

Missing Women and the Year of the Fire Horse: Changes in the Value of Girls and Child Avoidance Mechanisms in Japan, 1846, 1906, and 1966

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Abstract

This paper examines the effect of a short-term change in the value of female offspring on couples' childbearing and child-avoidance behaviors. We exploit a natural experiment in Japan in which girls born in a specific astrological year are regarded as less desirable. We then measure couples' relative usage of sex-selective and sex-blind methods of child avoidance in historical and modern Japan. In the 1840s, we find large and significant sex-selective responses, especially in poorer prefectures. By the 1960s, we find practically no sex-selective child avoidance but large and significant sex-blind responses (contraception and abortion), especially in wealthier areas.

JEL Codes: J13, J16, N35, O10, R2

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A large and growing literature exists attempting to explain ways in which poor parents limit the numbers of children they have, especially in sex-selective ways. Economists have examined the magnitude (Oster, 2005; Sen, 1992, 1988) and determinants (Kojima, 2005; Rosenzweig and Schultz, 1982) of these child avoidance behaviors, including the degree to which they respond to economic conditions (Rosenzweig and Schultz, 1982), female bargaining power (Qian, 2005), and public policies (Kalist and Molinari, 2006; Qian, 2004). The current study contributes to this literature by exploring another determinant of child avoidance behaviors: couples' subjective valuations of offspring. We estimate couples' demand responses (in terms of child avoidance behaviors) to a short-term, sex-specific change in the value of girls. We do so by examining a natural experiment that occurred in Japan in 1846, 1906, and 1966. We measure the effects of these price changes on the sizes of surviving male and female cohorts both nationwide and across different prefectures. We also measure effects on marriages, contraception, abortions, and infant and one-year-old deaths. Through these measurements, our estimation strategy provides a uniquely clear and detailed description of what couples do when they are trying to avoid having girl children.⁴

The relationship between child avoidance and the value of girl children is difficult to measure for a variety of reasons. Parents value their children in ways that are not easily expressed in monetary terms. Few price mechanisms exist that regulate gender preference and family size for potential parents. Furthermore, the willingness-to-pay to have or to avoid girl

⁴ Because we obtain and compare estimates across so many contexts, it may be possible to generalize some of our results to learn about child avoidance in other countries. However, caution should be used in generalizing these estimates to other types of demand responses. Short-term price changes are likely to have very different effects than long-term price changes would. For the short-term price change examined in this study, couples can respond by adjusting the timing of their births. Couples cannot effectively avoid long-term costs (such as those caused by gender inequality or cultural norms) except through permanent changes to family size.

children is correlated with many economic and cultural characteristics such as income, education, altruism, and gender inequality.

To measure the effect of the value of girls, we must identify a variable that affects couples' valuations of girl children but is otherwise unrelated to these unobserved characteristics. This paper attempts to partially solve these identification problems by using the year of the fire horse in Japan as a natural experiment. The Chinese Zodiac ascribes one of five elements and one of twelve animals to each year. The combination of fire and horse, which occurs once every sixty years, is regarded as particularly undesirable for women in Japan. Fire horse women – or *hinoeuma* – are said to be stubborn and independent, traits which are considered very negative for women. Fire horse women are said to have troubled marriages, to mistreat men, and to cause their husbands and fathers early deaths (Azumi, 1968; Lebra, 1978; Cortese, 2004; Hashimoto, 1974). No such stigma applies to men born in these years. The superstition became widespread in Japan in 1682 with a popular tale of a fire horse woman who nearly burned down the city of Edo.⁵ Discrimination against fire horse women has been discussed by casual observers (Cortese, 2004), in the anthropology literature (Lebra, 1978, pg. 34), and has been the subject of popular literature (Houston, 2003).⁶ One of the authors of this study has also examined long-term economic effects of being a fire horse woman (Yamada, 2006).

Previous researchers have documented many child avoidance behaviors associated with the year of the fire horse. Births in Japan were unusually low in 1966 relative to surrounding years, as a number of sociologists and health researchers have found (Azumi, 1968; Hashimoto,

⁵ In the mid-1930s, the superstition regained currency with a news story of a fire horse woman who murdered her lover (Kaku, 1972; Kaku and Matsumoto, 1975).

⁶ A milder form of the superstition also applies in Korea to women born in any year of the horse (Kim, 1997; Lee, 2005; Park and Cho, 1994; Se-hyon, 2002). However, Lee (2005) shows that this superstition does not have measurable effects on cohort size or the sex ratio for recent cohorts. Our own analyses confirm this finding (results not shown).

1974; Itoh and Bando, 1987; Kaku, 1972, 1975a, 1975b; Kaku and Matsumoto, 1975; Kim, 1979; Kuruso, 1992; Sakai, 1995; Shin, 1975; Usui, Houhami, and Kaneko, 1976). Researchers have also noted increases in the rates of abortions (Hashimoto, 1974; Kaku, 1975a), still births (Kim, 1979), and infant mortality (Kaku, 1975b) and misrepresentation of birthdates (Kaku, 1972; Usui, Houhami, and Kaneko, 1976) in 1966. The two earlier fire horse episodes have also been the subject of some academic research. Researchers have observed discontinuous drops in cohort size and increases in the sex ratio (more boys) among both the 1846 and 1906 cohorts (Azumi, 1968; Kuruso, 1992).⁷

The current study introduces this fire horse phenomenon to the economics literature on fertility.⁸ We examine the effects of the superstition in a more comprehensive way than has been done in the past. We use a multiple regression approach to correct for misrepresentation of girls' ages and substitution across birth years and to disentangle sex-selective and sex-blind child avoidance strategies. We measure how these effects vary with prefecture-level economic activity and population density and over time. We also relate our estimates to a more general structural equation describing couples' demand for offspring. Subject to our instrumental variables assumptions, we identify the sex ratio of children who would be alive if not for the change in the value of girls. This "sex ratio of marginal children" provides a composite measure of the relative importance of sex-selective versus sex-blind child avoidance responses. We calculate this parameter in each of the many contexts examined in this study, and we compare this sex ratio to the sex ratio of surviving children in surrounding cohorts.

The instrumental variables strategy used in this paper allows for unusually direct evidence on child avoidance. Recent results by Oster (2005) have shown that sex ratios alone do

⁷ Some comparisons have been made between the 1846 and 1906 episodes (Kurosu, 1995) and also in the spatial distribution of the 1846 and 1966 episodes' effects (Itoh and Bando, 1987; Kurosu, 1995).

⁸ There is also brief mention of this phenomenon in Hashimoto (1974).

not suffice to accurately measure sex-selective child avoidance. Because the fire horse superstition affects the value of girls in one specific cohort, we can measure effects on many different outcome variables, some of which are only available in country-level time-series. The time-series variables we examine include male and female cohort sizes, marriage, contraception, abortion, still births, live births, and infant and one-year-old deaths. The cyclicity of the Zodiac allows us to measure these effects in Japan in three very different time periods: 1846, 1906, and 1966. Using prefecture-level panel data for both the 1846 and 1966 episodes, we also measure how much these effects varied with per capita income and population density. In addition, for the 1966 episode, we compare child avoidance strategies in Okinawa (where abortion was illegal) with those in the rest of the country (where abortion was legal).⁹

The change in couples' valuations of girls produces very different responses in different periods and locations in Japan. We find nontrivial amounts of sex-selective child avoidance in response to the 1846 episode, especially in poor areas. We observe considerably less sex-selective child avoidance in response to the 1906 episode. We do, however, observe disproportionately many boys being born and disproportionately many girls dying as infants among the fire horse cohort. This phenomenon diminished considerably over time and appeared in only negligible quantities in 1966. We observe large amounts of sex-blind child avoidance in all three episodes, with the largest drop in cohort size (22%) occurring in 1966. In 1966, this effect was larger in wealthier prefectures and in prefectures with less dense populations. In 1906, delayed marriage appears to have been a common response to the superstition. Much of the 1966 effect can be explained by increases in the rates of contraceptive use and abortions. The drop in 1966 was smaller in Okinawa (where abortion was illegal) than in the rest of the

⁹ This comparison is made in a brief and qualitative way in Potts, Diggory, and Peel (1977, pp. 136-7), and is also suggested in an endnote in Jitsukawa and Djerassi (1994).

country. However, prefecture-level differences in per capita GDP appear to explain much of this difference.

The remainder of this paper proceeds as follows. Section II includes a description of the data and presents descriptive evidence on child avoidance in the year of the fire horse. In Section III, we describe the econometric methodology used in our more formal analysis. Section IV includes regression results and estimates of the sex ratio of marginal children. Section V concludes.

II. Data and Descriptive Evidence

In this next section, we describe the data used in this analysis, and we present descriptive evidence on the effects of the year of the fire horse. First, we describe the population size, birth, birth control, abortion, and infant death data used in this study. Second, we discuss the effects of the fire horse superstition on male and female cohort size in three episodes: 1846, 1906, and 1966. We then measure the contributions of abstinence, contraception, abortion, and infant deaths to the drop in births in each episode.

A. Data Description

Population Size by Sex, Birth Cohort, Marital Status, and Prefecture. Population size by birth year and sex are taken from the earliest available years for each of the three fire horse episodes. These Census years are 1886, 1955, and 1970 for the 1846, 1906, and 1966 episodes, respectively.¹⁰ These data are used to measure population size by sex and age for 1840-1850, 1900-1910, and 1960-1970, respectively. The data from the 1886 and 1970 Censuses are

¹⁰ Earlier data are available for the 1906 episode; however, 1955 is the earliest year in which population size is measured by *calendar* year (January to December) of birth.

measured at the prefecture-level. The 1955 data we use are measured at the country level. Birth years are measured according to the Western calendar, with the year beginning on January 1st.¹¹ Data from the 1886 Census are taken from Japan Ministry of Home Affairs, 1886. Data from the 1955 and 1970 Censuses are taken from Japan Statistics Bureau of the Prime Minister (1955, 1970).

Prefecture-Level Characteristics. For the 1846 and 1966 episodes, we use data describing land area, total population, and economic activity for each prefecture. The data on land area are taken from Wikipedia (2006b). Data on total population at the end of 1890 are taken from Unemura, Nobukiyo and Itoh (1983). Unfortunately, prefecture-level GDP data are not available for the 1840s-era. We proxy for this variable using prefecture-level data on the value of industrial production in 1891, also taken from Unemura, Takamatsu and Itoh (1983). For the 1960s episode, we take a time-series average of prefecture-level GDP from 1960 to 1969, taken from Japan Cabinet Office (2006). We then divide this time-series average by prefecture-level population in 1965, taken from Japan Statistics Bureau and Statistical Research and Teaching Institute (2006).

Table 1 shows descriptive statistics for the prefecture-level data used in this study. The average prefecture has land area of 8,370 km². Population in the average prefecture was 0.9 million in 1890 and 2.2 million in 1965. Both income variables are measured in nominal yen. For the average prefecture, per capita industrial production was ¥6.8 in 1891. For the average prefecture from 1960 to 1969, per capita GDP was ¥290,000.

¹¹ In 1846, Japan followed the Chinese calendar, in which the fire horse year lasted from January 27th to February 14th. Hence, in 1846, roughly 10% of the observations are miscoded. In 1872, Japan officially switched to the Western calendar (Azumi, 1968). For 1906 and 1966 episodes, the fire horse superstition appears to have applied to the Western calendar. As Azumi (1968) and Kakui (1972) have observed, the drop in births began in January, not February, of 1966. Using monthly data, we have also found that the drop began in January in both 1906 and 1966 (results not shown).

Birth Control Data. Three types of birth control are measured in this study. First, couples may avoid childbirth during the fire horse year by remaining abstinent and delaying marriage. For both 1900-1910 and 1960-1970, we measure annual numbers of marriages for the years beginning nine months before the New Year. The year is defined in this way so that 1905 and 1965 are the years when conceptions were likely to produce fire horse children. Monthly marriage data for 1900-1910 are taken from Japan Cabinet Imperial Bureau de le Statistique General (1904-1914). Monthly marriage data for 1960-1970 are taken from Japan Bureau of Statistics (1960-1971). For the most recent fire horse episode, we also measure rates of contraceptive use and abortion. Data on contraceptive use for 1950, 1952, 1954, and biannually from 1955-1969 are taken from Muramatsu (1967) and Mainichi News Service (1970). Contraceptive use is measured as affirmative responses to “currently using contraception” from a survey of married women 50 and under.¹² Abortion figures are official statistics taken from Mainichi News Service (1970) and United Nations (1973).¹³

Annual Vital Statistics. In addition to measuring population size, this study makes use of extensive data on births, infant deaths, and birth control. The long time-series on annual live births shown in Figure 1 is taken from Japan Statistical Bureau (1999). Years in these data are measured from October to September; hence, roughly one third of these births are miscoded. Later in the paper, when we examine annual births by sex for 1900-1910 and 1960-1970, we measure births from January to December. Hence, our more formal analysis does not suffer from this miscoding problem. For 1900-1910, live births and by sex and year are taken from

¹² The two data sources disagree for the 1965 observation. Muramatsu (1967) reports a rate of contraceptive use of 51.9, while Mainichi News Service (1970) reports a rate of 55.5. We use Mainichi’s figure for this analysis, because the source is more recent and the original survey was taken by Mainichi News Service. Indeed, Muramatsu himself cites the larger 1965 number in later work (Muramatsu, 1974).

¹³ The true levels of abortion are likely to have been higher than the official numbers used here. Muramatsu (1974, pp. 133-134) estimates that true rates of abortion are roughly three times the officially stated rates.

Japan Statistical Bureau (1915). Live births by sex and year for 1960-1970 are taken from Japan Division of Health and Welfare Statistics (1960-1970). In addition to live births, we measure still births and neonatal and infant deaths by sex and year for 1900-1910 and 1960-1970. For 1900-1910, still births and neonatal, other infant, and one-year-old deaths (all by sex and year) are taken from Japan Statistical Bureau (1915). Data are missing for still births and one-year-old deaths for 1908. Annual still births and neonatal, infant, and one-year-old deaths by sex for 1960-1970 come from Japan Division of Health and Welfare Statistics (1960-1970).

B. Changes in Births and Male and Female Cohort Size

As Figure 1 shows, the 1966 fire horse year is one of the most striking features of Japanese fertility in the post-war period. Figure 1 shows the number of live births in Japan from 1896 to 1996. From the graph shown here, we observe that births were roughly one fourth lower in 1966 than in surrounding years.¹⁴ We also observe a significant drop (albeit smaller) during the previous fire horse episode in 1906. However, it is difficult to disentangle this drop from the 1904-1905 Russo-Japanese War.

Figure 2 shows the population size by age and sex for each of the three fire horse episodes examined in this study. By measuring the effects of the fire horse year on male and females separately, we can disentangle sex-selective from sex-blind child avoidance mechanisms. Panel A shows population size by sex for the 1840-1850 birth cohorts, taken from the 1886 Census. Panel B shows the same data for the 1900-1910 cohorts, taken from the 1955 Census. Panel C shows these data for the 1960-1969 cohorts, taken from the 1970 Census. In 1846, we find that the population size of females is considerably smaller than for surrounding

¹⁴ Surrounding cohorts include two years before and after the fire horse year, as explained in the econometric section of this paper. This graph understates the true effect, because births are measured from October to September, rather than January to December.

cohorts. We observe no such effect for males. Observing this population at such a late date may lead us to incorrectly measure the effects of the superstition. Nevertheless, migration and suicide occurred in very small amounts in this period, and life expectancy at age 6 was considerably higher than 44.¹⁵ Hence, we attribute much of the drop in female population to sex-selective child avoidance caused by the fire horse year. We also observe a drop in the population of girls in the 1906 cohort. However, population size was disproportionately high for females born in 1905 and 1907, indicating that parents were probably misrepresenting girls' birthdays.¹⁶ Births may have also been slightly lower than normal from 1904 through 1906 because men were abroad fighting the Russo-Japanese War. In the 1966 episode, population size dropped equally for boys and girls, indicating that couples were avoiding childbirth through sex-blind strategies such as abstinence, contraception, or abortion.

C. Direct Measures of Sex-Blind Child Avoidance and Infant Deaths

Figures 3 and 4 provide direct evidence on sex-blind and sex-selective child avoidance mechanisms for the 1906 and 1966 fire horse episodes. Delaying marriage and remaining abstinent was one way in which couples could avoid having girls in fire horse years. Panels A and B of Figure 3 show marriages for 1900-1909 and 1960-1970, respectively. In both cases, the year is defined from April to March, beginning roughly 9 months before the New Year.

¹⁵ From 1906 to 1910, suicides constituted less than 1% of all deaths (Japan Statistical Bureau, 1915, pg. 25). Precise data on life expectancy are not available for these 1840s cohorts. However, of the women born from 1843 to 1847 and alive in 1890, more than half were still alive in 1908 (Japan Statistical Bureau, 1915, pg. 6). Hence, living until age 65 and older appears to have been very common.

¹⁶ Alternatively, it is possible that the rise in the sex ratios in surrounding years indicates real changes in survival rates. If infanticide was common, parents may have taken resources from fire horse girls and used them to increase survival rates for other girls. To the extent that this phenomenon occurred, our estimates capture the net effect of the fire horse superstition on infanticide. Other researchers have used this same approach to measure mislabeling. Kaku (1972) finds that roughly 11,000 fire horse girls' birthdays were misreported as 1965 or 1967. For the 1906 and 1966 episodes, Usui, Houhami, and Kaneko (1976) also estimate similar amounts of misrepresentation of ages as we do.

In the earlier episode in Panel A, we observe marriages dropping for two reasons – the fire horse year and the Russo-Japanese War. In the absence of these two factors, we might expect marriages to have increased linearly from 1902 to 1909. Marriages increased slightly from 1903 to 1904, probably in anticipation of the war. In 1904, marriages were 24,000 below the 7-year trend from 1902-1909. By 1905, marriages had dropped to 71,000 below this trend. In 1906, marriages increased dramatically, but were still 33,000 below trend. The high values in 1907 and 1908 probably reflect marriages that were delayed due to both the war and the fire horse year. A rough back-of-the-envelope calculation suggests that the Russo-Japanese can explain a drop of about 30,000 marriages per year.¹⁷ Hence, the majority of the drop from 1904 to 1906 can be explained by the war. However, the additional drop of 40,000 marriages (an 11% drop) in 1905 is probably attributable to the fire horse superstition. Hence, even after accounting for the effects of the war, abstinence appears to have been a major child avoidance mechanism used in this episode.

As Figure 2 shows, sex-blind child avoidance techniques were very common in 1966. In this later episode, shown in Panel B of Figure 3, marriages were flat for most of the period. Hence, delayed marriage appears to have been a much less common child avoidance mechanism in the 1960s than in the earlier episode. Panels C and D of Figure 3 present evidence on other types of sex-blind child avoidance techniques used in the 1960s. Panel C shows the fraction of couples reporting the use of contraceptives from 1950 to 1970. We observe a discontinuous jump in contraceptive use in 1965, the year before the fire horse year. Contraceptive use

¹⁷ About 400,000 Japanese troops, mostly in their early twenties, fought in the Russo-Japanese War (Zuljan, 2000; Ross, 1912, pg. 490). During this time period, roughly 8% of men in their early twenties married each year (Japan Bureau de la Statistique General, 1910; Japan Statistical Bureau, 1915). Assuming constant rates of marriage across troops and civilians, the war can explain a drop of roughly $0.08 \times 400,000 \approx 30,000$ marriages/year. Japan did not sign a treaty with Russia until September 1905 (Zuljan, 2000). Moreover, the war could have had delayed effects on marriage by postponing search activities. Hence, it is reasonable to expect that marriages would remain low in 1906.

declined slightly after this episode; however, most of the rise in contraceptive use appears to have been permanent. Hence, it appears that couples continued to use this new technology after first adopting it (though with less frequency, as the birth statistics show). Panel D shows legal abortions per live birth from 1960 to 1970. While births dropped by 26%, we see no trend break in the number of legal abortions. Put together, these phenomena produced a 28% increase in the rate of legal abortions, as shown in Panel D. This increase in the rate of legal abortions accounts for roughly one third of the drop in live births in 1966.

Figure 4 shows some evidence that measurable amounts of female infanticide occurred in 1906 and in 1966. Panels A and B show the sex ratio (male over female) of reported still and live births from 1900-1910 and 1960-1970, respectively. We observe a rise in the sex ratio of live births in 1906. However, we also observe drops in the sex ratio in the years before and after, indicating that parents were misrepresenting girls' birthdates. As we see later in the population regression in Table 2, this apparent misrepresentation of birthdays can account for all of the increase in the sex ratio in 1906. We observe no change in the sex ratio of reported still births in 1906. Hence, girls do not appear to have been disproportionately classified as still in 1906. We also observe significant amounts of misreporting of births in 1966, as shown in Panel B. The sex ratio of live births jumped discontinuously in 1966. The magnitude of this jump indicates roughly 14,000 too few girls' births in the fire horse year. However, as Figure 2 showed, no such imbalance appeared in the sex ratio in the 1970 Census. Hence, the jump is probably attributable to births that went unreported. The sex ratio of still births is highly variable over this period, owing to the low numbers of still births. At most, the slight drop observed in 1966 accounts for roughly 200 missing girls.

The next two panels show the sex ratios of infant deaths for 1900-1910 and 1960-1970, respectively. Panel C shows the sex ratios of infant and one-year-old deaths occurring between 1900 and 1910. We observe a discontinuous drop in infant deaths in 1906. This magnitude of this drop indicates that roughly 1,800 fire horse girls died in the first year of life as a result of the superstition.¹⁸ The sex ratio of one-year-old deaths increased slightly in 1906. Hence, parents may have redistributed resources from fire horse girls to girls from other cohorts. If we ignore the increase in 1906, we observe a significant drop in the sex ratio of one-year-old deaths from 1905 to 1907. Data on one-year-old deaths are missing for 1908. The magnitude of the drop from 1905 to 1907 indicates that about 700 fire horse girls died after one year of life as a result of the superstition. Together, these 2,500 infant and one-year-old deaths constitute roughly 0.4% of the 670,000 live girl births in 1906. Panel D shows the sex ratio of infant deaths from 1960 to 1970. These data are fairly noisy, and we observe spikes in two years totally unrelated to the fire horse superstition. The slight trend break in 1966 and 1967 may reflect infanticide or fatal neglect of fire horse girls. However, the magnitude is very small and can probably explain at most about 100 girls' deaths.¹⁹

III. Econometric Methodology

This next section describes the econometric methodology used in this paper. First, we describe a structural regression equation for the demand for children. Second, we introduce the

¹⁸ The graphs are noisier when we examine neonatal and other infant deaths separately (graphs not shown). However, the magnitudes of the drops from 1905 to 1906 indicate that roughly half of these 1,800 infanticides were neonatal deaths.

¹⁹ Male and female infant deaths were 15,000 and 11,000 in 1966 and 17,000 and 12,000 in 1967. If we draw a linear trend from 1965 to 1969 or 1970, the predicted sex ratio is 0.01 higher than observed. In both of these cases, we estimate roughly 50 missing women for each of 1966 and 1967. If we draw a trend from 1965 to 1968, we obtain higher estimates of roughly 200 missing women for each of 1966 and 1967. However, the high sex ratio in 1968 appears to have been a temporary blip and not an accurate measure of the natural rate. We observe no clear pattern with one-year-old deaths. These data are not shown because the data are noisy and one-year-old deaths were rare at this time.

year of the fire horse as an instrument, and we describe a reduced-form demand equation. Third, we describe our statistical corrections for substitution across birth years and parents' misrepresentation of fire horse girls' birth years. Finally, we characterize the "sex ratio of marginal children," a key parameter of interest in this study.

A. Structural Equation for Child Avoidance

Let $N_{g,c}$ be the total number of children of gender g and cohort c alive in the population in the survey year. Let τ_c be the cost that a single girl of cohort c imposes on a household. We will suppose that the aggregate demand for male and female children is a semi-log function of this cost, a vector of observable characteristics $\mathbf{X}_{g,c}$ and serially correlated unobservables $\varepsilon_{g,c}$.

$$(1) \quad \ln(N_{g,c}) = \alpha_1 * \tau_c + \alpha_2 * Female_g * \tau_c + \alpha_3 * Female_g + \mathbf{X}'_{g,c} \boldsymbol{\beta} + \varepsilon_{g,c}$$

where $Female_g$ is a dummy variable indicating the sex of the people measured in that observation. A change in the cost of girls may also affect male cohort sizes due to sex-blind child-avoidance mechanisms. However, the demand response may be different for girls than for boys due to the existence of sex-specific child-avoidance mechanisms.

In addition to the cost of raising girls, the cost of raising boys is also likely to affect couples' numbers of children. The focus of this study is a change in the cost of girls. Consequently, for the purposes of this analysis, we do not include a separate variable for the cost of raising boys. The corresponding terms for the cost of boys appear in the unobservables $\varepsilon_{g,c}$.

B. Reduced-Form Equation and the Year of the Fire Horse as an Instrument

One major obstacle that arises in attempting to estimate α_1 and α_2 is measuring τ_c . Raising children entails a wide range of costs, and many of these costs are difficult to measure. Moreover, those costs that are measurable are likely to be correlated with unobservable determinants of childbearing and child avoidance such as income, education, and female bargaining power. Moreover the cost of raising girls may be correlated with the cost of raising boys, which is also unobserved.

The estimation strategy used in this study is to use the year of the fire horse to as an instrument for τ_c . As discussed earlier, the cost of girls is higher for fire horse cohorts than for other birth cohorts. It is unlikely that other major social changes in Japan followed this same sixty year cycle. Hence, through this comparison, we can measure the effects of changing the cost of girl children, holding constant other important social factors such as income, education, and gender equality. Moreover, it is well documented that the fire horse superstition only affects the cost of raising girls. Consequently, we can observe changes in τ_c that are totally unrelated to the cost of raising boys. Let $FireHorse_c$ be a dummy variable for being born in the year of the fire horse. The first stage of our instrumental variables design can be described as

$$(2) \quad \tau_c = \gamma * FireHorse_c + \mathbf{X}'_{g,c} \boldsymbol{\delta} + u_{g,c}$$

Because we do not observe τ_c , we cannot estimate equation (2), and γ is not identified. We can, however, estimate the reduced-form effects of $FireHorse_c$ and $Female_g * FireHorse_c$ on male and female cohort sizes. Substituting equation (2) into equation (1), the reduced-form equation for our instrumental variables design can be expressed as

$$(3) \quad \ln(N_{g,c}) = \pi_1 * FireHorse_c + \pi_2 * Female_g * Firehorse_c + \\ + \pi_3 * Female_g + \mathbf{X}'_{g,c} \boldsymbol{\zeta} + v_{g,c}$$

where $\pi_1 = \gamma^* \alpha_1$, $\pi_2 = \gamma^* \alpha_2$, $\pi_3 = \alpha_3$, $\zeta = \beta + \gamma^* \delta$, and $v_{g,c} = \varepsilon_{g,c} + \gamma^* u_{g,c}$. This reduced-form equation is the primary estimation equation in this analysis. As a key identifying assumption, we suppose that $FireHorse_c$ and $Female_g * FireHorse_c$ are both orthogonal to $\varepsilon_{g,c}$ and $u_{g,c}$. Hence, we assume that income, education, gender inequality, and other omitted determinants of childbearing and child avoidance did not change discontinuously in the fire horse year. Subject to these assumptions, the reduced-form parameters in equation (3) can be identified using ordinary least squares.

C. Timing of Births and Misrepresentation of Birth Dates

To accurately measure the effect of the superstition on cohort size, we must identify a comparison group of nearby cohorts that were not affected. One way that couples responded to the year of the fire horse was to adjust the timing of births. Indeed, we observe unusually large cohort sizes immediately before and after the fire horse cohorts, suggesting that such substitution did occur. Consequently, using these before and after cohorts as a comparison group could lead us to overstate the effects of the superstition. To solve this problem, we use the cohorts two years before and two years after as our control group. Our resulting $\mathbf{X}_{g,c}$ matrix includes year dummies for every year except for the fire horse year (which is included as a regressor of interest), two years before, and two years after.

The misrepresentation of birthdates represents a possible confounding factor associated with our regression model. In addition to sex-selective and sex-blind child-avoidance strategies, couples could have waited until fire horse girls were born and then misrepresented their birth dates. The easiest form of misrepresentation probably would have been to alter a child's official

birthday by a year or less.²⁰ This strategy would look like infanticide in the data, because disproportionately few girls would appear in the fire horse cohort. However, disproportionately many girls would appear in the data in the cohorts immediately before or after the fire horse cohort. To adjust for this relabeling of girls, we include two variables, $Female_g * [Fire Horse_c - Fire Horse_{c+1}]$ and $Female_g * [Fire Horse_c - Fire Horse_{c-1}]$, as controls in the vector $\mathbf{X}_{g,c}$. The first variable adjusts for changes in the sex ratio that can be explained by disproportionately many girls in the previous year. The second variable adjusts for changes in the sex ratio that can be explained by disproportionately many girls in the later year.

D. Heterogeneous Effects and Prefecture-Level Data

In addition to varying by sex of the child, the effect of the fire horse year might vary across geographic regions. For instance, the effects of the superstition might be correlated with the wealth or population density of a prefecture. Next, we incorporate prefecture-level time-series data to relax the assumption that the effect of the fire horse year is constant across prefectures. We suppose that the effect of the fire horse year varies in a linear way with a time-invariant prefecture-level variable $x_{1,p}$. The time-invariant characteristics ($x_{1,p}$ variables) used in this study appear in Table 1 and include per capita GDP, per capita industrial production, and population density. We de-mean $x_{1,p}$ in our regressions so that the main (un-interacted) effect is unchanged. We also interact $x_{1,p}$ with all the control variables in the regression. Our new regressions take the form

$$(4) \quad \ln(N_{g,c,p}) = \tilde{\pi}_1 * (x_{1,p} - \bar{x}_1) * FireHorse_c + \tilde{\pi}_2 * (x_{1,p} - \bar{x}_1) * Female_g * Firehorse_c \\ + \tilde{\pi}_3 * (x_{1,p} - \bar{x}_1) * Female_g + \tilde{\pi}_4 * FireHorse_c \\ + \tilde{\pi}_5 * Female_g * Firehorse_c + \tilde{\pi}_6 * Female_g + \mathbf{X}'_{g,c,p} \tilde{\zeta} + \tilde{\nu}_{g,c,p}.$$

²⁰ Such relabeling to nearby “safe years” has been documented in the medical literature (Kaku, 1972).

D. The Sex Ratio of Marginal Children

The fire horse superstition allows us to observe the effects of large changes in couples' subjective valuations of girls across many different contexts. Unfortunately, we are not able to measure the magnitude of this change in valuation. Consequently, we cannot estimate elasticities of child avoidance with respect to couples' perceived values female offspring. That is, the reduced-form equation (3) is identified, but the structural demand equation (1) is not. We can, however, measure the sex ratio of children who would be born if not for the drop in the value of girls. This "sex ratio of marginal children" provides a composite measure of the degree to which price-sensitive child avoidance mechanisms are sex-blind or sex-selective. If parents only used sex-blind child avoidance strategies, then the sex ratio of marginal children would be close to one. If parents avoided girls through sex-selective means, then we would expect this ratio to fall below one.

To estimate the sex ratio of marginal children, we calculate the reduction in boys attributable to the fire horse year and divide it by the reduction in girls. Substituting the appropriate dummy variables into equation (3) and rearranging terms, the sex ratio of marginal children, $SR_{marginal}$, can be defined as:

$$(5) \quad SR_{marginal} = \frac{\exp(\pi_1 + \mathbf{X}'_{g,c} \zeta) - \exp(\mathbf{X}'_{g,c} \zeta)}{\exp(\pi_1 + \pi_2 + \pi_3 + \mathbf{X}'_{g,c} \zeta) - \exp(\pi_3 + \mathbf{X}'_{g,c} \zeta)}$$

The numerator of equation (5) shows the measured number of boys in the fire horse cohort minus the measured number of boys in the average nearby cohort. The denominator of equation (5) shows the corresponding difference for girls. Similarly, we can express the sex ratio of surviving children in surrounding cohorts, $SR_{surrounding}$, as:

$$(6) \quad SR_{surrounding} = \frac{\exp(\mathbf{X}'_{g,c} \zeta)}{\exp(\pi_3 + \mathbf{X}'_{g,c} \zeta)}.$$

Equation (6) shows the measured number of boys divided by the measured number of girls in the average nearby cohort. Estimating this sex ratio provides a benchmark against which to compare the sex ratio of marginal children.

In addition to these sex ratios, we also estimate the fractional change in cohort size associated with the fire horse year. We measure this change as:

$$(7) \quad F_{change} = \frac{[\exp(\pi_1 + \mathbf{X}'_{g,c} \zeta) + \exp(\pi_1 + \pi_2 + \pi_3 + \mathbf{X}'_{g,c} \zeta)] - [\exp(\mathbf{X}'_{g,c} \zeta) + \exp(\pi_3 + \mathbf{X}'_{g,c} \zeta)]}{e \exp(\mathbf{X}'_{g,c} \zeta) + \exp(\pi_3 + \mathbf{X}'_{g,c} \zeta)}.$$

Equation (7) shows the measured number of children (boys plus girls) in the fire horse cohort minus the average number for nearby cohorts, all divided by the average number for nearby cohorts.

IV. Empirical Results

This next section shows the results from our regression analysis. First, we measure the reduced-form effects of the 1846, 1906, and 1966 fire horse episodes on sex-selective and sex-blind child avoidance mechanisms. Second, we measure how these effects vary with prefecture-level economic activity and population density for the 1846 and 1966 episodes. Third, we explore how the effects of the superstition in 1966 in Okinawa (where abortion was illegal) differed from those in the rest of Japan (where abortion was legal). Finally, we present estimates of the sex ratios for marginal children and children in surrounding cohorts for a number of cases studied here.

A. Reduced-Form Estimates of Sex-Selective and Sex-Blind Child Avoidance

Table 2 shows GLS regression results from Equation (3). Each column shows results from a different regression. The level of observation is the sex by birth year combination. Columns (1), (2), and (3) show results for the 1846, 1906, and 1966 fire horse episodes, respectively. The first row shows the coefficient on $Female_g * FireHorse_c$. This coefficient measures the extent of sex-selective child avoidance as a response to the fire horse episode. The second row shows the coefficient on $FireHorse_c$. This coefficient measures the extent of sex-blind child avoidance as a response to the fire horse episode. The third and fourth rows include two different measures of the misrepresentation of birthdates, as described in Section III. Each regression includes controls for female, year dummies (with the exception of two years before and two years after the fire horse year), and a constant term. The specifications assume within-period correlation across sexes and sex-specific AR(1) terms and are estimated using Prais-Winsten panel regressions (xtpcse in Stata).²¹

The results from Table 2 confirm the descriptive findings shown in Figure 2. In 1846, we observe a large and significant sex-selective drop of 0.031 log points associated with the fire horse year. We observe no such sex-selective drop in the later episodes. We observe large sex-blind responses to the fire horse year in all three episodes. The drops are insignificant in both the 1846 and 1906 episodes, and much of the 0.06 log point drop 1906 may be attributable to the Russo-Japanese War.²² In 1966, we observe a large and significant sex-blind response to the fire

²¹ In the years immediately before and after the fire horse years, we observe compensating increases in cohort size. We measure the magnitude of the drop in cohort size using the endpoints of this five-year interval so that our results are not influenced by these compensating increases. The precision of our estimates varies depending on the assumed covariance structure for the error terms. Nevertheless, the qualitative results of this study are not sensitive to excluding year dummies, controlling for sex-specific trends, assuming independent errors across sexes, or excluding the AR(1) term.

²² When we measure the sex-blind child avoidance as the drop from 1905 to 1906, we find a much smaller effect of 0.02. However, measuring the difference between 1905 and 1906 may understate the effect of the fire horse year. As we will argue later, the Russo-Japanese War's effects on marriage appear to have lasted through 1906. Nevertheless, the return of already married soldiers may have raised birth rates in 1906 above their normal levels.

horse year of -0.26 log points. We also observe large and significant amounts of relabeling of girls' birthdays in all 3 episodes.

B. Heterogeneous Effects across Prefectures

In addition to varying across time periods, the effect of the fire horse year could have varied across geographical areas for many reasons. We might expect especially large responses to the fire horse year in poor areas, where people are relatively less educated and possibly more superstitious. Wealthier areas are likely to have had better access and knowledge regarding contraception and abortion. Hence, we might expect more sex-blind child avoidance in wealthier prefectures. Additionally, the effects of the superstition might vary with population density. Couples living in more densely populated areas could experience greater levels of stigma or spousal selection. Alternatively, urban areas might be more educated and less insular and consequently less susceptible to the superstition.

In Table 3, we estimate how much the effects of the fire horse year varied at the prefecture level with economic conditions and population density. The specifications for these regressions are the same as in equation (4). Columns (1) and (2) show estimated regression coefficients for the 1846 and 1966 fire horse episodes, respectively. The dependent variable is the natural log of population size by year of birth, sex, and prefecture. For the 1846 episode, the regressors of interest are $Firehorse_c * Female_g$ (to measure sex-selective child avoidance), $Firehorse$ (to measure sex-blind child avoidance) and the interactions of those variables with prefecture-level per capita industrial production in 1891 and population density. For the 1966 episode, the regressors of interest are $Firehorse_c * Female_g$ and $Firehorse_c$ and the interactions of those variables with prefecture-level per capita GDP and population density. Per capita

industrial production, per capita GDP, and population density are all demeaned, so that the coefficients on *Firehorse*Female* and *Firehorse* represent the effects for the average prefecture. The regressions include the same sets of controls as in Table 2, including both forms of relabeling (coefficients not shown). Interaction variables (per capita industrial production, per capita GDP, and population density) are also included in levels and are interacted with all controls (coefficients also not shown). Specifications assume within-period correlation across sexes and prefectures and sex- and prefecture-specific AR(1) terms and are estimated using Prais-Winsten panel regressions, as before.

In both columns, the coefficients on *Firehorse_c * Female_g* and *Firehorse_c* are similar to those found in the country-level regressions in Table 2. In column (1) of Table 3, we find a positive and significant coefficient of 0.005 for *Fire Horse_g * Female_g * per Capita Industrial Production_p*. Hence, for an increase in per capita industrial production from ¥6.8 to ¥11.8 (a one standard deviation increase), our estimated magnitude of sex-selective child avoidance decreases by more than half, from -0.034 to -0.008. Hence, we observe less of a sex-selective response to the fire horse year in more developed prefectures. We also observe a negative (although insignificant) coefficient of -1.1×10^{-4} for *Fire Horse_c * Female_g * Population Density_p* in the 1846 episode. Hence, for a rise in population density from 180 to 350 (a one standard deviation increase), we measure a rise in the magnitude of the sex-selective response by almost half, from -0.035 to -0.054. Hence, we find suggestive evidence that denser populations exhibit more of a sex-selection response. At the same time, we observe a small but significant negative coefficient on *Fire Horse_g * per Capita Industrial Production_p*. Hence, among the wealthier prefectures, we observe less sex-selective and slightly more sex-blind child avoidance.

In column (2), we find significant relationships between sex-blind child avoidance (the coefficient on *Fire Horse*) and both per capita GDP and population density. Our estimated coefficient for the interaction of *Fire Horse_c* and *per Capita GDP_p* is -4.5×10^{-7} . For the average prefecture, a one-standard deviation increase in GDP per capita (from ¥290,000 to ¥368,000) would magnify the amount of sex-blind child avoidance by 15%, from -0.27 to -0.31.²³ Hence, wealthy areas, (presumably with better access to contraception) were the most likely to exhibit sex-blind responses. Alternatively, a one-standard deviation increase in population density (from 440 to 1,200 people per square kilometer) would attenuate the amount of sex-blind child avoidance by 14%, from -0.27 to -0.23.²⁴

C. Okinawa and the Effect of the Prohibition of Abortion

Next, we consider the effects of the 1966 fire horse episode in Okinawa as they compare with the rest of the country. During the 1966 fire horse episode, Okinawa was a protectorate of the United States. Consequently, while abortion was legal in the other 46 prefectures of Japan, it was illegal in Okinawa during this episode. The pattern of evidence we present here is not sufficiently clear to draw conclusive causal inferences about the effects of prohibiting abortion. Nevertheless, the results from Figure 5 and Table 4 provide suggestive evidence that prohibiting abortion reduces the effects of the fire horse year.²⁵

Figure 5 shows cohort size by sex and birth year for children born between 1960 and 1969 and living in Okinawa in 1970. As the graph shows, we still observe a large and apparently

²³ $-4.5 \times 10^{-7} * (\text{¥}368,000 - \text{¥}290,000) = -0.035$. This coefficient is smaller in magnitude than is the corresponding interaction term for per capita industrial production from column (1).

²⁴ $0.05 * (1.20 - 0.44) = 0.038$.

²⁵ For recent work on a similar topic, see Kalist and Molinari's (2006) study examining the effect of prohibiting abortion on infanticide in the United States.

sex-blind effect of the fire horse year, despite the prohibition of abortion. In percentage terms, the magnitude of the drop is lower than the drop for the country at large.

In Table 4, we measure the degree to which child avoidance was different in Okinawa in 1966 than it was in other prefectures. The specifications for the regressions in Table 4 are similar to those in column (2) of Table 3. Both regressions add a dummy for Okinawa prefecture interacted with the fire horse effects and controls. Column (2) includes interactions of per capita GDP and population density with the fire horse variables and controls; column (1) does not. The first row of Table 4 shows coefficients on $Fire\ Horse_c * Female_g * Okinawa_p$. This coefficient measures the degree to which sex-selective child avoidance differed between Okinawa and other prefectures. The second row of Table 4 shows coefficients on $Fire\ Horse_c * Okinawa_p$. This coefficient measures the degree to which sex-blind child avoidance differed between Okinawa and other prefectures.

The first row of Table 4 shows a positive and significant coefficient for $Fire\ Horse_c * Female_g * Okinawa_p$. Hence, the proportion of girls was abnormally high for the fire horse cohort in Okinawa. The reasons for this result are unclear and could simply reflect a statistical anomaly due to randomness in the sex ratio at birth.²⁶

As Figure 5 showed, Okinawa experienced a drop in cohort size for the 1966 fire horse cohort, despite the prohibition of abortion. The second row of Table 4 shows that this drop was smaller than it was in the rest of Japan, where abortion was legal. In column (1), we observe a positive and significant coefficient on $Fire\ Horse_c * Okinawa_p$. Hence, the sex-blind response to the fire horse year was 0.11 log points smaller in Okinawa than in the rest of the country. This result is not sufficient, however, to conclude that the prohibition of abortion was responsible for

²⁶ Okinawa is an especially small prefecture, accounting for less than one percent of Japan's total population at this time. For the birth years examined here, the typical cohort included 10,300 boys and 10,700 girls. Consequently, the sex ratio was considerably more variable for Okinawa than for the country at large.

Okinawa's drop in cohort size. The history and culture of Okinawa differ from those of the rest of Japan in many important ways.²⁷ Per capita GDP was also considerably lower in Okinawa (¥144,000) than in the rest of the country (¥294,000) at this time. In column (2), as in Table 3, we allow for the possibility that the effects of the fire horse year vary with a prefecture's GDP per capita and population density. After adjusting for these effects, the coefficient on *Fire Horse_c * Okinawa_p* drops by roughly half, to 0.057. Hence, adjusting for the income differences between Okinawa and other prefecture explains roughly half of the difference in the sex-blind response to the fire horse year. If additional control variables were available in the data, adding them might change the coefficient on other ways. Hence, it appears that the rest of Japan does not constitute a valid control group for measuring the effects of prohibiting abortion in Okinawa. This finding calls into question the validity of this natural experiment for measuring the effects of prohibiting abortion. Nevertheless, we do obtain a positive and significant coefficient on *Fire Horse_c * Okinawa_p*. Hence, we find suggestive evidence that prohibiting abortion may have reduced the fire horse response.

D. Sex Ratios of Marginal Children and Children from Surrounding Cohorts

Table 5 presents a summary of the results from this paper in a slightly different form. The three columns in Table 5 show the three parameters of interest defined in part D of Section III. Column (1) shows F_{change} , the fractional change in cohort size associated with the fire horse year from equation (7). Column (2) shows $SR_{marginal}$, the sex ratio of marginal children, defined in equation (5). This parameter provides a composite measure of couples' preferences for sex-selective versus sex-blind child avoidance strategies. If this ratio is lower than one, then the data suggest that parents used sex-selective techniques to avoid girls. Column (3) shows $SR_{surrounding}$,

²⁷ Kurosu (1994, pg. 90), Wikipedia (2006a).

the sex ratio of surrounding cohorts, from equation (6).²⁸ This parameter provides a basis against which to compare $SR_{marginal}$. The standard errors for all the parameters in this table are calculated using the delta method.

Each row of Table 5 shows estimates for a different subset of the data. The first three rows show the parameters of interest for the 1846, 1906, and 1966 fire horse episodes, respectively. These first few rows confirm the main results from Table 2. From column (1), we observe moderately large proportional drops in cohort size (0.061 and 0.056) during the two earlier episodes. The drop during the 1966 episode is considerably larger, at 0.232. The standard errors for the sex ratio of marginal children are large, and in no case can we reject that this ratio equals one. Nevertheless, during the 1846 episode, the sex ratio of marginal children was 0.660, suggesting nontrivial amounts of sex-selective child avoidance. We estimate that the sex ratios of marginal children in the 1906 and 1966 episodes are both close to one, as are the sex ratios of surrounding cohorts. Hence, we find no evidence for sex-selective child avoidance in the later episodes.

As before, we obtain unusual results for the prefecture of Okinawa. Row 4 of Table 5 shows the parameters of interest for Okinawa during the 1966 fire horse episode. The drop in population associated with the fire horse episode was considerably smaller in Okinawa (0.127) than in Japan overall (0.232). As Table 4 showed, this difference is at least partially attributable to the low levels of GDP per capita in Okinawa at the time. We also observe disproportionately many boys among the “missing children,” leading to a sex ratio of 1.516 for the marginal children. As mentioned earlier, the reasons for this result are unclear.

Rows 5, 6, and 7 of Table 5 show the parameters of interest for the 1846 episode as they vary with prefecture-level economic activity. In these rows, the data for the 1846 episode are

²⁸ Surrounding cohorts include 1844 and 1848 for the earlier episode and 1964 and 1968 for the later episode.

divided into prefectures with the lowest, medium, and highest levels of 1891 industrial production per capita. The fractional change in cohort size associated with the fire horse year was similar across the three groups, ranging from 0.054 to 0.067. As with the previous estimates, the standard errors for the sex ratio of marginal children are large. None of the sex ratios estimated here differ significantly from zero. The data are consistent, however, with the theory that sex-selective child avoidance decreases with income. For the prefectures with the lowest levels of economic activity, our estimate of $SR_{marginal}$ is 0.547. For the middle group, our estimate is slightly higher, at 0.620. And for the prefectures with the highest levels of economic activity, we obtain an estimate of $SR_{marginal}$ that is close to one, at 0.822. Hence, we find that higher prefecture-level economic activity is associated with a higher sex ratio of marginal children. As with the first three rows, we find that the sex ratio of surrounding cohorts is slightly higher than unity for all three groups.

In the last three rows of Table 5, the data for the 1966 episode are divided into prefectures with the lowest, medium, and highest levels of per capita GDP. For the poorest prefectures, we find that the fractional drop in cohort size associated with the fire horse year was 0.207. For the other two groups, we find considerably larger drops of 0.240 and 0.234. This finding may appear because wealthier areas have greater access to contraceptives and abortion. The sex ratios are slightly higher than unity for the marginal children and for surrounding cohorts in all three groups. Hence, it appears that sex-selective child avoidance strategies were not common for any of the three income groups at this time.

V. Conclusion

This paper explores the ways in which couples respond to a short-term change in the value of girl children. We focus on a natural experiment in Japan in which girls born in the year of the fire horse are regarded as undesirable. We find that the superstition had nontrivial effects of the superstition on male and female cohort sizes in three episodes: 1846, 1906, and 1966. We use linear regressions to adjust for substitution across years in births and the misrepresentation of girls' birthdays, and to disentangle sex-selective and sex-blind child avoidance strategies. We also describe and estimate a structural parameter – the sex ratio of the marginal child – that is identified through this natural experiment.

In 1846, when Japan was relatively undeveloped, we find large and significant levels of sex-selective child avoidance in response to the fire horse year. The phenomenon was most pronounced in poor prefectures. As the country became wealthier, however, it appears that couples ceased to use sex-selective child avoidance as a response to a change in the value of girls. Using data on live births and infant deaths, we observe evidence of some sex-selective child avoidance for the 1906 cohort. During the 1906 episode, much of the drop in cohort size can be explained by a drop in marriage rates. Hence, couples had switched to sex-blind strategies long before contraception and abortion were common. By the 1966 episode, sex-selective child avoidance had dropped to negligible levels. We also observe a substantial amount of sex-blind child avoidance strategies (a 22% drop in cohort size) during the 1966 episode. The drop in 1966 was most pronounced in wealthy and sparsely populated prefectures. Much of this drop appears attributable to the use of contraceptives and abortion.

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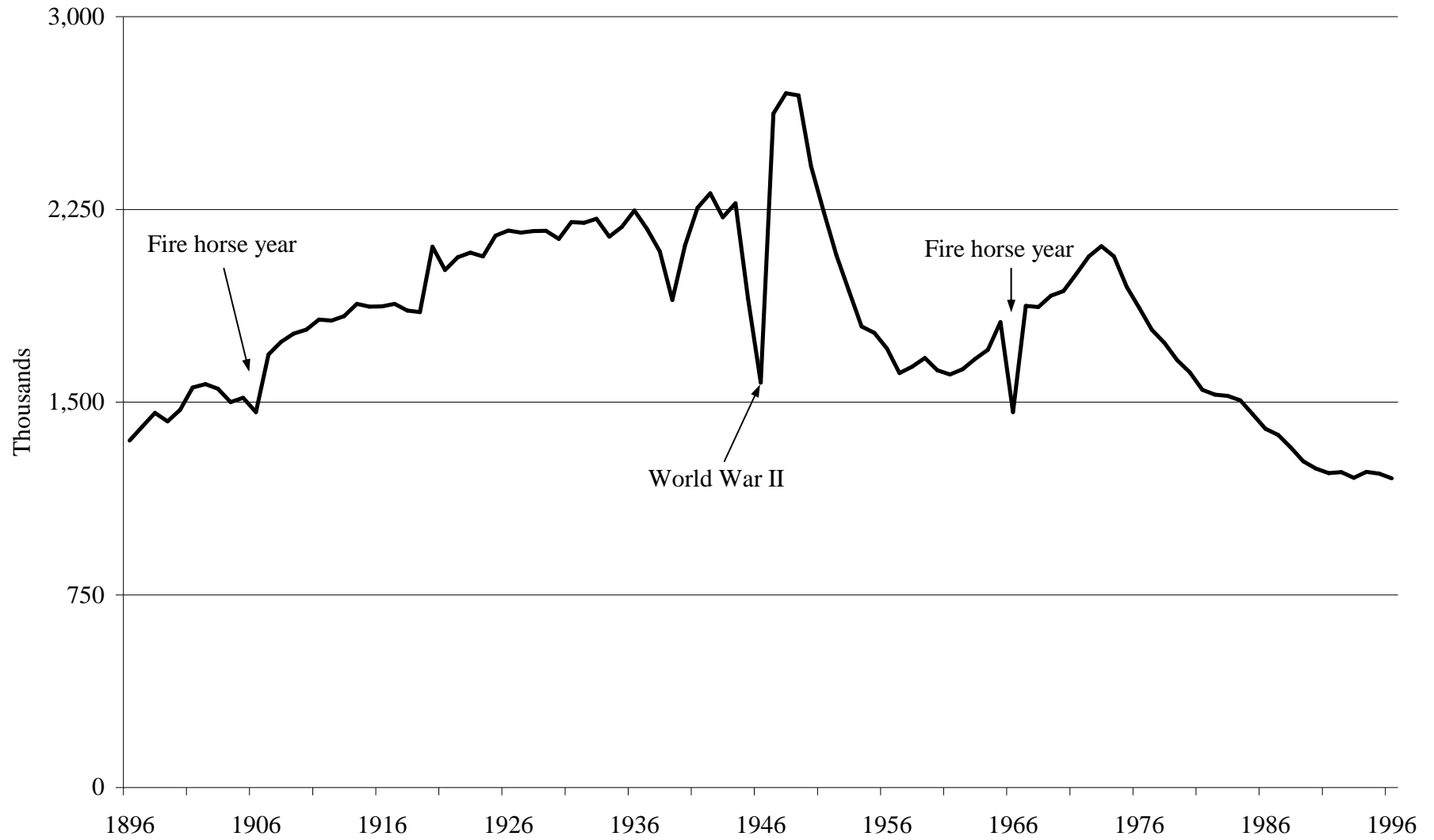
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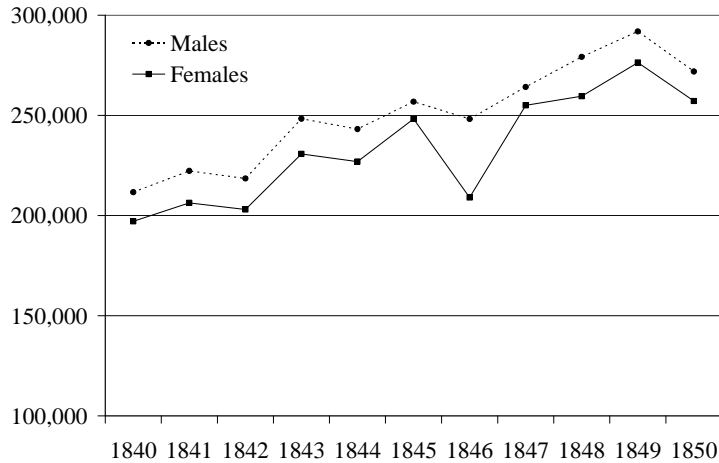
Figure 1: Live Births in Japan, 1896-1996



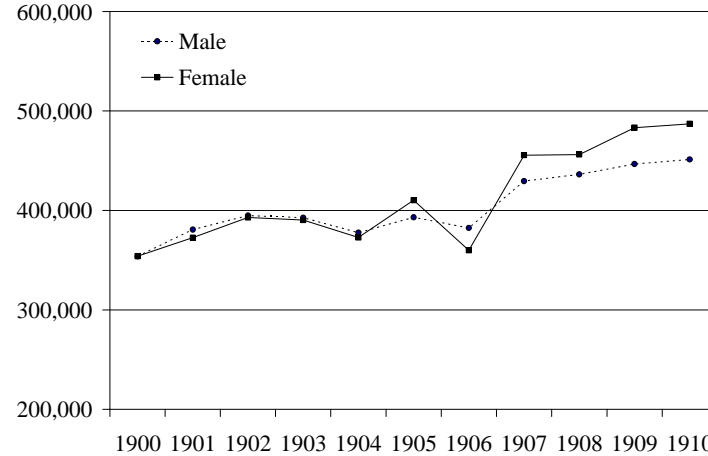
Notes: Source: Japan Statistical Yearbook, 1999. Annual births measured from 1 October to 30 September.

Figure 2: Population Size by Sex and Birth Cohort for Three Fire Horse Episodes

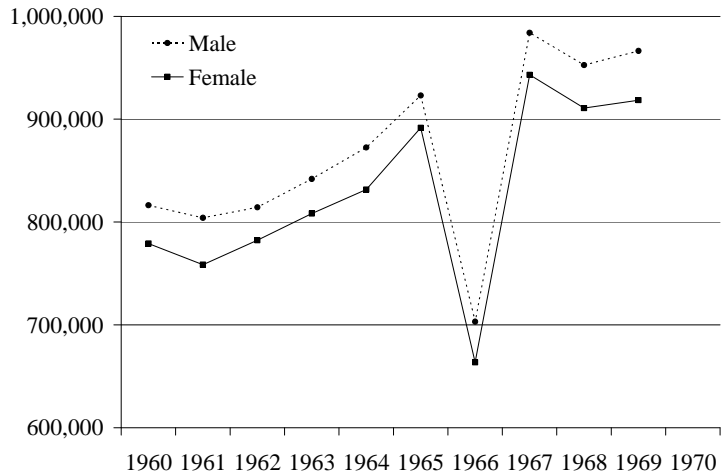
Panel A: 1840-1850 Birth Cohorts in 1886 Census



Panel B: 1900-1910 Birth Cohorts in 1955 Census



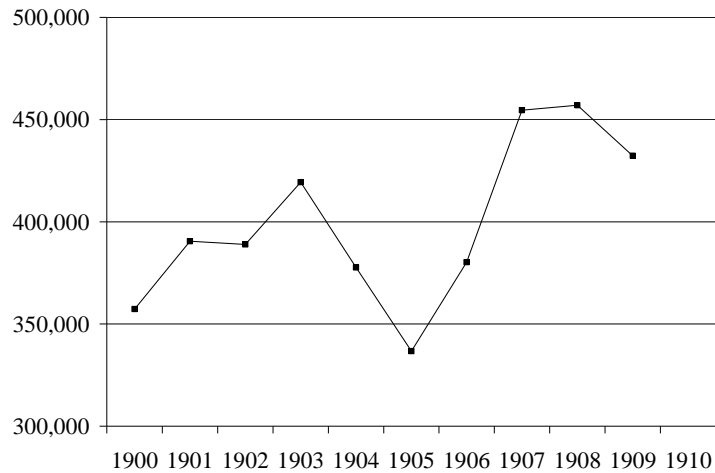
Panel C: 1960-1969 Birth Cohorts in 1970 Census



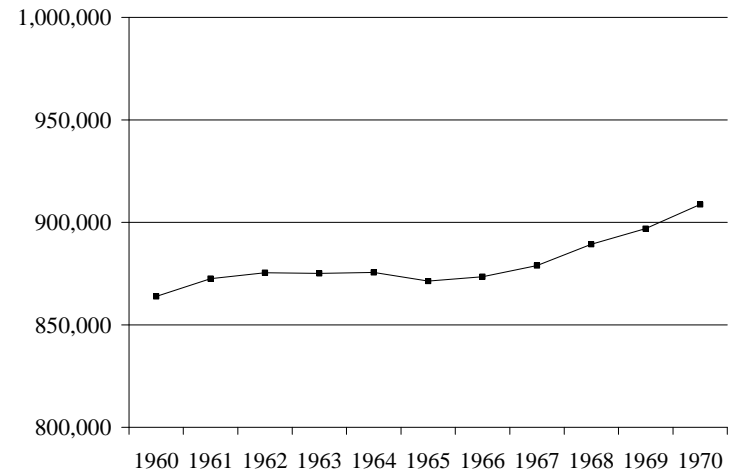
Notes: These graphs show male and female population size by birth cohort for three different fire horse episodes. The dotted line shows the male population, and the solid line shows the female population. The fire horse years studied are 1846, 1906, and 1966. The Census years are 1886, 1955, and 1970. Hence, the ages of the birth cohorts are different for the different points along each graph. For these three censuses, the three fire horse cohorts were 44, 39, and 4, years old respectively. Year is measured from January to December. Data sources: Japan. Ministry of Home Affairs (1886); Japan Statistics Bureau (1955, 1975).

Figure 3: Sex-Blind Child Avoidance Techniques

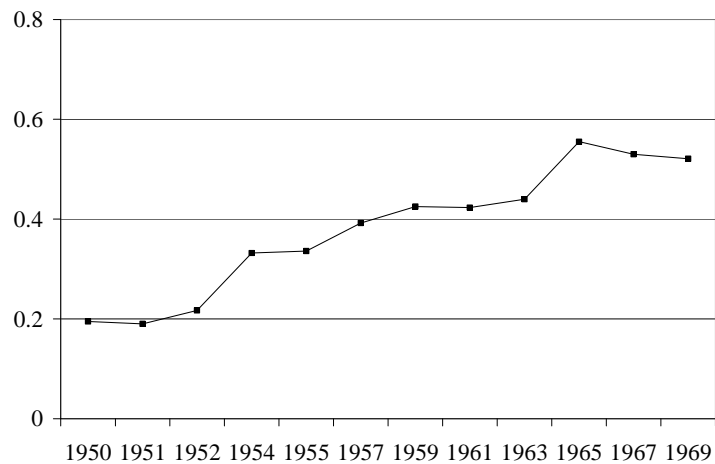
Panel A: Marriages, 1900-1909 (Year Beginning in April)



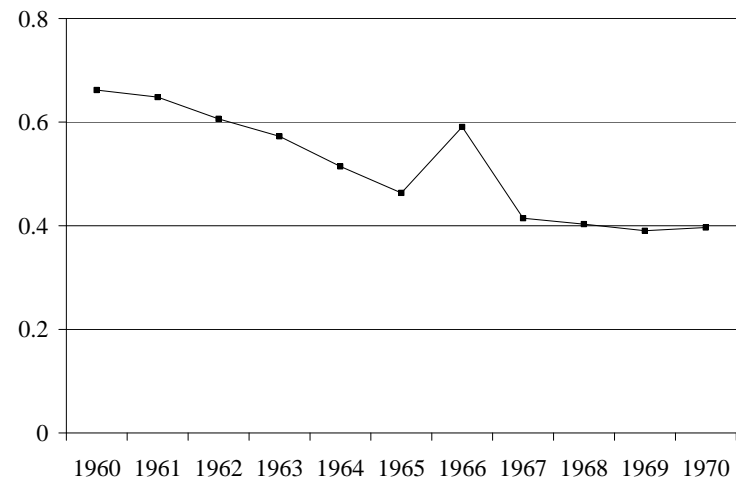
Panel B: Marriages, 1960-1970 (Year Beginning in April)



Panel C: Fraction of Wives Using Contraception



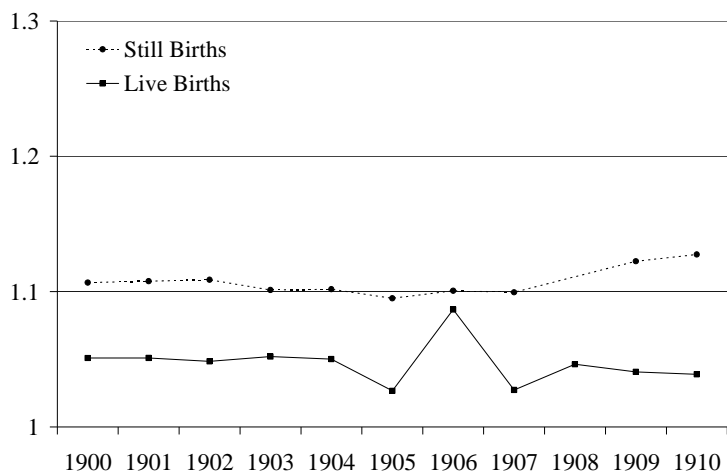
Panel D: Legal Abortions per Live Birth



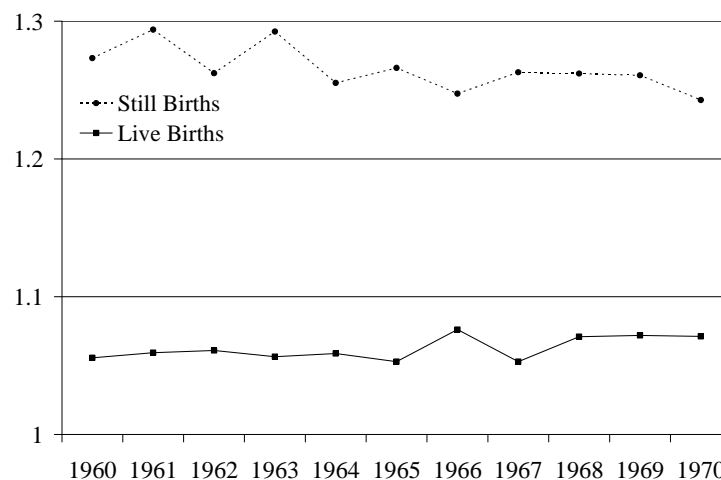
Notes: See notes to Figure 2 and Table 1. For Panels A and B, the year runs from 1 April to 31 March, nine months before the calendar year. The year is defined in this way so that 1905 and 1965 are the years when conceptions were likely to produce fire horse children. Sample for Panel C includes married women 50 and under. Contraception data are irregular. Sources described in the text.

Figure 4: Direct Measures of Infant Deaths

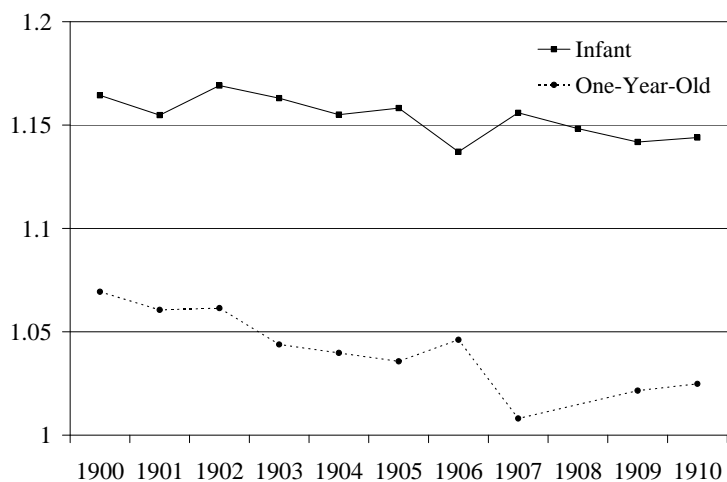
Panel A: Sex Ratio of Still and Live Births, 1900-1910



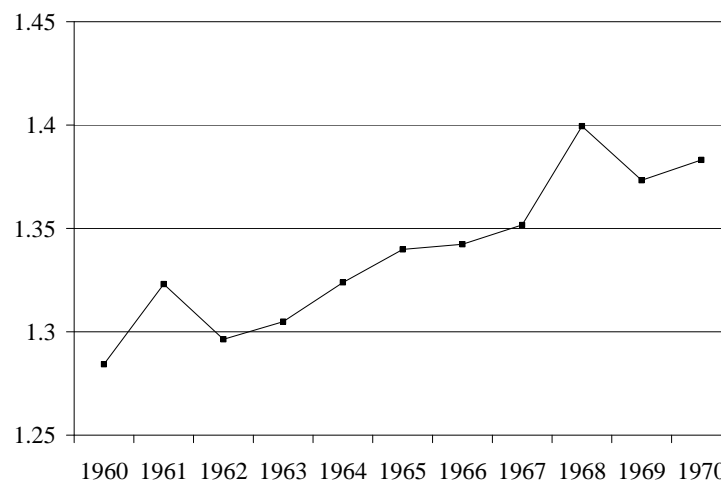
Panel B: Sex Ratio of Still and Live Births, 1960-1966



Panel C: Sex Ratios of Infant, & One-Year-Old Deaths, 1900-1910

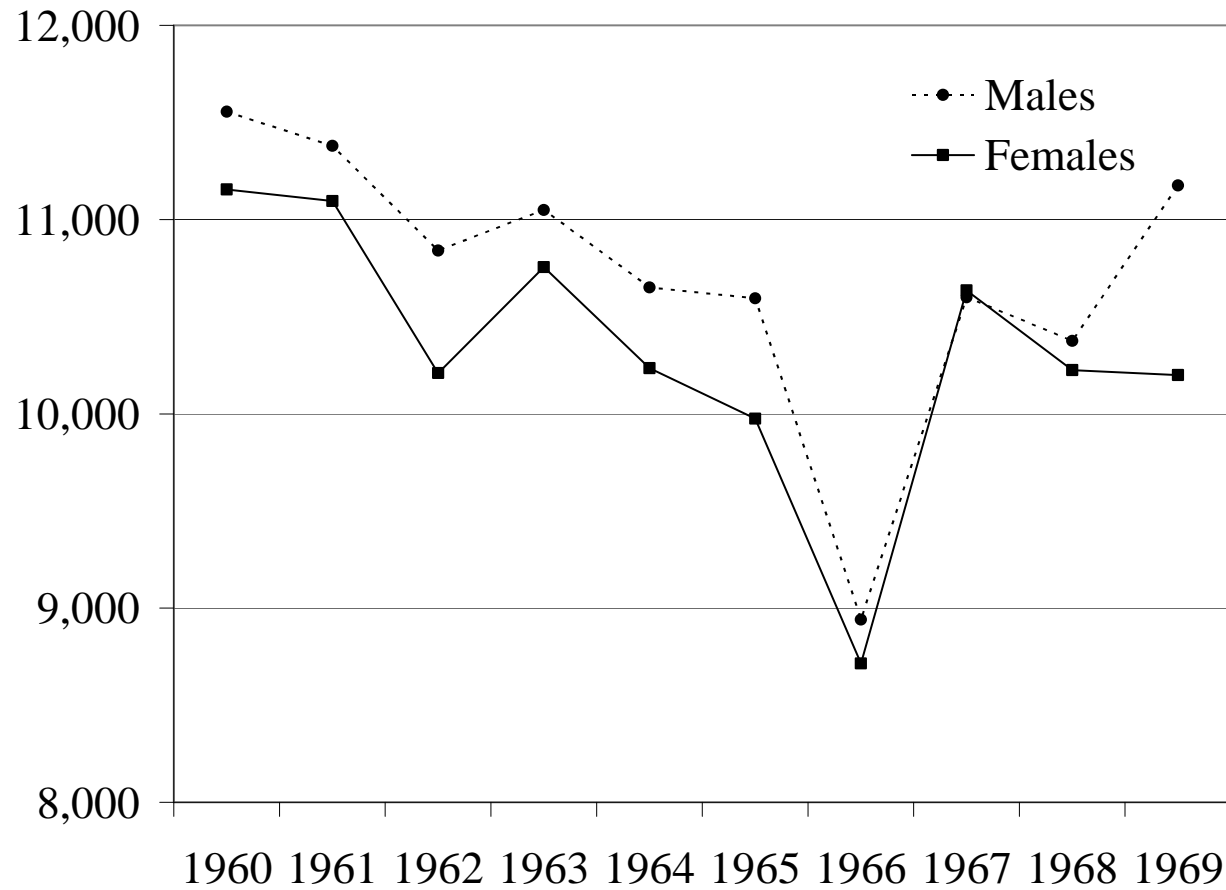


Panel D: Sex Ratios of Infant Deaths, 1960-1966



Notes: See notes to Figure 2 and Table 1. Male:female sex ratios shown. Still births in Panel B are measured as “late fetal deaths.” Data for still births (Panel A) and one-year-old deaths (Panel C) are missing for 1908. One-year-old deaths were rare in the 1960s. The sex ratio data are consequently very noisy and are not shown. Sources described in the text.

Figure 5: Fire Horse Episode in Okinawa, 1960-1969



Notes: See notes to Figure 2. Graph shows 1970 population size by sex and birth year for the prefecture of Okinawa, where abortion was illegal in 1960s. Additional details in the text.

Table 1: Descriptive Statistics,
Prefecture-Level Data

Area (km ²)	8,370 (11,900)
Population in 1890	918,000 (378,000)
Population in 1965	2,200,000 (1,930,000)
Per Capita Industrial Production in 1891	¥6.82 (5.02)
Per Capita GDP, Average, 1960-1969	¥290,000 (79,000)
Prefectures	45

Notes: See notes to Figure 1 and Table 1. Monetary amounts are expressed in nominal (unadjusted) yen. Per Capita GDP for 1960 to 1969 measured as average GDP from 1960 to 1969 divided by population in 1965. Standard errors measured from 45-observation (prefecture-level) dataset. For reasons of data availability, Nara and Osaka prefectures are added together and treated as one prefecture, and Kagawa and Ehime are also added together and treated as one prefecture. Sources described in the text.

Table 2: Sex-Selective Child Avoidance, Sex-Blind Child Avoidance, and Misrepresentation of Girls' Ages in Three Fire Horse Episodes, Japan 1840-1849, 1900-1909, and 1960-1969

Dependent Variable is Ln(Population Size) by Birth Year and Sex			
	(1)	(2)	(3)
Regressor	1846 Episode	1906 Episode	1966 Episode
$Fire\ Horse_c * Female_g$	-0.031 (0.012)**	0.003 (0.067)	-0.008 (0.005)
$Fire\ Horse_c$	-0.048 (0.037)	-0.059 (0.040)	-0.260 (0.024)**
Relabeling to Earlier Year	-0.025 (0.007)**	-0.007 (0.033)	-0.006 (0.003)**
Relabeling to Later Year	-0.045 (0.005)**	-0.068 (0.027)**	-0.006 (0.002)**
R ²	1.00	1.00	1.00
Observations	20	20	20
Total Population Across 10 Cohorts	4.8 million	8.0 million	15.4 million

Notes: See notes to Figure 1. Each column shows results from a different regression. The level of observation is the sex, birth year combination. Relabeling to Earlier Year is defined as $Fire\ Horse_c * Female_g$ minus the lead of $Fire\ Horse_c * Female_g$. Relabeling to Later Year is defined as $Fire\ Horse_c * Female_g$ minus the lag of $Fire\ Horse_c * Female_g$. Data are the same as in Figure 1. All three regressions include controls for female, year dummies with the exception of two years before and two years after the fire horse year. Regressions are corrected for within-period correlation across sexes and sex-specific AR(1) terms. Additional details in the text.

Table 3: Interaction of Fire Horse Effects with Economic Activity and Population Density
 Dependent Variable is Ln(Population Size) by Birth Year, Sex, and Prefecture (Obs = 900)

Regressor	(1) 1846 Episode	(2) 1966 Episode
<i>Fire Horse_c * Female_g</i>	-0.034 (0.010)**	-0.004 (0.007)
<i>Fire Horse_c * Female_g * per Capita Industrial Production_p</i>	0.005 (0.002)**	
<i>Fire Horse_c * Female_g * per Capita GDP_p</i>		6.8*10 ⁻⁹ (1.6*10 ⁻⁷)
<i>Fire Horse_c * Female_g * Population Density_p</i>	-1.1*10 ⁻⁴ (0.7*10 ⁻⁴)	-0.007 (0.011)
<i>Fire Horse_c</i>	-0.053 (0.033)	-0.275 (0.007)**
<i>Fire Horse_c * per Capita Industrial Production_p</i>	-0.0019 (7.8*10 ⁻⁴)**	
<i>Fire Horse_c * per Capita GDP_p</i>		-4.7*10 ⁻⁷ (2.3*10 ⁻⁷)**
<i>Fire Horse_c * Female_g * Population Density_p</i>	2.9*10 ⁻⁵ (2.9*10 ⁻⁵)	0.048 (0.005)**
R²	1.00	1.00

Notes: See notes to Figure 2 and Tables 1 and 2. GLS regressions adjust for random prefecture by sex interactions and an AR(1) term. Regressions control for *Fire Horse_c * Female_g*, both forms of relabeling from Table 1, and prefecture by sex fixed effects. Interacted variables (*Per Capita GDP_p*, *Per Capita Industrial Production_p*, and *Population Density_p*) are interacted with all controls. Industrial production, GDP, and population density variables are demeaned, so that the main effect can be interpreted as the effect for the average prefecture. Additional details in the text.

Table 4: Interaction of 1966 Fire Horse Effect with Okinawa Dummy

Dependent Variable is Ln(Population Size) by Birth Year, Sex, and Prefecture		
	(1)	(2)
Regressor	1966 Episode	
$Fire\ Horse_c * Female_g * Okinawa_p$	0.052 (0.019)**	0.073 (0.028)**
$Fire\ Horse_c * Okinawa_p$	0.111 (0.009)**	0.057 (0.033)*
Control for <i>Per Capita GDP_p</i> ?		Yes
Control for <i>Population Density_p</i> ?		Yes
R ²	1.00	1.00
Observations	900	900

Notes: See notes to Figure 2 and Tables 1, 2, and 3. Regressions are the specifications as in Table 2. Regressions control for $Fire\ Horse_c * Female_g$ and both forms of relabeling from Table 1. Interacted variables ($Okinawa_p$, *Per Capita GDP_p*, and *Population Density_p*) are included in levels and are interacted with all controls. For 1966 episode, per capita income is measured as the unweighted average of annual prefecture-level GDP from 1960 to 1969. *Per Capita GDP_p* and *Population Density_p* are demeaned, so that the main effect can be interpreted as the effect for the average prefecture. Additional details in the text.

Table 5: Sex Ratio of Marginal Children

	(1)	(2)	(3)
	% Drop in Cohort Size in Fire Horse Year	Sex Ratio of Marginal Children	Sex Ratio of Surrounding Cohorts
1. 1846 Episode	0.061 (0.036)	0.660 (0.217)	1.072 (0.002)
2. 1906 Episode	0.056 (0.052)	1.039 (0.210)	0.988 (0.013)
3. 1966 Episode	0.232 (0.018)	1.021 (0.018)	1.047 (0.001)
4. Okinawa, 1966	0.127 (0.009)	1.516 (0.220)	1.042 (0.003)
1846 Episode, by Per Capita Industrial Production in 1891			
5. Lowest, 1846 (Avg. = ¥3.5)	0.061 (0.037)	0.547 (0.264)	1.074 (0.004)
6. Middle, 1846 (Avg. = ¥5.2)	0.054 (0.035)	0.620 (0.252)	1.081 (0.002)
7. Highest, 1846 (Avg. = ¥12.5)	0.067 (0.036)	0.822 (0.165)	1.061 (0.001)
1966 Episode, by Per Capita GDP			
8. Lowest, 1966 (Avg. = ¥215,000)	0.207 (0.012)	1.009 (0.059)	1.046 (0.003)
9. Middle, 1966 (Avg. = ¥285,000)	0.240 (0.014)	1.044 (0.002)	1.041 (0.029)
10. Highest, 1966 (Avg. = ¥432,000)	0.234 (0.030)	1.017 (0.038)	1.050 (0.002)

Notes: Parameters calculated from the regression results in Tables 2 to 4, using formulas from part D of Section III. Fractional Drop in Cohort Size measures the proportional difference in population (male and female) between the fire horse cohort and surrounding cohorts. Sex Ratio of Marginal Children estimates the sex ratio of children who would have been born if not for the fire horse year. Standard errors calculated using the delta method. Additional details in the text.