

household members, we can identify the relationship of each child (biological, step, foster, or adopted) to their mother and their father. The information on disability status of children is also available.

Although it has never been exploited, the SIPP has a number of advantages for studying adoption demand. First, its large sample sizes and frequent waves provide us with a sufficient number of observations of adoptive parents to conduct multivariate analysis. Second, the data allow us to distinguish different types of adoption, i.e., unrelated adoption, related adoption, stepparent adoption, foster care adoption, and the adoption of disabled children.<sup>29</sup> Third, in contrast to the NSFG whose focus is women of childbearing age, the SIPP is more representative of the population, including single men as well as older women who are also part of adoptive parents. Fourth, while the NSFG has better data on women's reproductive health and fertility history, the SIPP contains better data on economic characteristics in general.

In short, the two datasets, NSFG and SIPP, complement each other in many important aspects. By using both sources to estimate adoption demand, we can address a wider set of questions, and to the extent that the two datasets overlap, we are able to cross-examine reliability of the data and robustness of our findings.

Due to low adoption and relinquishment rates in general, in both datasets, the sample size of adoptive or relinquishing mothers in any single-year sample is very small. By pooling data across all cycles or waves, however, we obtain 677 adoptive mothers and 208 relinquishing mothers in the NSFG, and 1,739 adoptive mothers and 573 relinquishing mothers in the SIPP. As a result, unlike previous work that used single-year NSFG data to study adoption demand or supply, we can implement a multivariate regression to estimate individual's propensity to adopt a child or relinquish a child for adoption. To guide the choice of explanatory variables included in a regression, we consider competing hypotheses that can explain why individuals adopt, rather than bear, children, or why individuals give up their biological children for adoption rather than keeping them.

For the demand analysis, we consider (1) the *infertility hypothesis* that points to individuals' difficulty in producing their own biological children (proxy variables: sterility status, use of

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<sup>29</sup> We can only partially identify inter-country adoption in SIPP data.

infertility treatment, the absence of biological children), (2) the *opportunity cost hypothesis* that emphasizes the high opportunity cost of childbearing for women pursuing a career as the primary reason for adoption (proxy variables: employment status, occupation, work experience), (3) the *humanitarian hypothesis* that highlights altruistic motives, particularly in adopting children with special needs (proxy variables: religious affiliation, charity donation), and (4) the *resource constraint hypothesis* that emphasizes time, financial resources, and psychological support to undertake costly adoption (proxy variables: woman's work status, family income, assets, marital status).

Clearly, these hypotheses are not mutually exclusive and can jointly impact individuals' decision to adopt. With the multivariate analysis, we can estimate the marginal effects of each proxy variable and evaluate the relative importance of different motivations in determining adoption. It also enables us to examine interactive effects of certain variables. In particular, we use two empirical strategies exploiting both across-time and across-group variations in the data. First, we include dummies for adoption types and their interactions with certain explanatory variables in order to examine variations in the relative importance of the chosen variables across adoption types. For example, under the humanitarian hypothesis, the religion variables would be more important (relative to other socio-economic characteristics) for foster care adoption or the adoption of disabled children than for other types of adoption. Second, we include time dummies and their interactions with certain variables to examine differential time changes in the coefficients associated with those variables. For example, under the opportunity cost hypothesis, increasing returns to education and work experience for women in the last thirty years would increase the opportunity cost of childbearing, thus the importance of the labor market attachment variables (relative to other socio-demographic characteristics) would increase over time.

## **6.2 Preliminary Results from NSFG Data**

**Table 5** presents sample definition, sample size, and the numbers of adoptive mothers and adopted children by type of adoption in each cycle of the NSFG. The 1973 and 1976 samples include only ever-married women of childbearing age, in contrast to all women of childbearing age in the subsequent cycles. Due to changes in the wording of survey questions, types of adopted children available in the sample are different across years. Most importantly, for the 1973-1988 samples, adoption may include not only formal but also informal (not legally approved) adoption, while for the 1995 and 2002 samples women are explicitly asked to exclude informal adoption. Similarly, for

the 1973 sample women are asked to exclude stepchild adoption.<sup>30</sup> From 1982 and on, we have information on related and unrelated adoption, and from 1988 and on, we have information on public agency adoption. In the pooled sample, we have a total of 677 adoptive mothers and 903 adopted children. On average, 68% of adoptions in the sample are unrelated adoption, 12% are stepparent adoption,<sup>31</sup> and 35% are public agency adoption. 54% of adoptive mothers have both biological and adopted children, while 46% have only adopted children.

**Table 6** reports descriptive statistics comparing the observed characteristics of mothers by type of mother (i.e., biological, adoptive, and relinquishing mothers) for 1973, 1988, and 2002.<sup>32</sup> (The observed characteristics of adoptive and relinquishing mothers for all years are reported in **Tables 7 and 8**.) Compared to all women who have given birth to at least one child (i.e., biological mothers), women who have adopted at least one child (i.e., adoptive mothers) are more likely to be white, married or cohabitating with a partner, have received some college education, and have higher household income, all at the time of the interview. Adoptive mothers are also more likely to be (surgically or non-surgically) sterile at the time of the interview, have experienced a miscarriage, have received infertility treatment, have given the first birth at higher age, and have been a foster parent. In addition, adoptive mothers are more likely to work full time in 1988 and 2002 and less likely to participate in welfare programs at the time of the interview. Compared to all biological mothers, women who have given up at least one child for adoption (i.e., relinquishing mothers) are more likely to be white, have lower educational attainment, either unmarried or married more than once, have lower household income, and have fewer children living in the household, all at the time of the interview. Relinquishing mothers are also more likely to have had an (induced) abortion, had first intercourse at lower age, given first birth at lower age, and had at least one child out of wedlock.

We now present preliminary results from the logit regression analysis using NSFG data. The results on adoption demand are presented in **Tables 9-16**, and the results on adoption supply are presented in **Tables 17 and 18**.

In **Table 9**, we perform a basic regression for single-year samples and a pooled sample, including a

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<sup>30</sup> [Provide the exact wording of the questions here.]

<sup>31</sup> The share of stepparent adoption is small in our sample because women adopt far less stepchildren than men.

<sup>32</sup> Due to changes in sampling methods and definitions, these statistics are not perfectly comparable across years.

set of explanatory variables available across all years. The numbers reported in the table are marginal effects (MEs) of different variables on the propensity to adopt evaluated at mean values and expressed in %. In the pooled regression presented in the last column, the mean propensity to adopt is 1.34%. Controlling for the age of respondent, the propensity to adopt increases by 0.52 percentage point to 1.86% if a women is sterile, by 0.27 percentage point if she is married or cohabitating with a partner, by 0.27 percentage point if she has experienced a spontaneous pregnancy loss, by 0.30 percentage point if she is Catholic, and by 0.21 percentage point if she is Protestant. The effects of education and the number of marriages are also positive but small. By contrast, the propensity to adopt decreases by 0.34 percentage point with an additional number of births and by 0.26 percentage point if a women is working full-time.<sup>33</sup> It is interesting to note that the coefficient for full-time work becomes less negative and significant over time, eventually turning to positive (albeit not significant) in the 2002 sample. This is at least consistent with the opportunity cost hypothesis. Similarly, both the magnitude and significance of the coefficients for education and sterility fall over time, possibly indicating increasing diversity among adoptive mothers.

Immediate caveats are in order in interpreting the results. First, given the excess demand in the market for adoptable infants, the results may be capturing supply-side, rather than demand-side, factors, most notably, preferences of private agencies who selects adoptive parents among many applicants. Namely, it could be that sterile, married, or religiously affiliated women are more likely to be matched with adoptable children due to agency preferences. Second, even if the results are identifying demand-side factors, because we are pooling different types of adoption, as our discussion in the previous section suggests, the results likely mask heterogeneity of adoption demand across types. For example, it may be that income has no significance because the effect of higher income women adopting healthy infants and the effect of lower income women adopting foster care children with state subsidies are confounded.

In **Table 10**, we present regression results with richer explanatory variables. (For comparison, the pooled regression from the previous table is reproduced in the first column.) By including additional controls for fertility history and foster parent, the fitness of the model, measured by the pseudo R-squared, improves substantially from the first to second column. In the second column,

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<sup>33</sup> In contrast to previous studies, we find that race and household income have no significant effects on the propensity to adopt. This may be due to the fact that race, income, and marital status are highly collinear.

we find that the propensity to adopt also increases with age at first birth, having no biological children (parity=0), and a shorter interval between the first and last births. In the third column using the pooled 1988-2002 sample, having been a foster parent has by far the largest effect on the likelihood of adoption, increasing the mean propensity of 0.96% by 7.02 percentage points to 7.98%. The same regression also shows that having had an abortion would reduce the propensity to adopt by 0.12 percentage point, while having received infertility treatment would increase the propensity by 0.87 percentage point even after controlling for sterility status. If we believe that infertility treatment is a substitute for adoption, then we should expect a negative coefficient. But if we take into consideration that receiving infertility treatment is a proxy for fecundity status that is only imperfectly captured by sterility status and that the success rate of infertility treatment is not necessarily high, then the results are not surprising. In the last column using the pooled 1995-2002 sample, the “importance of religion” is positive and statistically significant, while none of the religious affiliations has a significant effect on the propensity to adopt.<sup>34</sup> As we show later, the importance of religion is likely a better proxy for humanitarian motives.

Next, to examine whether adoption demand differs systematically by adoption type, we classify adoptive mothers into those who adopted at least one *related* child and those who adopted at least one *unrelated* child (note that the two groups are not mutually exclusive). Recall that related adoption includes stepparent adoption, adoptions by relatives without involving agencies, and adoptions of foster care children by relatives. Unrelated adoption includes domestic private agency and independent adoptions, inter-country adoption, and foster care adoption. **Table 11** presents the regression results for related and unrelated adoptions using the pooled 1982-2002 and 1988-2002 samples. The table shows systematic qualitative (and quantitative) differences between the two types of adoption. For example, a negative effect of working full-time on the propensity to adopt is present only for unrelated adoption, indicating either women’s binding time constraints or private agency’s preference for non-working women. We also find a positive effect of household income on the propensity to adopt only for unrelated adoption, lending some support to the resource constraint hypothesis. The number of marriages has a positive and significant effect only for related adoption, presumably capturing the effect on stepchild adoption. Being Protestant or Catholic has a positive effect only for unrelated adoption, which is consistent with both the humanitarian hypothesis and the agency preference hypothesis. Furthermore, we find that both delayed motherhood (higher age at first birth) and having no biological children (parity=0) have *positive*

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<sup>34</sup> [Provide the definition of importance of religion here.]

effects on unrelated adoption and *negative* or no effects on related adoption. It seems to confirm the infertility hypothesis for unrelated adoption, but reject the same hypothesis for related adoption. Having received help to get pregnant increases the propensity to adopt for both related and unrelated adoption, however, suggesting that infertility also motivates related adoption (but the magnitude of the effect is smaller). Finally, having been a foster mother is the single most important determinant for both related and unrelated adoption, but the magnitude of the effect is much larger for unrelated adoption.

To study the demand for foster care adoption, in **Table 12**, we divide adoptive mothers between “foster” adoptive mothers (i.e., adoptive mothers who have even been a foster mother) and “non-foster” adoptive mothers (i.e., the rest of adoptive mothers) and compare regression results.<sup>35</sup> Due to the small number of foster adoptive mothers in the pooled 1988-2002 sample, much of our results are not statistically significant. Nonetheless, the table shows that the effects of sterility and receiving infertility treatment are much larger for non-foster adoptive mothers. Although not statistically significant, the effects of religious affiliation are positive for non-foster adoptive mothers while negative for foster adoptive mothers.

To further examine the heterogeneity of demand across types, we divide adoptive mothers between those who have *both* adopted and biological children and those who have *only* adopted children. The former group is more likely to include adoptive mothers who are motivated by reasons other than infertility. **Table 13** compares the regression results between the two groups using the pooled 1973-2002 and 1988-2002 samples. It shows that, compared to adoptive mothers who have no biological children, adoptive mothers with both biological and adopted children are more likely to have been a foster mother, less likely to work full-time, have lower household income, have fewer years of education, more likely to have experienced miscarriages, and less likely to have received infertility treatment.

In **Table 14**, to explore differences in the propensity to adopt by race, we interact our base line models with the white indicator variable. The coefficients reported in the first column (labeled “level”) show the marginal effects of the corresponding variables for *nonwhite* women in the pooled 1973-2002 sample, and the coefficients in the second column show the additional marginal effects of the same variables if a women is *white*. That is, the marginal effects for white women are

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<sup>35</sup> Because women can adopt foster care children without becoming a foster mother, “non-foster” adoptive mothers may have a child adopted from foster care.

the sum of the coefficients in the first and second columns. (The third and fourth columns report the results using the pooled 1988-2002 sample.) The results reveal systematic differences across race. Most notably, having low household income increases the propensity to adopt for nonwhite women but decreases that for white women; being Protestant or Catholic has a strong positive effect for white women, but a negative effect for nonwhite women; and working full time has a strong negative effect only for white women.

To address the concerns that the results we obtained so far may be reflecting supply-side factors, we also estimate the determinants of *potential* demand as opposed to fulfilled demand. Potential demand for adoption is measured by two variables: (1) women who are *considering* to adopt a child at the time of the interview, and, among those who are considering to adopt, (2) women who have *taken steps* (e.g., contact an adoption agency, doctor, or lawyer) to adopt a child at the time of interview. These variables are available only in the 1995 and 2002 NSFG cycles. **Table 15** compares the regression results for potential and fulfilled demands. In the pooled 1995-2002 regressions, the mean propensities for women to consider to adopt, to take steps to adopt, and to actually adopt are 27.4%, 3.17%, and 0.88% respectively. The variables whose coefficients remain significant and the same sign across the regressions are: age of respondent, age at last child, sterility status, use of infertility treatment, importance of religion, and being a foster mother. We thus place greater confidence in these variables as identifying demand-side effects. In particular, in our view, the significance of both infertility status and importance of religion in predicting the propensity to adopt is not a reflection of agency preferences, but instead provides some support to the infertility and humanitarian hypotheses. By contrast, the coefficients for full-time work, marital status, and religious affiliation vary widely across the regressions. It is likely that the significance of full-time work and marital status in predicting the fulfilled adoption is either a result of agency preferences or binding resource constraints, while that of religious affiliation can be attributed to the fact that many private adoption agencies are operated by religious organizations.

To assess if the marginal effects of certain covariates changed over time, in **Table 16** we estimate logit models that allow for interactions between covariates and time. (Note that to test if the effects of interacted terms are significantly different from zero, one cannot simply look at t-values but must construct formal statistics given the nonlinearity of the models.) According to our estimates, the negative effect of full-time work declined significantly after 1988. Furthermore, the effect of sterility was significantly higher in 1973-1988 than in 1988-2002, which is consistent with the infertility hypothesis that predicts the advancement in ART would reduce the importance of

infertility in adoption demand.

Finally, we turn to the analysis of adoption supply using relinquishment data.<sup>36</sup> **Table 17** presents the results where we use the sample of biological mothers (i.e., women who gave birth to at least one child). In the pooled 1976-2002 regression, the propensity to relinquish a child for adoption is positively correlated with being white, having low-level household income, the number of marriages, and the number of births. It is negatively correlated with being married or cohabitating with a partner, being Hispanic, being Protestant, working full or part time, participating in welfare, and age at first birth. The results from the pooled 1988-2002 regression are qualitatively similar. Because we observe many of the women's characteristics only at the time of the interview, however, we cannot establish the direction of causality. That is, women's current marital status, household income, employment status, or welfare participation is likely affected by their relinquishment decision in the past. Even more problematic, it is likely that these variables and relinquishment decisions are both correlated with women's unobserved characteristics.

To better control for women's unobserved characteristics, in **Table 18**, we restrict our sample to single mothers (i.e., women who gave births to at least one child out-of-wedlock). In the pooled 1982-2002 sample, conditional on being a single mother, the mean propensity to relinquish a child for adoption is 1.57%. Controlling for the age of respondent, the propensity to relinquish increases by 2.53 percentage point if a woman is white, by 0.31 percentage point if her current household income moves from medium to high level, and by 0.05 percentage point with an additional year of education. By contrast, the propensity to relinquish decreases by 0.43 percentage points if a woman is currently receiving welfare, by 0.37 percentage point if currently married or cohabitating with a partner, by 0.36 percentage point if Hispanic, and by 0.12 percentage point with age at first birth. In contrast to the results in the previous table, conditional on a being single mother, being Protestant or Catholic is no longer negatively correlated with the propensity to relinquish. But even among single mothers, we find that both current marital/cohabitating status and welfare participation are negatively correlated with the propensity to relinquish. Our results likely indicate that single mothers without a stable partner are more likely to relinquish a child, while those single mothers who have relinquished a child is less likely to receive welfare benefits subsequently because they have fewer dependent children. Finally, we find no significant correlations between

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<sup>36</sup> Our analysis is limited, because our data cover only the supply for domestic infant adoption, leaving out the supply for foster care adoption and inter-country adoption which is quantitatively more important in recent years.



the propensity to relinquish and ever having an abortion. One may expect negative correlations between the two if they are considered substitutes.

### 6.3 Preliminary Results from SIPP Data

In **Table 19**, we summarize the numbers and characteristics of biological, adoptive, and relinquishing mothers in SIPP data. We confirm that, compared to biological mothers, adoptive mothers are more educated, have higher family income, are more likely to have no biological children, and more likely to be married but at a higher age. With SIPP data, we also observe that adoptive mothers have longer job experience and are more likely to hold a managerial or professional occupation. By contrast, relinquishing mothers are less educated, less likely to be married and thus have lower family income, are more likely to give a birth before age 20, and receive lower wages.

**Table 20** presents the results from logit models estimating the propensity to adopt using the pooled 1984-2002 data. The results are consistent with what we obtained using NSFG data, showing the large effects of foster parent, infertility (proxied by parity=0), and marital status. In addition, the estimates indicate that family income has positive effects on adoption demand, which is consistent with the resource constraint hypothesis. After controlling for family income, job experience and wage of mother have negative effects on adoption demand.

In **Table 21**, in the first two columns, we divide adoptive mothers between those with at least one biological child and those with no biological children. We find that family income has strong positive effects only on adoptive mothers with no biological children. Full-time work and wage have significant and negative effects only on adoptive mothers with no biological children. In the next two columns, we compare the logit analyses for foster adoptive mothers and non-foster adoptive mothers. Due to the small sample size of foster adoptive mothers, however, the results are mostly inconclusive.

To take advantage of the information in SIPP data on the disability status of children, in **Table 22**, we estimate multinomial logit models with three outcomes: no adoption, adoption of a disabled child, and adoption of a non-disabled child. The reported numbers in the table are the ratios of relative risks (RRR) with the base outcome defined as no adoption. The estimates show some interesting patterns: (1) mother's work status ("full time") reduces the propensity to adopt both

types of children, but the effect is stronger for a child with disability as expected; (2) higher income is associated with a higher propensity of adopting a disabled child, but with a lower propensity of adopting a non-disabled child; (3) marriage is associated with a higher likelihood of both types of adoption over no adoption, but the effect on the adoption of a non-disabled child is far greater; and (4) being a foster mother is also associated with a higher propensity of both types of adoption over no adoption, but the effect on the adoption of a disabled child is much larger. Our formal tests comparing RRRs across adoption types indicate that these differences are statistically significant for work status, marriage, and foster parenthood.

## 7. Concluding Remarks

In this paper, we present adoption trends in the U.S. and empirically investigate the determinants of adoption demand and supply, using a variety of macro and micro data sources and exploiting both cross-sectional and across-time variations. In future work, we plan to develop a theoretical model of adoption-seeking and child relinquishment, based on the empirical findings from this paper, and conduct counterfactual experiments to evaluate the effects of possible adoption-related policies on adoption outcomes.

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## ***Asian Age Reckoning: Marriage and Young Women's Labor Supply***

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Preliminary and Incomplete

## I. Introduction

Marriage is a major barrier to women's participation in the labor market in many countries across different cultures. In South Korea (hereafter, Korea), despite a continuing secular increase in female labor force participation (LFP) in the past three decades, a significant portion of women still quit their job around the time they get married. According to the 2005 Korean Population and Housing Census, the LFP rate is 70.6 percent among childless never-married women aged 25 to 34, while the rate drops to 55.3 percent among childless married women.<sup>1</sup> According to the first wave of the Korean Longitudinal Survey of Women & Family, about 32 percent of married women reported that they quit their job within six months prior to or after marriage. Only 17 percent said that they continued to work until their first childbirth.<sup>2</sup> In contrast, in the United States, getting married in itself seems to have little impact on women's labor supply. According to the 2000 Current Population Survey Merged Outgoing Rotation Groups data, among those women aged 25-34 without own child under age 18 in the household, the LFP rate is 86.6 percent for never-married women, while the rate is 85.1 percent for married women.<sup>3</sup>

In this study, we attempt to estimate the effect of marriage on young women's labor supply in Korea. Following rapid economic growth after the Korean War, the country has been changing in various social and cultural aspects, in particular, regarding the gender role. For instance, despite strong social preference for sons, women's college attendance rates have increased to 77.8 percent in 2003, approaching men's level, 81.5 percent. It seems, however, that the traditional division of labor by gender still exists within households, as women bear a significant portion of housework and child care. According to the 1999 Korean Time Use Survey data, even among dual-earner couples, husbands spend only about 27 minutes on household care per day, while wives spend almost 4 hours.<sup>4</sup> The main objective of this study is to ascertain the extent to which marriage is a barrier to young women's participation in the labor market.

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<sup>1</sup> The LFP rate for married women with children is 27.1 percent.

<sup>2</sup> Among those women who quit their job around marriage, 46.6 percent said that the main reason was to focus on housework, and 30.6 percent said that it was to prepare for childbearing.

<sup>3</sup> The rate drops to 65.5 percent for married women if there is at least one own child under age 18 in the household.

<sup>4</sup> The female-to-male ratio of the average time spent on household care or housework is 2.2 in the United States (2003), 2.8 in Australia (2001), and 2.9 in Germany (2002). The ratio is 8.2 in Korea (1999).

Since marriage and labor supply are jointly decided, selection into marriage on the unobservables makes it difficult to identify the *causal* effect of marriage on labor supply (Wilbert Van Der Klaauw, 1996). Our econometric strategy to identify causality is the instrumental-variable (IV) estimation method. The main contribution of this paper is the discovery of a novel instrumental variable, which is motivated by the unique Asian culture: *Asian Age Reckoning* (AAR). In many Asian countries, people count their age differently than do people in other countries. According to the traditional age system, a new-born baby is already one year old<sup>5</sup> and, afterwards, gains one year at each turn of the calendar year.<sup>6</sup> For example, someone who is born in December will be already two years old two months later in January.

This unique culture creates exogenous variation in marital status according to birth month within school-year cohorts.<sup>7</sup> In Korea, a school year begins in March and ends in February, and children who turn to six before the first day of March are allowed to enter an elementary school.<sup>8</sup> Thus, a school cohort consists of those born between March and December in a year (say, year  $t$ ) and those born in January and February in the next year (year  $t + 1$ ). According to the traditional age reckoning method, those born in January and February are one year younger because their birth year is one year behind that of their classmates, even compared to those born in December.<sup>9</sup> We hypothesize that those born in January and February are less likely to be married than others, as people count their cultural age rather than the physical age. Our data indeed strongly support the hypothesis of what we term the AAR effect.

We exploit the exogenous variation in the marriage probability to estimate the causal effect of marriage on female labor supply. The critical issue is whether our instrument satisfies the validity

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<sup>5</sup> Due to the traditional age reckoning method, infants aged less than one year tend to be undercounted in Chinese population surveys (Judith Banister, 1984).

<sup>6</sup> The age according to the Western age system is referred to as “man-nai” in Korean (“man” means full and “nai” age), as opposed to “hankook-nai” (“hankook” means Korean). Such a distinction also exists in Japan, 数え年 [*kazoedoshi*] (traditional age) versus 満年齢 [*mannenrei*] (Western age) and in China, 虚歳 [*xusui*] (traditional age) versus 實歳 [*zhousui*] (Western age). See [http://en.wikipedia.org/wiki/East\\_Asian\\_age\\_reckoning](http://en.wikipedia.org/wiki/East_Asian_age_reckoning). To our best knowledge, the origin of this unique age system is unknown. We just conjecture that it is related to the use of the lunar calendar.

<sup>7</sup> Our identification strategy is similar to Joshua Angrist and Alan Krueger (1991) in the sense that they exploit heterogeneity within a school cohort.

<sup>8</sup> Beginning in 2009, the cutoff date is changed to January 1<sup>st</sup>.

<sup>9</sup> It is anecdotally said that those born in January and February are more likely to be bullied by their classmates. This suggests that even young children use the cultural age.



condition, that is, those born in January and February are not different from others in terms of the unobservables that might affect labor supply. The most plausible criticism is the existence of the relative age effect. Specifically, it is possible that the marriage probability differentials for those born in January and February arise because they are the youngest among classmates and, as a consequence, they are likely to underperform academically (Kelly Bedard and Elizabeth Dhuey, 2006; Elizabeth Cascio and Diane Whitmore Schanzenbach, 2007). Then, they might be disadvantaged in the marriage market and marry later involuntarily. Or they might marry earlier since they are likely to get lower offered wages. To address this concern, we control for a continuous measure of birth month. The assumption is that the relative age effect, if any, is likely to be continuous. In other words, we assume that there is no reason why the marriage probability or LFP rate discretely changes at the turn of the calendar year, say, between December and January. The data support this assumption.

There are some other potential issues regarding the validity condition of our instrument, such as reporting of the lunar calendar birthday and strategic school entry delay. We will discuss these issues in detail later and implement various robustness checks. One robustness check worth mentioning here is to compare women with men. Although the traditional age system is equally prevalent among men, we believe that men's marriage decision is less sensitive to their cultural age than women's for various reasons. We should find no significant marriage probability differential for men born in January and February. This prediction is also supported by the data.

In summary, we find that marriage significantly decreases young women's labor supply. Compared to never-married single women, married women are by 54 percentage points less likely to participate in the labor force. The OLS estimates are biased toward zero. This implies that there is positive selection into marriage on the unobservables. We also find that the marriage penalty in terms of LFP is larger among older women, regardless of their education level.

The remainder of the paper is organized as the following: In the next section, we will estimate the linear marriage probability model where one's marital status depends on birth month. In particular, we are interested in ascertaining the marriage probability differentials specific to those women born in January

and February. The findings will be the backbone of our identification strategy. In Section III, we check whether those born in January and February are already different from their classmates before they enter the marriage market as well as the labor market. In Section IV, we will estimate the linear LFP equation by different estimation methods. We will also check the robustness of our findings across different specifications as well as different sub-samples. Section V concludes.

## II. Asian Age Reckoning and Marriage

We first examine whether the traditional age system creates significant differentials in the marriage probability for those born in January and February. The main data we use in this paper come from the Korean Economically Active Population Survey (KEAPS) from 2003 to 2007, which is a similar but abridged version of the Current Population Survey in the U.S.<sup>10</sup> The survey is done every month, and we use pooled data of monthly surveys. In each month, about 900 households are replaced. Thus, more than 30 percent of households are replaced every year. The time period of our sample is restricted from 2003 to 2007, first, because information on graduation year (which is important to know as we will elaborate later) is available only since 2003 and, second, because household identifiers are no longer publicly available after 2007.<sup>11</sup> Also, the National Statistical Office recognized that many people reported their birthday in the lunar calendar and, since 2003, they have asked explicitly whether the reported birthday is based on the solar or lunar calendar.<sup>12</sup> This information is important to know since school entry age is defined by the Western age system based on the solar calendar.

We restrict the sample by the following criteria. First, we focus on those women aged between 24 and 30 in terms of *school age*. School age is defined as survey year minus birth year if birth month is from March to December and survey year minus birth year plus one if birth month is January or February. At

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<sup>10</sup> The survey was practiced via Computer Assisted Personal Interviewing (CAPI) in 2003 and via PDA afterwards, which must have improved the data quality substantially.

<sup>11</sup> Also the sampling design changed in 2003 based on the 2000 Population and Housing Census.

<sup>12</sup> According to the 1998 Korea Labor and Income Panel Study (KLIPS), about 60 percent of people use the lunar calendar for their birthday. The proportion increases in age (e.g., 30.4% for age 15-19, 66.5% for 25-29, and 83.2% for 55-59).

school age 24, only about 17.5 percent of women are married, while about 78.6 percent are married at age 30. A majority of women should have their first marriage in the age range. We also assume that those who get married too early or are not still married at older ages are qualitatively different. Second, the sample includes only women who are never married or married, excluding the divorced or the widowed. There are very few divorcees (N = 1,294) or widows (741) given the above age restriction. Third, we exclude those whose education is lower than high school (3,105) and those who are still students (7,472), temporarily quit schooling (2,411), or dropped out (7,019). To further ensure the homogeneity of the sample, we also exclude those who advanced to graduate school (9,978). Similarly, we construct the sample of men. Finally, there are 229,951 observations in the female sample and 177,382 observations in the male sample.<sup>13</sup>

Table 1 presents means and standard deviations of individual characteristics by birth month for the sample of women. In the sample, 59 percent are graduates of 2-year colleges or 4-year universities. About 97 percent are in non-farm households. About 51 percent are married, and 60 percent participate in the labor force. Looking at variation by birth month, we find that the first two variables, *College* and *Non-farm*, do not significantly vary by birth month. On the other hand, in marriage and LFP, there seem to be some observable variation by birth month. In particular, women born in January and February are 6.2 percentage points less likely to be married and 3.3 percentage points more likely to participate in the labor force than those who were born in the other months. Chi-squared tests show that the differences are statistically significant ( $p < 0.05$ ). The Wald estimate indicates that marriage decreases the LFP rate by about 53.4 percentage points ( $p < 0.001$ ).

To see if the result holds after controlling for other individual characteristics, we estimate the following linear probability model:

$$Married = \beta_0 + \beta_1 Jan + \beta_2 Feb + \beta_3' SchAge + \beta_4 RelAge + \beta_5' YrsGrad + \beta_6' X + U \quad (1)$$

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<sup>13</sup> Men's sample is smaller because they are more likely to attend school and serve their mandatory military service in this age range.

where *Married* is the indicator of whether a woman is married and *Jan* and *Feb* are the indicators of whether she was born in January and February, respectively. Under the hypothesis of the AAR effect, the coefficients are expected to be negative.

There are three key explanatory variables that are potentially critical to control for. First, the vector *SchAge* is the set of dummies for school age. After controlling for school-age fixed effects,  $\beta_1$  and  $\beta_2$  capture the marriage probability differentials for those born in January and February relative to the other months. Second, *RelAge* is an integer-valued variable, which is supposed to capture a linear relative age effect. It is coded as zero for February, increasing by one unit each month and reaching up to eleven for March. Formally,  $RelAge = (2 - \text{birth month})$  if birth month is January or February and  $(12 - \text{birth month} + 2)$  otherwise. As discussed earlier, *RelAge* is a critical control variable to disentangle the AAR effect from the relative age effect (cognitive and non-cognitive development by birth month). Third, we control for the number of years since graduation by including the vector *YrsGrad*. This is also potentially an important control variable since it is possible that those born in January and February might enter school later perhaps due to parents' strategic enrollment postponement and, as a result, graduate later. If they indeed graduate later, then it is not surprising to find that they marry later. This case is, if any, very rare. The proportion of delayed enrollments is less than 1 percent for the school cohorts included in the sample (The Center for Education Statistics, various years). Still it is likely that even if they enter school at the same year, those born in January and February might be more likely to repeat grades due to their relative age disadvantage. In order to justify our empirical strategy using instrumental variables, we want to identify the AAR effect among those who have spent the same years after graduation.<sup>14</sup> The KEAPS asks what year the respondent graduated. Since the main objective of the question is to find labor market experience for recently graduated young people, the variable is top coded. If the respondent graduated 10

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<sup>14</sup> A psychological affect is that those born in January and February might feel that they have one extra year than their classmates and, as a consequence, more comfortable with repeating grades or taking terms off. This is unlikely to be a case for high school graduates or below since it is very rare that grade-school children repeat grades or take terms off in Korea.