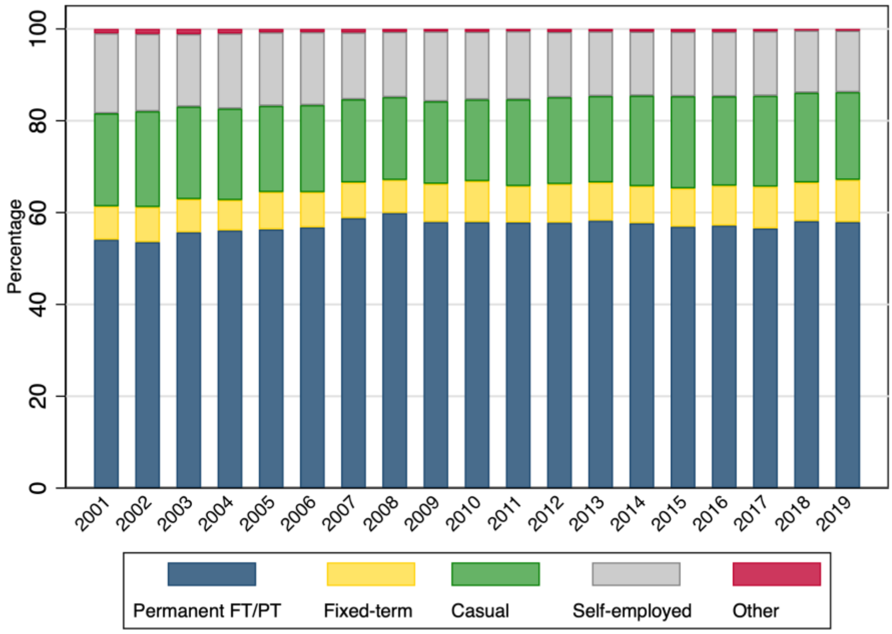
Table 7 Contingent employment and fertility consequence (Men) – Heterogeneity analysis

Variable	Low education (1)	High education (2)	Low income (3)	High income (4)	Low occupation status (5)	High occupation status (6)
Fixed-term contract	-0.153	-0.208*	-0.150	-0.182*	0.016	-0.275**
	(0.104)	(0.083)	(0.104)	(0.081)	(0.144)	(0.104)
Test for equality of coefficients *(p-value)*	.338		<.001		.035	
Casual	-0.122**	-0.046	-0.099*	-0.089*	-0.096*	0.051
	(0.040)	(0.049)	(0.047)	(0.037)	(0.044)	(0.114)
Test for equality of coefficients *(p-value)*	<.001		.104		<.001	
R-squared	0.159	0.225	0.160	0.265	0.214	0.207
F-statistic	74.67	168.6	60.59	201.3	72.09	42.26
Observations	9,438	9,932	8,805	10,565	12,793	6,566
Variable	Australia born (7)	Other countries (8)	Aged 18-25 (9)	Aged 26-45 (10)	Single (11)	Cohabiting or married (12)
Fixed-term contract	-0.245**	0.042	-0.203**	-0.170†	-0.006	-0.382**
	(0.071)	(0.151)	(0.075)	(0.101)	(0.060)	(0.126)
Test for equality of coefficients *(p-value)*	<.001		.697		<.001	
Casual	-0.115**	0.009	-0.113**	-0.056	-0.051*	-0.160
	(0.031)	(0.081)	(0.033)	(0.073)	(0.023)	(0.108)
Test for equality of coefficients *(p-value)*	<.001		<.001		.105	
R-squared	0.219	0.196	0.162	0.222	0.045	0.104
F-statistic	193.6	185.7	55.39	204	573.8	86.41
Observations	16,505	2,865	9,069	10,301	11,770	7,600

Notes: Results from IV estimation. Numbers in parentheses are standard errors. All equations include the full set of controls listed in Table 1, as well as state and wave (year) dummies.
† $p < .10$; * $p < .05$; ** $p < .01$

Appendix (Supplementary Material)

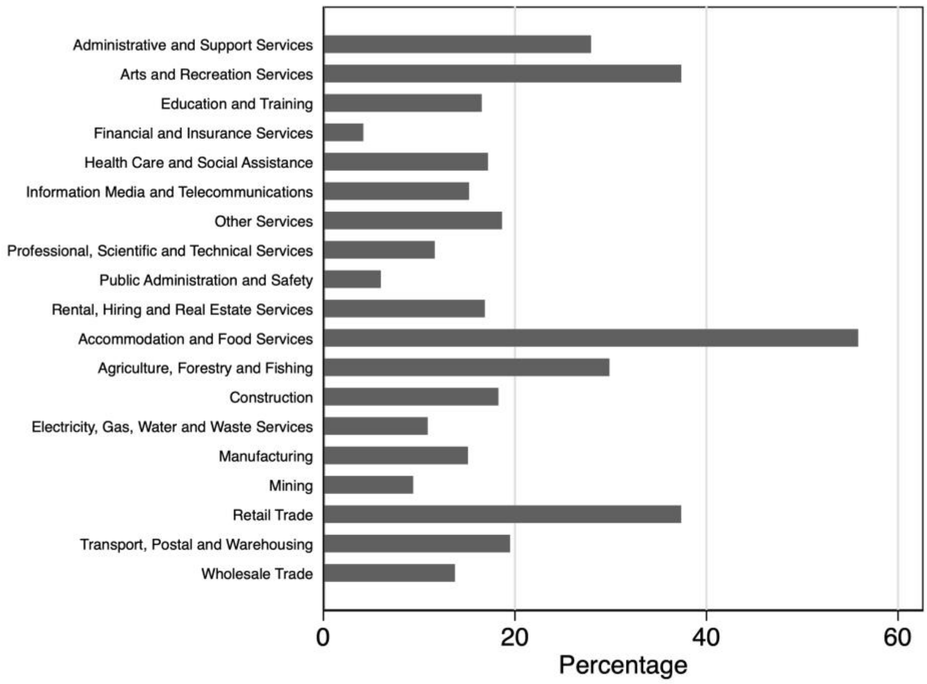
Figure A1 Trends in employment types, Australia, 2001-2019 (% of all employed persons)



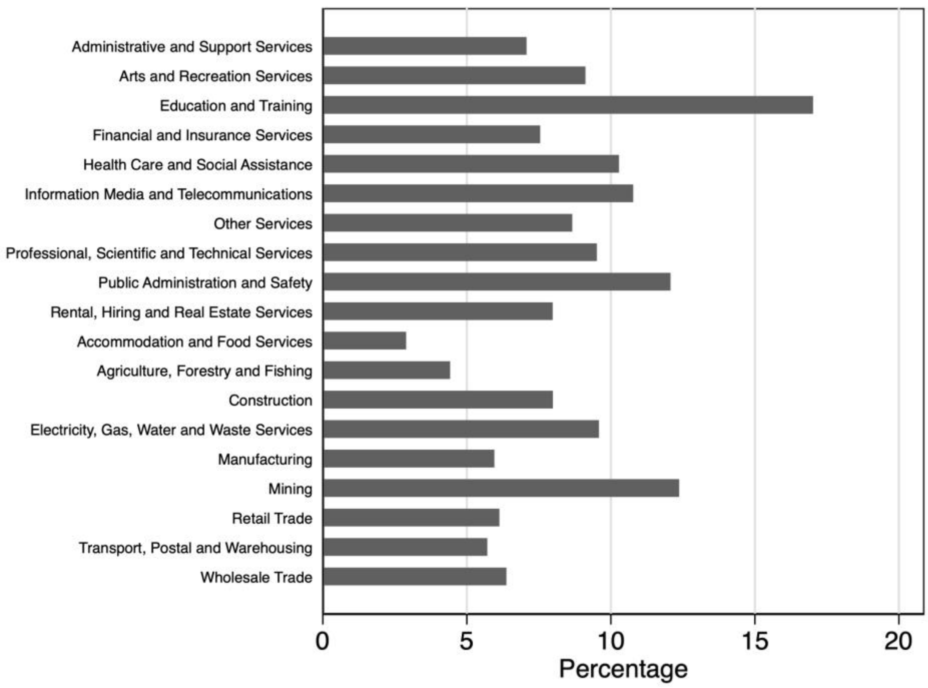
Source: HILDA Survey, Release 19 (doi:10.26193/3QRFMZ).

Figure A2 Share of contingent employment by industry

Panel A: Share of casual employment



Panel B: Share of fixed-term contract



Source: HILDA Survey, Release 19 (doi:10.26193/3QRFMZ).

Table A1 Contingent employment and fertility consequence – Results with controls

Variable	Men OLS (1)	Men IV (2)	Women OLS (3)	Women IV (4)
Fixed-term contract	-0.003	-0.184**	0.004	0.226**
	(0.009)	(0.065)	(0.010)	(0.067)
Casual	-0.019**	-0.097**	-0.055**	-0.046
	(0.006)	(0.031)	(0.007)	(0.030)
Self-employed	0.024*	-0.007	-0.045**	-0.018
	(0.010)	(0.013)	(0.015)	(0.018)
Others	0.036	-0.005	-0.015	0.007
	(0.032)	(0.036)	(0.049)	(0.047)
Unemployed	-0.006	-0.051*	-0.017	0.008
	(0.012)	(0.020)	(0.019)	(0.025)
Not in the labor force	-0.013	-0.059**	-0.017	0.007
	(0.012)	(0.022)	(0.014)	(0.022)
Partner employment type (Ref. Permanent)				
Fixed-term contract	-0.008	0.003	0.005	-0.011
	(0.018)	(0.014)	(0.019)	(0.016)
Casual	-0.122**	-0.116**	-0.090**	-0.090**
	(0.015)	(0.012)	(0.017)	(0.015)
Self-employed	-0.085**	-0.079**	0.020	0.021
	(0.025)	(0.019)	(0.016)	(0.013)
Others	-0.143*	-0.137*	0.091	0.096
	(0.067)	(0.062)	(0.082)	(0.064)
Unemployed	-0.065*	-0.061**	-0.054†	-0.063*
	(0.028)	(0.021)	(0.032)	(0.027)
Not in the labor force	-0.098**	-0.092**	-0.093**	-0.105**
	(0.018)	(0.015)	(0.025)	(0.023)
Age groups (Ref: 18-22)				
23-27	0.025**	0.010	0.026**	0.023*
	(0.007)	(0.009)	(0.008)	(0.010)
28-32	0.086**	0.063**	0.107**	0.106**
	(0.009)	(0.012)	(0.010)	(0.012)
33-37	0.031**	0.007	0.026*	0.021
	(0.010)	(0.013)	(0.012)	(0.014)
38-42	-0.086**	-0.111**	-0.159**	-0.156**
	(0.009)	(0.014)	(0.011)	(0.016)
43 and older	-0.160**	-0.188**	-0.205**	-0.200**
	(0.010)	(0.016)	(0.010)	(0.019)
Education level (Ref: Year 11 and below)				
Year 12	-0.013†	-0.009	-0.021*	-0.022*
	(0.007)	(0.008)	(0.010)	(0.010)
Vocational	0.024**	0.023**	-0.000	-0.005
	(0.008)	(0.008)	(0.011)	(0.011)
Bachelor and higher	0.022**	0.029**	0.006	-0.013
	(0.008)	(0.009)	(0.011)	(0.012)

Marital status (Ref. Single)				
Married	0.377**	0.369**	0.343**	0.350**
	(0.010)	(0.009)	(0.010)	(0.010)
Cohabiting	0.232**	0.226**	0.215**	0.215**
	(0.009)	(0.008)	(0.009)	(0.009)
Country of birth (Ref. Australia)				
Overseas: English-speaking	-0.007	-0.006	-0.005	-0.007
	(0.011)	(0.010)	(0.014)	(0.013)
Overseas: Other	-0.020*	-0.016†	-0.066**	-0.063**
	(0.009)	(0.009)	(0.010)	(0.010)
Other controls				
Long-term health condition	-0.021**	-0.015†	-0.021**	-0.022**
	(0.007)	(0.008)	(0.008)	(0.008)
Log equivalized real household income	0.002	-0.001	-0.002	-0.002
	(0.003)	(0.004)	(0.003)	(0.004)
Non-positive income	-0.087	-0.112	0.004	0.017
	(0.057)	(0.088)	(0.088)	(0.093)
R-squared	0.239	0.221	0.241	0.216
F-statistic	287.6	252.3	287.1	245.8
Observations	19,370	19,370	17,420	17,420

Notes: Results from IV estimation. Numbers in parentheses are standard errors. All equations also include state and wave (year) dummies.

† $p < .10$; * $p < .05$; ** $p < .01$

Table A2 Contingent employment and fertility consequence (Women) – Impact of additional control variables

Variable	(1)	(2)	(3)	(4)	(5)
Fixed-term contract	0.226**	0.167*	0.242**	-0.065	0.221**
	(0.069)	(0.074)	(0.076)	(0.106)	(0.069)
Casual	-0.046	-0.021	-0.058	-0.028	-0.048†
	(0.029)	(0.032)	(0.038)	(0.028)	(0.029)
Fertility intentions		0.032**			
		(0.001)			
Workplace entitlements			-0.008†		
			(0.004)		
Unionization rate				0.148**	
				(0.037)	
Egalitarian views					-0.038**
					(0.007)
R-squared	0.216	0.278	0.221	0.240	0.219
F-statistic	245.8	264.1	215	268.9	241.3
Observations	19,370	15,354	14,356	19,370	19,020

Notes: Results from IV estimation. Numbers in parentheses are standard errors. All equations include the full set of controls listed in Table 1, as well as state and wave (year) dummies.
† $p < .10$; * $p < .05$; ** $p < .01$

Table A3 Robustness test results (Women)

		Outcome window			
Variable	Baseline (1)	Two to three years (2)	Two to five years (3)	Higher parity births (4)	State and time interaction (5)
Fixed-term contract	0.226**	0.184**	0.283**	-0.093	0.226**
	(0.067)	(0.058)	(0.078)	(0.082)	(0.069)
Casual	-0.046	0.002	-0.107**	-0.075*	-0.046
	(0.030)	(0.024)	(0.034)	(0.035)	(0.029)
R-squared	0.216	0.145	0.255	0.247	0.217
F-statistic	245.8	145.5	290.4	460.3	221.9
Observations	17,420	19,282	15,370	17,843	17,420

Notes: Results from IV estimation. Numbers in parentheses are standard errors. All equations include the full set of controls. In column (4), we additionally control for number of children.
* $p < .05$; ** $p < .01$

Table A4 Contingent employment and fertility consequence (Women) – Heterogeneity analysis

Variable	Low education (1)	High education (2)	Low income (3)	High income (4)	Low occupation status (5)	High occupation status (6)
Fixed-term contract	0.182	0.267**	0.273*	0.211*	0.153	0.305
	(0.151)	(0.079)	(0.106)	(0.089)	(0.162)	(0.192)
Test for equality of coefficients *(p-*value)	<.001		.517		.139	
Casual	0.010	-0.121**	-0.023	-0.062	-0.077†	-0.068
	(0.042)	(0.045)	(0.041)	(0.041)	(0.041)	(0.204)
Test for equality of coefficients *(p-*value)	<.001		.104		<.001	
R-squared	0.195	0.199	0.139	0.264	0.200	0.202
F-statistic	82.26	161.3	61.22	194.5	135.4	117
Observations	7,249	10,171	7,651	9,769	9,942	7,469
Variable	**Australia born (7)**	**Other countries (8)**	**Aged 18-25 (9)**	**Aged 26-45 (10)**	**Single (11)**	**Cohabiting or married (12)**
Fixed-term contract	0.254**	0.089	-0.028	0.449**	0.175*	0.212†
	(0.075)	(0.183)	(0.084)	(0.112)	(0.076)	(0.109)
Test for equality of coefficients *(p-*value)	<.001		<.001		.006	
Casual	-0.025	-0.184*	-0.037	-0.173*	0.026	-0.197**
	(0.031)	(0.083)	(0.033)	(0.069)	(0.026)	(0.075)
Test for equality of coefficients *(p-*value)	<.001		<.001		<.001	
R-squared	0.219	0.194	0.220	0.124	0.058	0.140
F-statistic	210.1	460.2	80.71	117.2	201.4	121.7
Observations	15,082	2,338	8,831	8,589	9,572	7,848

Notes: Results from IV estimation. Numbers in parentheses are standard errors. All equations include the full set of controls listed in Table 1, as well as state and wave (year) dummies.
† $p < .10$; * $p < .05$; ** $p < .01$