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Effect of Introduction of Women's Suffrage on Infant Mortality

Supervisor:
Prof. Carolina Castagnetti

Student:
Amelie Caroline Bannwarth

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Abstract

This paper examines the effect of the introduction of women's suffrage on infant mortality rates across Swiss cantons over the period 1959 to 2000. Switzerland's system of direct democracy and the staggered adoption of cantonal women's suffrage provide a natural experimental setting well suited for causal identification. The analysis employs a classical difference-in-differences framework alongside the Callaway and Sant'Anna (2021) staggered treatment estimator, which explicitly accounts for treatment effect heterogeneity. The standard DiD estimator yields a statistically insignificant coefficient, reflecting distortion caused by heterogeneous treatment effects across adoption cohorts. By contrast, the Callaway and Sant'Anna estimator identifies a negative and statistically significant effect, indicating that the introduction of women's suffrage led to a meaningful reduction in infant mortality. Mechanism analysis suggests that this effect operates primarily through increased cantonal health care investment and rising female secondary school enrollment. These findings contribute to the literature by providing one of the first applications of the Callaway and Sant'Anna estimator to the women's suffrage and child health literature, and by demonstrating that conventional estimators can produce sign-reversed results in this setting.

Questo articolo esamina l'effetto dell'introduzione del suffragio femminile sui tassi di mortalità infantile nei cantoni svizzeri nel periodo compreso tra il 1959 e il 2000. Il sistema svizzero di democrazia diretta e l'adozione scaglionata del voto alle donne a livello cantonale offrono un contesto sperimentale naturale ideale per l'identificazione causale. L'analisi utilizza un approccio classico di difference-in-differences (DiD) insieme allo stimatore per trattamenti scaglionati

di Callaway e Sant'Anna (2021), il quale tiene esplicitamente conto dell'eterogeneità degli effetti del trattamento. Lo stimatore DiD standard restituisce un coefficiente statisticamente non significativo, riflettendo la distorsione causata dagli effetti eterogenei del trattamento tra le diverse coorti di adozione. Al contrario, lo stimatore di Callaway e Sant'Anna identifica un effetto negativo e statisticamente significativo, indicando che l'introduzione del suffragio femminile ha portato a una riduzione rilevante della mortalità infantile. L'analisi dei meccanismi suggerisce che questo effetto opera principalmente attraverso l'aumento degli investimenti cantonali nella sanità e la crescita delle iscrizioni femminili alla scuola secondaria. Questi risultati contribuiscono alla letteratura offrendo una delle prime applicazioni dello stimatore di Callaway e Sant'Anna al filone di studi sul suffragio femminile e sulla salute infantile, e dimostrando che gli stimatori convenzionali possono produrre risultati di segno opposto in questo contesto.

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1 Introduction

Infant mortality serves as a critical indicator of social inequality and structural health care deficits (Deichmann, 2024). Cross-national evidence shows that female literacy and health expenditure rank among the strongest structural determinants of infant mortality, underscoring the importance of simultaneous investment in female education and health infrastructure to reduce infant deaths (Dhrifi, 2019). As previous research has shown that women tend to favor family-friendly legislation and policies that improve women's position in society, women's suffrage may constitute a structural pathway through which health expenditure and female education are influenced (Besnier, 2020). Through participation in elections, women can directly influence the allocation of government expenditure. Given their preferences for policies supporting health care and families, increased political representation may therefore shift public expenditure towards areas that benefited women and infants.

This paper investigates whether the introduction of women's suffrage affected the infant mortality rate. The analysis was conducted at the canton level in Switzerland¹, within a time period from 1900 until 2000. The research setting in Switzerland is particularly interesting given its system of direct democracy. The system of direct democracy provides Swiss citizens the possibility to directly influence laws and political measures through referendums and initiatives (Swissinfo.Ch, 2025). With the introduction of women's suffrage, women can launch or support initiatives aimed at improving health care, working conditions or social security. Furthermore, women can directly influence legislation through referendums by voting in favor of proposals relating to those areas. I expect to find a negative effect of the introduction of women's suffrage on infant mortality rates across cantons. The effect is expected to operate

¹A canton in Switzerland is an administrative unit with its own constitution, legislature, executive, and judiciary (Editors, 2008). The names of the cantons follow the official names stated in Art. 1 of the Federal Constitution of the Swiss Confederation (der Schweizerischen Eidgenossenschaft, 2024).

through channels such as increased government expenditure, improved health care provision, and rising female educational attainment rather than an immediate legislative shock.

This paper proceeds as follows: Section 2 provides an overview of the current state of research. Section 3 shows the historical development of women’s suffrage in cantons and at national level. In Section 4, outlines the details of the data compilation and processing procedures. Followed by Section 5, which states the main hypothesis of the research that will be analyzed and the hypothesis of potential treatment heterogeneity. Section 6 presents a comprehensive exposition of the empirical strategy. The empirical strategy begins with a classical static difference-in-differences estimator and a dynamic event study, before extending the analysis to a staggered treatment design to account for variation in the timing of treatment adoption across cantons and potential heterogeneous treatment effects. After outlining the empirical strategy, Section 7 presents the results of the analysis. In Section 8, the results are summarized and put into context of current research, furthermore the section outlines potential limitations associated with the analysis.

2 Literature Review

Overall, there has been limited research examining the relationship between women’s political empowerment and children’s health (Besnier, 2020). Existing evidence suggests that the association between political empowerment and child health is highly context-dependent and varies across countries (Besnier, 2020). According to her, the political empowerment of women encouraged politicians to include and focus on policies that correspond to women’s preferences, needs, and concerns. Furthermore, she argues that women seem to prefer legislation that is family-friendly and improves the position of women in society. Miller (2008) found in his

research that within a year after the introduction of women's suffrage law enactment patterns shifted and local public health spending increased by around 35 %. Consistent with these findings, Black et al. (2016) highlight that the health of children under five years is particularly vulnerable and strongly influenced by the conditions and resources available to their caregivers. Caregiving is mostly undertaken by women, which is why their health status affects not only their well-being but also that of the child (Besnier, 2020). Therefore, empowering women is also beneficial for future generations, and lack of empowerment regarding social determinants and psychological factors leads to poorer health of the society.

Evidence from the cross-national study by Swiss et al. (2012) supports the association between an improvement in child survival and the percentage of women in parliament. In addition to that, Homan (2017) found a negative relationship between infant mortality and gender inequality in the American state legislature in a period between 1990 and 2012. Further results were found by Miller (2008), who observed a decrease from 15 to 8 % in child mortality with the introduction of women's suffrage, but no changes in infant mortality. Research further indicates that in low- and middle-income countries, women's political participation has shown a positive effect on child and maternal health (Besnier, 2020). In India, Bhalotra and Clots-Figueras (2014) observed a wide-ranging association between infant mortality and an increase in the number of women in state legislatures, especially for neonatal mortality. However, political empowerment should not focus only on mothers, but on the societal level, which means the empowerment of the whole female population. The reason behind this is that, besides the mother, other female family members might affect children's health too (Besnier, 2020).

3 Historical Background of Women's Suffrage in Switzerland

Across Europe, a movement towards more equal treatment of women in a political environment could be observed during the late nineteenth and twentieth centuries. This development can be attributed to the events of World War I and World War II, which helped pave the way for the introduction of women's suffrage (Stegmüller, 2025). With the loss of lives, especially from the male population during both world wars, women played a major role in rebuilding society after the war. The introduction of women's suffrage was regarded as a form of recognition for women's contributions in many countries that participated in the world wars (Stegmüller, 2025). The first European country to grant women the right to vote was Finland in 1906 (Von Wyl, 2024), 42 years before universal suffrage became a human right in 1948 (Eugster, 2024). One of the last European countries to introduce women's suffrage was Switzerland, which granted women the right to vote on 7 February 1971. This took place 53 years after Germany, 52 years after Austria, 27 years after France and 26 years after Italy ("Alle Schweizer Kantone Übersicht der 26 Regionen", n.d.). Compared to other European countries, the two world wars, in which Switzerland mostly avoided participating, played a minor role in the improvement of women's political position in the country (Eugster, 2024). This can be regarded as one of the reasons why Switzerland introduced women's suffrage later than other European countries, particularly in comparison with those that had actively taken part in both world wars. Another contributing factor, which is said to be the main reason for the late introduction of women's suffrage, is the political system of direct democracy ("As if women didn't exist" – Switzerland", n.d.). Amendments to the law require the approval of a majority of the electorate, which, prior to 1971, consisted exclusively of the male population.

The first step towards the introduction of women's suffrage was taken in 1868, when women in the canton of Zürich called for women's suffrage to be introduced. This initiative took place a century before its introduction at the national level and emerged within the context of the revision of the cantonal constitution, although it ultimately failed to gain approval ("Alle Schweizer Kantone Übersicht der 26 Regionen", [n.d.](#)). The movement continued to develop in 1893, when the Swiss Federation of Women Workers also unsuccessfully called for both women's suffrage and the right to stand for election. The first political party to support efforts to secure women's suffrage was the Socialist Party (SP) starting in 1904.

The movement gained further organizational strength in 1909, when several associations formed the Swiss Association for Women's Suffrage (ASSF) ². As a response to the growing movement, two motions demanding women's suffrage were submitted to the National Council in 1918. The National Council referred them to the Federal Council, but they were left unaddressed ("Alle Schweizer Kantone Übersicht der 26 Regionen", [n.d.](#)). Following this attempt, ASSF submitted the first petition on national level for women's suffrage to the Federal Court with 249,237 signatures (78,840 men and 170,397 women) in 1919, which was collected by women's associations, the SP and trade unions, to the Federal Chancellery. The petition was forwarded by the Parliament, but proved to be unsuccessful too ("Alle Schweizer Kantone Übersicht der 26 Regionen", [n.d.](#)). During the 1930s, the women's suffrage movement experienced a period of stagnation, as a consequence of the economic crisis and the resulting increase in support for conservative and fascist tendencies, where a key element is that women should primarily be regarded as housewives and not receive more rights ("Alle Schweizer Kantone Übersicht der 26 Regionen", [n.d.](#)). Between 1919 and 1956, 29 cantonal votes on the introduction of women's suffrage were rejected (Kaufmann, [2024](#)). Among the cantons that

²One of the two organisations advocating for women's suffrage. The organisation was became the Swiss Association for Women's Rights in 1971 as a result of the introduction of womens suffrage.(Wikipedia contributors, 2026)

considered, but rejected the issue were Basel-Stadt, Basel-Landschaft, Genève, and Ticino in 1946; Zürich in 1947; Neuchâtel and Solothurn in 1948; and Vaud in 1951 (“Alle Schweizer Kantone Übersicht der 26 Regionen”, n.d.). The United Nations Commission on the Status of Women, which was established in 1946, on a regular basis criticized Switzerland’s refusal to introduce the right to vote for women in their reports. This criticism led to negative headlines in international media, though this did not prompt the Swiss Government to reconsider their perception on women’s political rights (Kaufmann, 2024). At the same time, pressure from Swiss society and organizations such as ASSF, the Swiss Catholic Women’s League, and the Alliance of Swiss Women’s Societies, for women’s political rights continued to intensify (“Alle Schweizer Kantone Übersicht der 26 Regionen”, n.d.). These organizations had mobilized against the government’s proposal to introduce a compulsory civil protection service for women. This proposal was a result of growing tension of the Cold War. They argued that it was unjust to impose additional civic duties on women while denying them fundamental political rights such as suffrage. As a response to growing public discontent, the Federal Council submitted a bill on women’s political participation in 1957. In 1958, the bill was approved by both chambers and passed on as a referendum to the public. At this point in time only men were allowed to vote in referendums. The referendum resulted in a rejection of the bill with 66.9 % of voters voting against the proposal (“Alle Schweizer Kantone Übersicht der 26 Regionen”, n.d.). Despite rejection on national level, the three cantons Vaud, Genève and Neuchâtel, where the male population voted in favor of introducing women’s suffrage, decided to introduce political rights for women at the cantonal and communal level. In 1966 the canton Basel-Stadt followed with the introduction.

The continuing negative headlines on Switzerland’s refusal to introduce women’s suffrage led to the possibility of damaging its international reputation. As a result, the Swiss government

voted again in 1968 on the introduction of women’s suffrage. In this vote, two-thirds of the men in the government voted in favor of women’s suffrage, which led to the introduction of women’s suffrage at national level in 1971 (Kaufmann, 2024). With the introduction, women were not only allowed to vote, but also stand as a candidate (Kaufmann, 2024).

However, the introduction of women’s suffrage at cantonal level did not take place solely in 1971, but over a period of 31 years. The first canton to give women the right to vote was Vaud in 1959 (““As if women didn’t exist” – Switzerland”, n.d.). It took more than thirty years and a ruling of the federal court until the last canton, Appenzell Innerrhoden, introduced the female right to vote in 1990 (Von Wyl, 2024). Table 1 and Figure 1 give a detailed chronology of women’s suffrage introduction at the federal and cantonal levels.

Table 1: Chronology of Women’s Suffrage in Switzerland at the Federal and Cantonal Levels.

Canton	Date	Notes
Vaud	1 Feb. 1959	First canton to adopt women’s suffrage.
Neuchâtel	27 Sep. 1959	
Genève	6 Mar. 1960	
Basel-Stadt	26 Jun. 1966	First German-speaking canton.
Basel-Landschaft	23 Jun. 1968	
Ticino	19 Oct. 1969	
Valais	12 Apr. 1970	
Lucerne	25 Oct. 1970	
Zürich	15 Nov. 1970	

Canton	Date	Notes
Switzerland (Federal)	7 Feb. 1971	Referendum passed at the national level.
Aargau, Fribourg, Schaffhausen, Zug	7 Feb. 1971	Adopted women's suffrage on the same day as the federal vote.
Glarus	2 May 1971	
Solothurn	6 Jun. 1971	
Bern, Thurgau	12 Dec. 1971	
St. Gallen	23 Jan. 1972	
Uri	30 Jan. 1972	
Schwyz, Graubünden	5 Mar. 1972	
Nidwalden	30 Apr. 1972	
Obwalden	24 Sep. 1972	
Jura	1 Jan. 1979	Suffrage carried over at the canton's founding; had held it since 1971 as part of Bern.
Appenzell Ausserrhoden	30 Apr. 1989	Adopted at the Landsgemeinde by a narrow show-of-hands vote.
Appenzell Innerrhoden	27 Nov. 1990	Imposed by the Federal Supreme Court after the male electorate rejected it at the Landsgemeinde.

The table presents the exact date when women received political rights. Cantonal dates refer to the introduction of women's voting rights in cantonal elections, which preceded or followed the federal introduction in 1971.

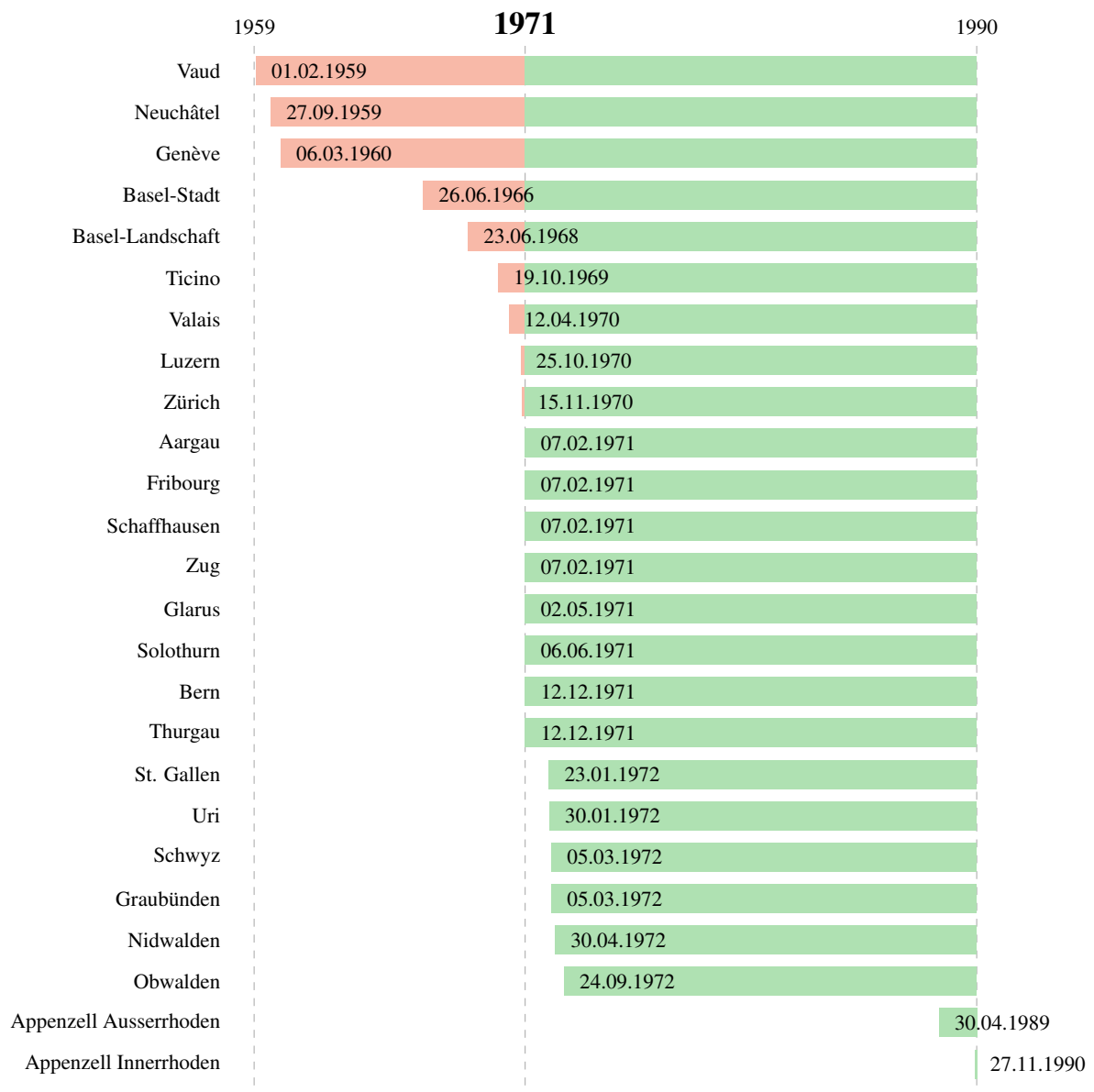


Figure 1: Chronology of women's suffrage introduction across the Swiss cantons relative to its adoption at the federal level (1971). Cantons shaded in red introduced suffrage *prior* to the national referendum, while those in green did so *afterwards*.

4 Data

Testing whether the timing of cantonal suffrage introduction is associated with changes in infant mortality requires observations that vary across cantons and over time. The analysis is therefore built on a time-series cross-sectional panel covering all 26 Swiss cantons for the time

period from 1900 to 2000, yielding 2781 canton-year observations before the data preparation process.

The dates on which women's suffrage was introduced in the each cantons are taken from Schweizerische Eidgenossenschaft. For the analysis, I use the year of suffrage introduction rather than the exact date to align with the annual frequency of infant mortality rate. The data collection was based on the Statistische Jahrbuch der Schweiz (Statistical Yearbooks of Switzerland), which has been published annually since 1891 by the Bundesamt für Statistik (Federal Statistical Office). Prior to its founding, the yearbook was provided by Statistical Bureau of the Federal Department of Home Affairs. These statistical yearbooks provide a comprehensive compilation of key statistical indicators related to Switzerland, its cantons, and its population. The yearbooks are available to download online ³ on the website of the Federal Statistical Office of Switzerland in the languages German and English.

The extracted data include the number of births, infant deaths, and stillbirths, as well as the population size for each canton, which were used to calculate the birth rate, infant mortality rate and stillbirth rate.

To examine the mechanisms through which suffrage affects infant mortality, this thesis analyses cantonal-level data on fiscal capacity, health care infrastructure, and female educational attainment as intermediate outcomes. Therefore, data at cantonal level on population, the number of physicians per capita, hospital beds, and hospitals were drawn from the statistical yearbooks. These variables are used to capture the medical provision at canton level.

Additionally, data on cantonal government's revenue and expenditure were extracted and used to calculate a proxy that indicates the general fiscal strength and public-investment capacity of a canton in a given year.

³<https://www.bfs.admin.ch/bfs/de/home/dienstleistungen/uebersichtsdarstellungen/statistisches-jahrbuch.html>
- the yearbooks are available in German and French

In a next step, data on the absolute number of students in primary and secondary schools were collected from the yearbooks. As part of this data extraction process, data on the number of female students in primary and secondary schools were extracted.

During the data preparation process, adjustments were required to account for changes in Switzerland's cantonal structure, particularly the transition from 25 to 26 cantons in 1979. The new founded canton Jura carries no observations until this point. DUE to its lack of data within the observed time period, Jura was excluded from the research.

Table 2: Description of Variables Included in the Analysis

Variable	Unit	Description
Canton	string	Name of the canton
Canton ID	numeric	Fixed numeric code for each canton (1–26) and the value 27 for the national aggregate row, see Table A2
year	date	Calendar year of the observation
Infant Death	numeric	Absolute number of children who died during the first year of life
Infant Mortality Rate	numeric	Percentage change in infant mortality – outcome variable used in the empirical analysis
Suffrage	Binary	Treatment variable – equals 1 if the canton has introduced women's suffrage, 0 otherwise
Stillbirth	numeric	Absolute number of stillborn children
Stillbirth Rate	numeric	Percentage change in stillbirth

Variable	Unit	Description
Births	numeric	Absolute number of births including live births and still-births
Birth Rate	numeric	
Population Size	numeric	Total number of people living in the canton/Switzerland
Canton Revenues	numeric	Total revenues in Swiss Francs
Canton Expenditures	numeric	Total spending in Swiss Francs
Financial Balance	numeric	Gross domestic product per capita in Swiss Francs
Female Primary Enrollment	numeric	Absolute number of female students in primary schools
Total Primary Enrollment	numeric	Absolute number of students in primary schools
Female Primary Share	numeric	Share of female population with primary education
Female Secondary Enrollment	numeric	Absolute number of female students in secondary schools
Total Secondary Enrollment	numeric	Absolute number of students in secondary schools
Female Secondary Share	numeric	Share of female population with secondary education
Physician per Capita	numeric	Average size of the population served by a single physician in a given area
Hospital Beds	numeric	Amount of hospital beds available
Hospitals	numeric	Number of hospitals
Health Index	numeric	Index to capture the level of health infrastructure

Overall, I used a set of observations $ct \in \Omega$, with a total amount of 2,525. The set of treated observations is specified by $\Omega_1 = (ct \in \Omega : D_{ct} = 1)$ of size N_1 and the set of untreated observations by $\Omega_0 = (ct \in \Omega : D_{ct} = 0)$ of size N_0 .

The dataset contained missing observations across several variables, with the respective pro-

portions of missing data reported in [Table 3](#). Especially for those variables that are used to examine underlying mechanisms, a high percentage of missing values could be observed. The highest percentages are observed for hospital beds and hospitals with around 70% of missing data. The same applies to the other control variables female secondary enrollment, physicians per capita, total secondary enrollment, female primary enrollment, total primary enrollment, which have between 40% and 50% of missing values. These missing values resulted from incomplete and inconsistent historical records in the statistical yearbook, which was used as the main data source. The limited availability of observed data presented a challenge in identifying a sufficiently robust underlying pattern to support the imputation of plausible values for the missing observations.

Missing values were addressed using multiple imputation for time-series cross-sectional data following Honaker and King ([2010](#)). For the purpose of deciding on the appropriate time trend specification (linear vs. polynomial) within the imputation model, the skewness of each variable was examined to determine whether a log-transformation should be applied prior to imputation ((Kaufmann, [2024](#))). It was found that the variables infant mortality rate, population, cantonal revenue and expenditure, hospital beds, hospitals and doctors per capita are right-skewed ($|s| > 1$), see [Table 4](#). The variables infant mortality rate, population size, canton revenue, canton expenditure, and revenue per capita exhibit pronounced right skewness (skewness values ranging from 1.26 to 5.16) and follow a multiplicative rather than additive process over the observed period.

To capture distributional properties within the imputation model, these variables were transformed to logs prior to the implementation of the imputation. Although they were right-skewed, the variables hospital beds ($s = 2.17$) and hospital ($s = 2.57$) were not log transformed, as the skewness largely results from cross-canton differences⁴. For the log-transformed variables, the

⁴Cross-canton differences are already absorbed by adding canton fixed effects via the `cs` - argument

Variable	Percentage Missing
hospital_beds	71.9%
hospitals	68.7%
female_secondary_enrollment	47.0%
female_secondary_share	47.0%
physicians_per_capita	46.5%
total_secondary_enrollment	45.6%
female_primary_enrollment	41.6%
female_primary_share	41.6%
total_primary_enrollment	40.6%
revenue_pc	14.8%
canton_revenue	12.9%
canton_expenditure	12.9%
birth_rate	9.86%
stillbirths	8.30%
stillbirth_rate	8.30%
infant_deaths	8.15%
infant_mortality_rate	8.15%
live_births	7.96%
population_size	4.91%
births_total	3.01%
year	0.00%
canton	0.00%
canton_ID	0.00%
suffrage_year	0.00%
suffrage	0.00%

Table 3: Percentage of missing observations by variable

imputation was done linear on the log scale and for the remaining variables the imputation was linear.

Table 4: Skewness of Observed Variables

Variable	Skewness	Log-transformed
Infant deaths	3.98	Yes
Live births	2.10	Yes
Population size	2.24	Yes
Total births	2.08	Yes
Canton revenue	4.55	Yes
Canton expenditure	4.49	Yes
Hospital beds	2.11	No
Hospitals	2.50	No
Female primary enrollment	2.26	No
Total primary enrollment	18.92	No
Female Secondary Enrollment	3.53	No
Total Secondary Enrollment	3.87	No
Physicians per capita	1.40	No

Notes: Skewness computed on observed (non-imputed) values. Variables with skewness $|s| > 1$ are candidates for log-transformation. Variables already bounded in $[0, 1]$ or with near-symmetric distributions are left in levels. Log-transformation is applied within the Amelia II imputation model via the `logs` argument.

After running the data imputation process, I tested whether the imputation model included meaningful data. The overimputation test takes 20 % of the observed data and temporarily hides them, pretending that these values are missing (Kaufmann, 2024). In a next step, the imputation model runs again on this artificially degraded dataset and tries to include those hidden values. The imputed data were compared to the true values to examine whether the imputation was successful.

Results from the test were illustrated in overimputation diagnostic plots⁵ which were produced for each imputed variable following the procedure outlined in (Kaufmann, 2024). If the imputation model is well-specified, the imputed values should cluster closely around the 45-degree line, and the true observed values should fall within the reported confidence intervals ((Kaufmann, 2024)).

⁵The plots are presented in the Appendix.

When examining the overimputation plot of infant deaths, total births, live births, population size, revenue, and expenditure, the imputed values cluster reasonably close around the 45-degree line, suggesting the imputation model recovers the true observed values with acceptable accuracy.

For the variables stillbirths, physicians per capita, hospital beds and hospitals, a moderate quality of imputed values was observed, which is likely due to the limited availability of the data (47% missingness).

Moderate quality of imputation could also be observed for variables relating to the number of female students in primary and secondary school and the total number of students in secondary school. For the total number of primary students, the data imputation process was insufficient. Therefore, I decided to focus on the number of students (female and male) in secondary schools to avoid potential bias in the estimation.

Following the imputation of the missing data, the infant mortality rate was calculated:

$$IMR = \frac{\text{Number of infant deaths (under age 1)}}{\text{Number of live births}} \times 1000 \quad (1)$$

In order to analyze the direct effect of the introduction of women's suffrage on maternity the stillbirth rate (SBR) was calculated:

$$SBR = \frac{\text{Number of stillbirth deaths}}{\text{Number of live births}} \times 1000 \quad (2)$$

To ensure comparability of the education control variable across cantons, the ratio of female students (FSS) was calculated:

$$FSS = \frac{\text{Number of female students in secondary schools}}{\text{Total number of students in secondary schools}} \quad (3)$$

To capture variations in canton-level financial capacity, I controlled for canton financial balance (FB). The variable was calculated by subtracting expenditures from revenue for each canton. As the population size differs, comparing the financial balance across cantons will lead to bias. Therefore, to account for differences, financial balance was expressed on a per-capita basis:

$$FB = \frac{\text{Revenue} - \text{Expenditure}}{\text{population size}} \quad (4)$$

Since the variables physician per capita, hospital beds, and hospitals are likely to be correlated with each other, I decided to build a health index, which includes information from all three variables. To ensure the ability to combine the variables, hospital beds and hospitals must be divided by the population size to a per capita basis, because data on the number of physicians within a canton is available on per capita basis. To be able to combine the variable in one index variable, each needs to be standardised:

$$x_c^* = \frac{x_c - x_{\min}}{x_{\max} - x_{\min}} \times 100 \quad (5)$$

where x_c is the observed value of canton c , $x_{\max}$ the maximum and $x_{\min}$ the minimum observed value for the cantons. x_c^* is the standardised value on a scale from 0 to 100.

The potential problem that can arise when standardising the variables is that one canton might have an unusually high number of physicians, hospital beds or hospitals. This can cause other cantons' standardised values to be squished downwards. To avoid using any outliers for the standardisation, the 1st percentile is used for the minimum value and the 99th for the maximum

value.

When constructing the index from the standardized variables, the variable hospital beds was weighted at 50%, as this plays an important role in childbirth and the method of imputation of missing values worked best here, meaning the available data is the most reliable out of the three. For the other two variables we have a moderate quality for the data imputation process. However, physicians tend to play a more important role during a pregnancy for a mother's health. Therefore, physicians received a weight of 30% and hospitals 20%. The health index was calculated as follows:

$$HealthIndex = 0.5 \times hospitalbeds + 0.3 \times physicians + 0.2 \times hospitals \quad (6)$$

The index ranges from 0 to 100, where 0 represents no health care and 100 represents the highest level of health care provision to the population.

5 Hypothesis

Hypothesis 1 (Women's Suffrage Effect) - Main Hypothesis: *The introduction of women's suffrage decreases infant mortality rates in Swiss cantons.*

Granting women the right to vote increased their political influence and altered policy priorities. Through Switzerland's system of direct democracy, the introduction of women's suffrage enabled women to directly influence legislation and public spending decisions by participating in referendums and initiatives. This encourages politicians to introduce more legislative proposals relating to the areas that play an important role to women, in order to win women over as voters. Research by Besnier (2020), indicated that women may place greater emphasis on issues related to maternal and child health, access to health care, social welfare, and family

support programs than men. As a result, the introduction of women's suffrage may have led to policies and investments that improved prenatal care, maternal health services, nutrition, and living conditions for families, ultimately contributing to lower infant mortality rates.

Hypothesis 2 (Treatment Heterogeneity): *The effect of women's suffrage on infant mortality varies across cantons.*

The impact of women's suffrage depends on the political responsiveness of cantonal governments and the population before the introduction of women's suffrage. In cantons with more progressive political preferences, typically characterized by stronger support for redistribution, public health spending, and social welfare policies, the introduction of women's suffrage is more likely to translate into actual policy change. In these environments, political institutions and voters are already more receptive to interventions aimed at improving social outcomes such as health care access, child welfare, and poverty reduction. As a result, when women gain the right to vote, their preferences ⁶ are more easily integrated into existing policy debates and legislative decisions. By contrast, in more conservative cantons with lower support for social spending, even though women gain formal voting rights, their ability to influence policy may be constrained. Political opposition to redistribution or welfare expansion can weaken the translation of suffrage into effective policy changes, limiting its impact on outcomes such as infant mortality.

In addition to the political differences between cantons, there are also differences in wealth. Wealthier cantons have more financial resources to improve health care provision.

The year of introduction may affect the magnitude of the effect. The treatment effect may be larger than in cantons that introduced it earlier.

As a result, responses to the introduction of women's suffrage might differ between cantons,

⁶Women often focus on areas such as family welfare, health, and social services

leading to treatment heterogeneity.

6 Empirical Strategy

This section outlines the empirical framework used to test the hypotheses developed in Section 5. The empirical strategy relies on a natural experiment, as the introduction of women's suffrage was not driven by research considerations (Leatherdale, 2018). Another feature of a natural-experiment is that the policy was not randomly assigned, but rather resulted from the legal introduction of women's suffrage. A key advantage of the natural experiment is the generation of timely practice-based evidence, which is important to evaluate government policies. However, when analyzing a natural experiment, the timing must be taken into account, as it can lead to a bias in the results (Leatherdale, 2018). One reason for this may be that insufficient data was collected during the observed period. As an initial step, a difference-in-differences (DiD) estimator was applied using a baseline specification, followed by the inclusion of control variables. Although DiD provides an estimate of the average treatment effect across all post-treatment periods, it does not account for heterogeneous treatment effects arising from staggered adoption. Therefore, an event study was conducted to examine the dynamic profile of the treatment effect and to test the parallel trends assumption. Finally, a staggered treatment design was employed to address the heterogeneity bias inherent in Two-way-fixed-effects (TWFE) under staggered adoption.

6.1 Baseline Difference-in-Differences (DiD)

6.1.1 DiD Regression

As an initial step, I evaluated the impact of the introduction of women's suffrage on infant mortality rate, using a static DiD approach. The decision is motivated by its ability to address the problem that randomization of participants to the treatment and control group is not possible. In the absence of random assignment, alternative econometric strategies are required to address time trends, as well as unobserved, time-invariant heterogeneity between treatment and control groups. The DiD framework is commonly used in this context, as it allows for the estimation of treatment effects while accounting for both common time shocks and fixed group-level differences (Wing et al., 2018). This implementation is based on the assumptions of common trend and strict exogeneity. The assumption of a common trend is based on the premise that the treatment group and the control group would have followed the same path in the absence of treatment (Wing et al., 2018). If this holds, then the estimated results are not biased. The assumption of strict exogeneity implies that the timing of treatment must be statistically independent of the possible outcomes (Wing et al., 2018). Under strict exogeneity, future treatments ($t+1$) cannot be anticipated or related to outcomes of previous or current periods.

The DiD captures the change in infant mortality in the treated group, relative to the contemporaneous changes in the control group.

$$\begin{aligned}
ATT &= E[IMR_{ct} \mid D_c = 1, t = \text{post}] - E[IMR_{ct} \mid D_c = 1, t = \text{pre}] \\
&\quad - \left(E[IMR_{ct} \mid D_c = 0, t = \text{post}] - E[IMR_{ct} \mid D_c = 0, t = \text{pre}] \right) \\
&= \left[(\alpha + \beta + \gamma_1 + \delta_{\text{post}}) - (\alpha + \gamma_1 + \delta_{\text{pre}}) \right] \\
&\quad - \left[(\alpha + \gamma_0 + \delta_{\text{post}}) - (\alpha + \gamma_0 + \delta_{\text{pre}}) \right] \\
&= \beta.
\end{aligned} \tag{7}$$

It shows that the DiD estimator β recovers the average treatment effect on the treated (ATT), which is captured by the coefficient β . Thus, β identifies the ATT and can be obtained from:

$$IMR_{ct} = \alpha + \beta \times \text{suffrage}_{ct} + \lambda_c + \delta_t + \varepsilon_{ct} \tag{8}$$

The outcome variable infant mortality rate is denoted by IMR_{ct} for canton c at time t . The treatment is denoted by the indicator variable suffrage_{ct} , which takes the value one if the canton introduced women's suffrage and zero if it did not introduce it. Here, β specifies the average treatment effect on the treated (ATT) under the parallel trend assumption. Fixed effects are identified by λ_c , which refers to canton fixed effects and δ_t to year fixed effect. Based on the basic DiD regression, the null hypothesis of no effect of the introduction of the women's suffrage on the infant mortality rate was tested.

$$H_0 : \beta = 0 \tag{9}$$

It is tested against the alternative hypothesis H_1 , stating that the introduction of women's suf-

frage had an effect on the infant mortality rate.

$$H_1 : \beta \neq 0 \quad (10)$$

Assumption 1 - Stable Unit Treatment Value Assumption:

The assumption implies that, for each member of the population, only one of the potential outcomes is observable (Lechner, 2011). Having only one potential outcome for each member of the population suggests that there are no relevant interactions between them.

Assumption 2 - Exogeneity of Covariates:

The control variables are assumed to be exogenous and strictly unaffected by the treatment, to ensure that conditioning on covariates does not compromise the identification of the treatment effect (Lechner, 2011). Formally, the potential covariates under treatment and control are identical:

$$X_{ct}(1) = X_{ct}(0) = X_{ct}$$

Assumption 3 - Parallel Trend:

The assumption of parallel trends implies that in the absence of treatment, the treated ($D = 1$) and control ($D = 0$) groups exhibit the same time trends (Lechner, 2011).

$$\begin{aligned} E[Y_t(0) - Y_{t-1}(0) \mid X = x, D = 1] &= E[Y_t(0) - Y_{t-1}(0) \mid X = x, D = 0] \\ &= E[Y_t(0) - Y_{t-1}(0) \mid X = x] \end{aligned}$$

If the potential outcome for the treated and control groups shares the same time trend in

the absence of treatment, any deviation from the common time trend following the adoption of treatment can be attributed to the effect of treatment. Therefore, it is the key assumption of the DiD approach, as identification of the treatment effect requires the counterfactual difference ($E[Y_t(0) - Y_{t-1}(0)|X = x, D = 1]$) to equal the observed difference ($E[Y_t(0) - Y_{t-1}(0)|X = x, D = 0]$).

6.1.2 TWFE event study

To complement the static baseline DiD estimate, I estimate a dynamic specification that allows the treatment effect to vary freely across event-time periods k using an event study specification (Baker et al., 2025). This is possible as the ATT is calculated for each period k . The TWFE event study uses all other units as the comparison group and not explicitly define a comparison group. Also it does not distinguish between never-treated and not-yet-treated units. The parallel trends assumption is imposed globally and untested across cohorts.

The model is specified as:

$$IMR_{ct} = \sum_{k=-5, k \neq -1}^{10} \beta_k \mathbf{1}\{\text{suffrage}_{ct} = k\} + \lambda_c + \delta_t + \varepsilon_{ct} \quad (11)$$

The outcome variable infant mortality rate for canton c in year t is denoted by IMR_{ct} . For the purpose of absorbing time-invariant differences between cantons, canton fixed effects were included, denoted by λ_c . To identify time trends and shocks that affect cantons, year fixed effects were added, denoted by δ_t . As a reference period serves the period right before the introduction of treatment ($k \neq -1$). The variable of interest is β_k , representing the deviations of the infant mortality rate relative to the reference period. The indicator variable $\mathbf{1}\{\text{suffrage}_{ct} = k\}$ is a dummy variable for being k periods away from the treatment.

In order to obtain meaningful results, Assumption 2 of parallel trends needs to hold:

$$H_0 : \beta_{-5} = \beta_{-4} = \beta_{-3} = \beta_{-2} = 0 \quad (12)$$

The null hypothesis states that in the five periods prior to the introduction of women's suffrage the cantons follow the same trajectory of the infant mortality rate.

$$H_1 : \text{at least one } \beta_j \neq 0 \text{ for } j \in \{-5, -4, -3, -2\} \quad (13)$$

Using the specification outlined in [Equation 11](#), the following null hypotheses were tested to evaluate the statistical significance of the estimated effects:

$$H_0 : \beta_k = 0 \quad \forall k \geq 0 \quad (14)$$

$$H_1 : \beta_k \neq 0 \quad (15)$$

The null hypothesis is used to analyze if the introduction of the treatment had an effect on the outcome variable or not (Baker et al., [2025](#)).

6.1.3 Robustness Check

To conduct a robustness check, I accounted for the possibility that cantons with different pre-treatment mortality levels followed systematically different trends over time. This was implemented by including an interaction term, in which the average infant mortality rate prior to treatment $\overline{IMR}_{c,pre}$ is interacted with a time trend $(t - \bar{t})$. This allows the trajectory of infant

mortality to differ across cantons depending on their baseline level, thereby relaxing the assumption that all cantons would have followed parallel trends in the absence of suffrage. The extended specification for the DiD regression was defined as:

$$IMR_{ct} = \alpha + \beta \times suffrage_{ct} + \gamma_2 \times \overline{IMR}_{c,pre} \times (t - \bar{t}) + \lambda_c + \delta_t + \varepsilon_{ct} \quad (16)$$

And the extended specification to perform a robustness check for the TWFE event study model is as follows:

$$IMR_{ct} = \sum_{\substack{k=-5 \\ k \neq -1}}^{10} \beta_k 1 \{suffrage_{ct} = k\} + \gamma \cdot \overline{IMR}_{c,pre} \cdot (t - \bar{t}) + \lambda_c + \delta_t + \varepsilon_{ct} \quad (17)$$

6.2 Heterogeneous Effect

As the cantons granted women's suffrage at different times, they were also subject to the treatment for a varying length of time. Being treated earlier might lead to a larger effect of the treatment relative to cantons that were treated later (Callaway & Sant'Anna, 2021). The canton Vaud, which introduced the right to vote for women in 1959, has more time to propose and establish new laws concerning topics such as medical provision for pregnant women, maternity leave, and economic security for families, compared to cantons that introduced women's suffrage in 1972. Therefore, when aggregating ATT in a staggered framework, it will lead to a heterogeneous bias.

To consider heterogeneous effects, I used the formula proposed by Callaway and Sant'Anna (2021), the treatment for each cohort was conducted. It calculates the average effect of introducing women's suffrage $\theta(\tilde{g})$ among the cantons belonging to group $\tilde{g}$, across all their post-treatment periods (Callaway & Sant'Anna, 2021):

$$\theta(\tilde{g}) = \frac{1}{T - \tilde{g} + 1} \sum_{t=\tilde{g}}^T ATT(\tilde{g}, t) \quad (18)$$

The sum runs from $t = \tilde{g}$ (treatment year) to $t = T$ (end of panel). The respective component of the equation $\frac{1}{T - \tilde{g} + 1}$ is the averaging weight used to make the cohort's $ATT(\tilde{g}, t)$ comparable.

For the analysis, the cantons were grouped by the year they introduced women's suffrage in [\(Table A3\)](#).

6.3 Staggered Treatment Design

6.3.1 Group-Time Average Treatment Effects

In a classical DiD approach all treated units are assumed to be treated simultaneously. However, the cantons introduced women's suffrage at different points in time, starting in 1959 until all cantons had adopted it by 1990. When treatment was implemented at different locations and at different times, treatment adoption follows a staggered design (Callaway, [2022](#)). Therefore, using a TWFE event study might lead to biased results due to negative weights of the average treatment effect on treated (Callaway & Sant'Anna, [2021](#)). Some units that are treated in the middle of the time period are weighted more than those treated earlier or later.

Furthermore, the TWFE model only delivers unbiased estimates if the treatment effect is homogeneous across cantons (Callaway & Sant'Anna, [2021](#)). Since the timing of women's suffrage adoption varied across Swiss cantons, treatment effects are likely heterogeneous both across cohorts and over time. The group-time average treatment effect $ATT(g, t)$ proposed by Callaway and Sant'Anna ([2021](#)) accommodates this by estimating separate effects for each group of cantons that adopted suffrage in the same year, allowing the impact of enfranchisement on infant mortality to evolve dynamically rather than imposing a single homogeneous treatment

effect across all adopters. Another advantage of the group-time average treatment estimate is that it allows for potential treatment anticipation (Callaway & Sant'Anna, 2021).

Drawing on the assumptions outlined by Callaway and Sant'Anna (2021), the following assumptions are considered relevant to the analysis:

Assumption St.1 - Conditional Parallel Trends based on a "Not-Yet-Treated" Group:

The parallel trend assumption in a staggered treatment design is based on the research design that all the cantons eventually introduced women's suffrage at a given point in time. Unlike the parallel trend assumption based on the comparison between the trajectory of the treated group and control group in pre-treatment periods, this assumption compares the trajectory of the treated group g and the not-yet-treated group s by time $t + \delta$:

$$E[Y_t(0) - Y_{t-1}(0)|X, G_g = 1] = E[Y_t(0) - Y_{t-1}(0)|X, D_s = 0, G_g = 0]$$

where G refers to the set of all cohorts, which were grouped by their treatment year and G_g is an indicator variable that takes on the value 1, if the canton belongs to the cohort g . The total number of time periods in the panel was defined by τ and $\bar{g}$ is the last period before any unit is treated. In order to account for potential anticipation before treatment, the variable δ was included. The condition $t + \delta \leq s \leq \bar{g}$ restricts the comparison group to units that are not-yet-treated at time s .

Assumption ST.2 - Staggered Treatment Adoption:

The analysis is based on the assumption that once a unit is treated, it remains treated in the following periods. This means that the treatment status cannot be randomly assigned or altered. If a unit was treated in period t ($D_t = 1$) it will stay treated for future periods ($D_{t+T} = 1$).

$$D_{c,t} \begin{cases} 0, & \text{if the canton is not treated} \\ 1, & \text{if the canton is treated} \end{cases}$$

The right to vote is enshrined in cantonal legislation and would require a lengthy legal process and the approval of the majority of the population to be abolished. It can therefore be assumed that, once women have been granted the right to vote, it will remain in place for future elections.

Assumption St.3 - Absorbing Treatment Structure:

The treatment follows an absorbing structure, which implies that once a canton grants women the right to vote, the right cannot be reversed.

$$D_{c,t} = 1 \{t \geq G_c\}$$

G_c refers to the year canton c adopted women's suffrage and once $t \geq G_c$, the canton is treated for all subsequent periods:

$$D_{ct} = 1 \longrightarrow D_{c,t+1} = 1$$

Assumption ST.4 - Staggered Treatment Anticipation:

Assuming that there exists a known $\delta \geq 0$ such that

$$E[Y_t(g)|X, G_g = 1] = E[Y_t(0)|X, G_g = 1]$$

$$\delta = \begin{cases} 0, & \text{no anticipation of treatment effects,} \\ > 0, & \text{anticipatory effects are allowed before treatment.} \end{cases}$$

Within this framework, I assume the absence of anticipation effects. Despite the population being aware about the possibility of the cantonal government introducing women's suffrage, it is unlikely that the government would increase health spending accordingly. This leads to the assumption that there is no treatment anticipation ($\delta = 0$).

Assumption St.5 - Overlap:

In order to obtain meaningful results, there must be a sufficient number of comparable units in the various treatment and control groups.

For the purpose of calculating the average treatment effect (ATT) the reference period $t = g - \delta - 1$ was determined under Assumptions 3 and 4. The reference period is the most recent period during which untreated potential outcomes were observed for units in group g . It is used as a starting point to measure any changes in the outcome to estimate the treatment effect. As all cantons are eventually treated ($\bar{g} < 0$) at some point during the observed time period, the ATT can only be calculated before the last canton received women's suffrage ($t < \bar{g} - \delta$, which corresponds to Appenzell Innerrhoden. The $ATT(g, t; \delta)$ can be used to highlight the heterogeneity of the treatment effect in relation to the group, the time period and/or the duration of treatment exposure (Callaway, 2022).

To calculate the unconditional group-time average treatment effect for not-yet treated ($ATT_{ny}(g, t; \delta)$), I followed the equation outlined by Callaway and Sant'Anna (2021):

$$ATT_{ny}(g, t; \delta) = E[Y_t - Y_{g-\delta-1} | G_g = 1] - E[Y_t - Y_{g-\delta-1} | D_{t+\delta} = 0] \quad (19)$$

Consistent with Assumption St.2, the analysis of $ATT_{ny}(g, t; \delta)$ assumes no treatment anticipation leading to $\beta = 0$.

6.3.2 Event Study in Staggered Treatment Design

To account for heterogeneity of individual treatment effects, an event-study model was applied within the framework of a staggered treatment design to analyze the dynamic effects of the introduction of women's suffrage on infant mortality. The event time model solves the problem of heterogeneous bias as a cause of staggered treatment adoption, by stacking all cantons relative to their own adoption (Callaway & Sant'Anna, 2021). The years of introduction of women's suffrage vary across cantons from 1959 to 1990. All cantons eventually adopted women's suffrage leading to a framework without a never-treated comparison group. The identification of the treatment effect relies on not-yet-treated cantons as a comparison group. The underlying intuition is that later adopting cantons serve as a valid counterfactual for earlier adopters, under Assumption 1 (Parallel Trends). Later adopting cantons can serve as a valid counterfactual under the assumption of parallel trends in the absence of the treatment.

$$\theta_{es}(k) = \sum_{g \in G} 1 \{g + kT\} P(G = g | G + kT) ATT(g, g + k) \quad (20)$$

$\theta_{es}(k)$ estimates the average effect of participating in the treatment k time periods after women's suffrage was introduced, across all cantons that ever introduced the right to vote for women (Callaway & Sant'Anna, 2021). Event time k is defined as $k = t - g_c$, where g_c denotes the year canton c adopted women's suffrage. In the adoption year k takes on the value 0 and

in pre-time periods negative values. Therefore, $ATT(g, g + k)$ identifies the average treatment effect of group g at event time k .

7 Results

7.1 Descriptive Analysis

To examine pre-existing differences prior to the introduction of women's suffrage, characteristics including official language, area, financial balance, health index and female secondary share were compared across cantons. [Table 5](#) provides the summary statistic of the 25 Swiss cantons for 1955. There are three official national languages in Switzerland, French, German, Italian and Romansh (Eidgenossenschaft, [2024](#)). Article 70 of the Swiss Federal Constitution states that each canton is free to determine its own official language (der Schweizerischen Eidgenossenschaft, [2024](#)). Overall, six cantons have French as an official language, 21 have German and two Italian. Valais, Bern, and Fribourg actually have two official languages, German and French. The same applies to Graubünden that has German and Italian as its official languages.

A comparison of the cantons' geographical areas (in km^2) shows a significant difference in the sizes. Graubünden is the largest canton with $7,105 \text{ km}^2$, followed by Bern with $5,959 \text{ km}^2$ and Valais with $5,224 \text{ km}^2$. Compared, the smallest canton is Basel-Stadt with only 37 km^2 . The second and third smallest cantons are Appenzell Innerrhoden with 173 km^2 and Zug with 239 km^2 .

Another comparison reveals substantial differences in wealth across cantons, when comparing their financial balances. Basel-Stadt, Neuchâtel, and Graubünden exhibit the largest financial surpluses, suggesting that these cantons are among the wealthiest in the sample. While Valais,

Appenzell Innerrhoden and Ticino have the largest financial deficit, indicating that they are among the poorest cantons. Furthermore, the two cantons Appenzell Ausserrhoden and Innerrhoden, who are the last to adopt women's suffrage, both exhibit a financial deficit.

There are also clear differences between cantons in the quality of health care provision. Whilst Graubünden, Zug and Appenzell Ausserrhoden have a high health index, suggesting relatively well-developed health care provision, the cantons Basel-Landschaft, Luzern and Solothurn prove to have a low-developed health care system with index values around 20.

Smaller variations between cantons were observed in the share of female students in secondary schools. The cantons of Basel-Stadt, Schaffhausen and Basel-Landschaft, which have the highest proportion of female students, account for a percentage of between 15 and 9 %. The cantons Valais, Solothurn and Genève with the lowest proportions of female students exhibit shares close to zero, indicating comparatively low levels of female educational attainment. When comparing the cantons smaller variations were observed in the share of female students in secondary schools. As the female educational attainment was relatively low in 1955, indicating female education to not be a major confound driving differential infant mortality trends.

The IMR range is striking, Fribourg with the highest infant mortality rate of 45 % is nearly five times that of Glarus which has 9.8 %. In 1955, birth rates across Swiss cantons ranged from 11.1 per 1,000 in Genève to 23.2 per 1,000 in Uri, reflecting substantial variation between French-speaking and German- and Italian-speaking cantons.

Table 5: Cantonal Characteristics in 1955 (Pre-Treatment
Baseline)

Canton	Adoption Group	Language	Area	Financial Balance	Health Index	Female Secondary	IMR	Birth Rate	
			(km ²)	(CHF per capita)		Share (%)	(per 1,000)	(per 1,000)	
Early (pre-1971)									
40	Neuchâtel	Early	French	803	48.60	31.25	7.27	31.3	13.8
	Vaud	Early	French	3,212	1.30	45.69	4.67	33.3	12.6
	Genève	Early	French	282	26.41	26.51	1.88	28.4	11.1
	Basel-Stadt	Early	German	37	54.07	31.62	15.13	25.0	12.6
	Ticino	Early	Italian	2,812	−7.99	45.93	3.60	35.6	12.7
	Basel-Landschaft	Early	German	518	13.15	16.92	9.81	23.7	18.1
	Zürich	Early	German	1,729	25.19	25.45	8.72	18.7	15.3
	Luzern	Early	German	1,493	−0.30	19.35	7.68	30.5	20.3
	Valais	Early	French, German	5,224	−23.53	41.61	0.37	41.3	21.2

(continued)

Canton	Adoption Group	Language	Area (km ²)	Financial Balance (CHF per capita)	Health Index	Female Secondary Share (%)	IMR (per 1,000)	Birth Rate (per 1,000)
<i>Middle (1971)</i>								
Aargau	Middle	German	1,404	4.40	32.49	5.99	26.6	19.6
Bern	Middle	French, German	5,959	2.05	31.84	9.79	25.4	17.0
Glarus	Middle	German	685	−5.35	20.33	6.82	9.8	15.8
Zug	Middle	German	239	4.09	63.11	6.62	32.0	19.8
Fribourg	Middle	French, German	1,672	−6.42	35.06	2.68	45.0	17.4
Solothurn	Middle	German	791	12.38	20.03	0.86	35.9	19.3
Schaffhausen	Middle	German	298	16.07	29.54	11.40	13.6	15.9
Thurgau	Middle	German	994	−3.92	35.85	7.24	18.9	18.0
<i>Late (post-1971)</i>								
St. Gallen	Late	German	2,026	−5.29	30.38	6.51	24.9	18.2

(continued)

Canton	Adoption Group	Language	Area (km ²)	Financial Balance (CHF per capita)	Health Index	Female Secondary Share (%)	IMR (per 1,000)	Birth Rate (per 1,000)
Graubünden	Late	German, Italian	7,105	42.50	76.01	8.49	31.5	17.5
Uri	Late	German	1,077	21.79	22.03	5.66	37.8	23.2
Schwyz	Late	German	908	6.02	24.30	5.03	34.0	20.5
Obwalden	Late	German	491	7.56	23.54	5.09	18.5	21.6
Nidwalden	Late	German	276	25.72	21.26	4.12	23.8	23.0
Appenzell A.Rh.	Late	German	243	−4.58	47.80	6.86	34.6	14.9
Appenzell I.Rh.	Late	German	173	−12.69	36.21	6.69	28.5	18.4

Notes: All variables are measured in 1955, prior to the introduction of women's suffrage in any Swiss canton. Cantons are grouped by their eventual year of suffrage adoption. Financial balance figures are in CHF per capita. IMR = infant mortality rate per 1,000 live births.

The geographical visualization (Figure 2) of the timing of the introduction of the women's suffrage indicates an east–west pattern. The cantons located in western Switzerland near the French border (Neuchâtel, Vaud and Genève) were the first cantons to introduce the right to vote for women. This is followed by the northern cantons of Basel-Stadt and Basel-Landschaft, located along the German border, and by the southern canton of Ticino, bordering Italy. These cantons either shared a border with Germany, Italy or other cantons. These early-adopting cantons either shared a border with France, Germany or Italy, being potentially exposed to spillover effects from those neighboring countries. All three countries introduced women's suffrage prior to any Swiss canton and Switzerland.⁷ The next cantons to introduce women's suffrage were the once located in the middle of Switzerland, followed by the cantons in the eastern Switzerland. The last two cantons that introduce the right to vote to their female population are Appenzell Ausserrhoden and Appenzell Innerrhoden, which lie in the north-east of Switzerland.

⁷France introduced women's suffrage in 1944, Germany in 1918 and Italy in 1946 ("Political Participation of Women", n.d.).

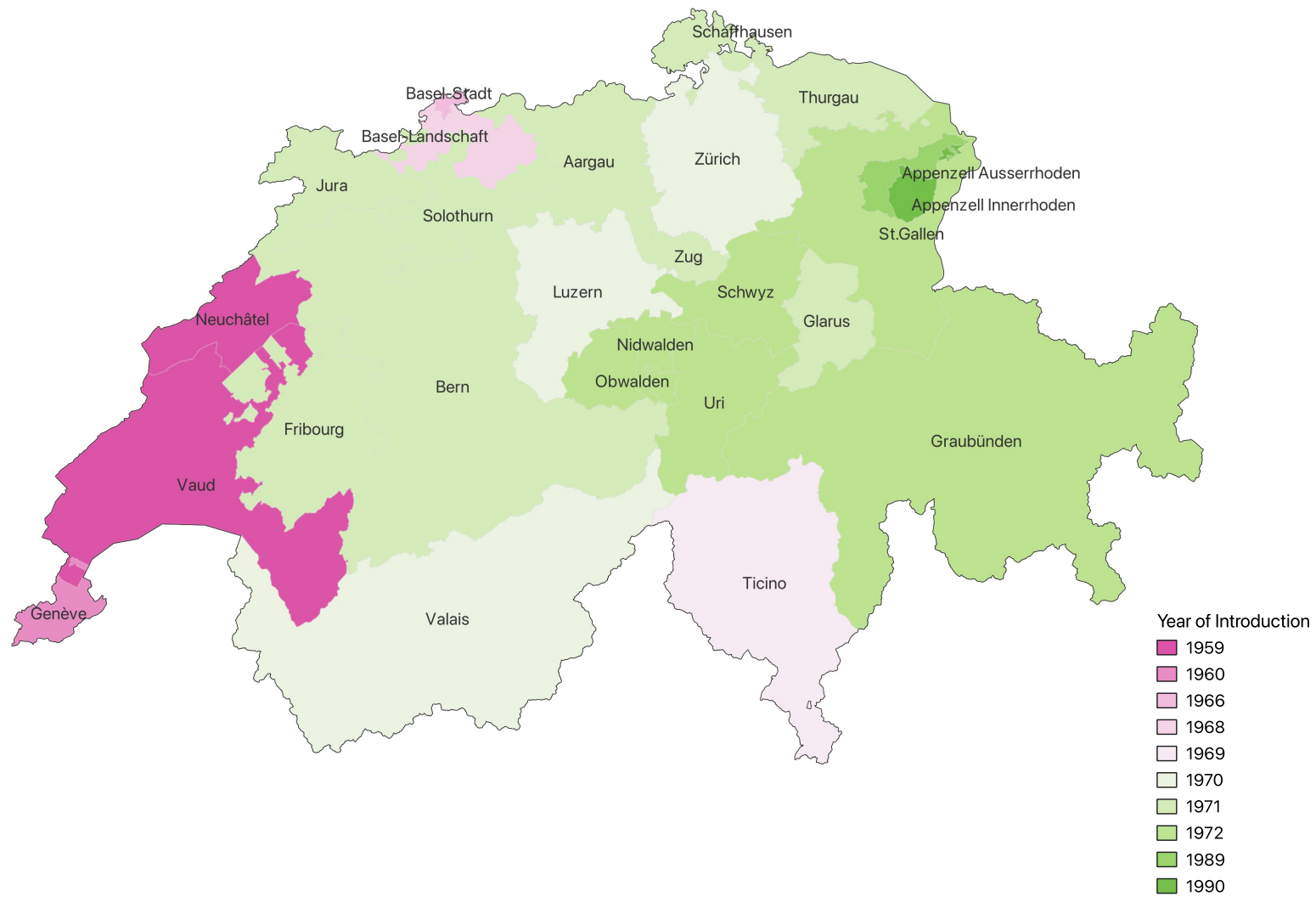


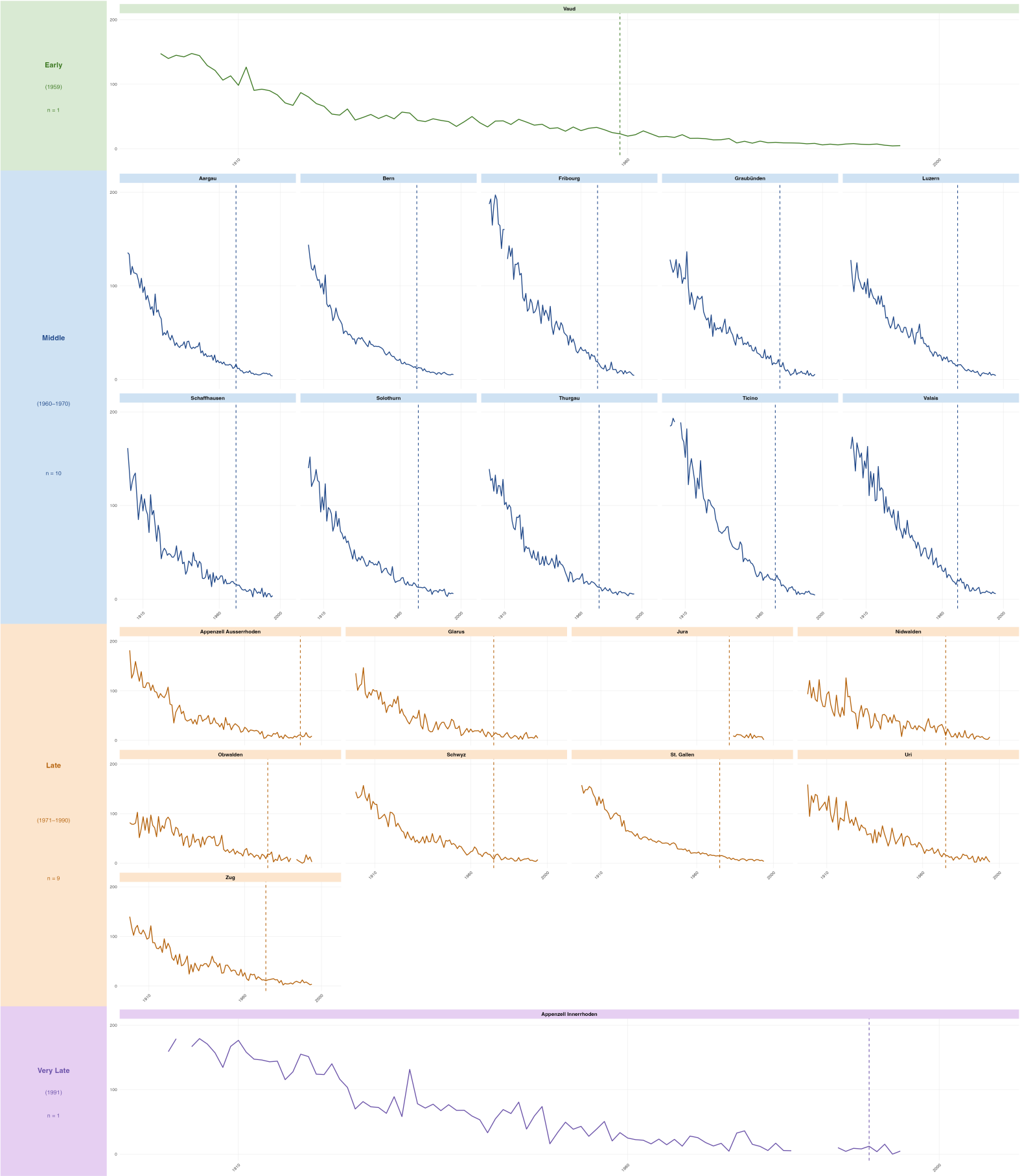
Figure 2: Geographical representation of the years of women's suffrage adoption

In order to determine the trend in infant mortality rate for the observed time period, I conducted an Im-Pesaran-Shin test (Im et al., 2003). Because the panel dataset is unbalanced and the observed time period of infant mortality rate for each canton varies, I ensured in a first step that each canton had at least 20 continuous observations for infant mortality rate. The Im-Pesaran-Shin unit root test yields a test statistic of $W = -6.337$ ($p < 0.001$), leading to a rejection of the null hypothesis of a unit root at all conventional significance levels. This supports the characteristics of the declining trend in infant mortality as deterministic rather than stochastic, where shocks have permanent effects. The series for all 25 cantons⁸ reverts to a predictable downward trend path consistent with the view that the long-run decline is driven by persistent structural factors improvements in sanitation, medical care, and living standards rather than the accumulation of random shocks.

⁸Excluding the canton Jura, because it was founded only 1979. Therefore, it is difficult to observe a clear trend over this short observation period of 21 years. Nevertheless, it can be observed that Jura had a relative low infant mortality rate, close to zero, since its foundation and it remained somewhat steady.

Infant Mortality Rate by Canton, Grouped by Timing of Women's Suffrage Introduction

Infant deaths per 1,000 live births



----- Year women's cantonal suffrage introduced

7.2 DiD Regression

For the baseline DiD regression estimator, the bootstrap p -value of 0.1325 exceeds all conventional significance levels, providing insufficient evidence to reject the null hypothesis ($\beta = 0$). As the null hypothesis cannot be rejected, no significant effect of women's suffrage introduction on infant mortality rate could be observed.

Table 6: Baseline DiD: Effect of Women's Suffrage on Infant Mortality Rate

	Infant Mortality Rate	
	(1)	(2)
	Without Controls	With Controls
Suffrage	6.1008 (4.0074)	6.8970* (3.6865)
Bootstrap p -value	[0.1325]	[0.0614]
Canton FE	Yes	Yes
Year FE	Yes	Yes
Controls	No	Yes
Cluster level	Canton	Canton
Imputations	$m = 10$, Rubin's Rules	
<i>Notes:</i> Dependent variable is the infant mortality rate (deaths per 1,000 live births). Pooled standard errors (Rubin's Rules) clustered at the canton level are reported in parentheses. p -values for suffrage are based on the wild cluster bootstrap. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.		

7.3 TWFE Event Study

To assess the validity of the parallel trends assumption, I conducted a joint chi-squared test on the pre-treatment event-time coefficients ($k = 5$ to $k = 2$). Averaging across the ten imputed datasets, a chi-squared statistic of 1.807 ($p = 0.771$) was obtained, providing no statistical evidence against the null hypothesis of parallel pre-treatment trends (Kahn-Lang & Lang, 2019). Therefore, the null hypothesis of parallel pre-treatment trends ($H_0 : \beta_{-5} = \beta_{-4} = \beta_{-3} = \beta_{-2} = 0$) could not be rejected. The pre-treatment coefficients are thus jointly indistinguishable from zero, lending for Assumption 3 to hold, which is essential for conducting meaningful results

for the TWFE estimator.

Table 8 presents the TWFE event study estimates, plotting the coefficients $\hat{\beta}_k$ for each event-time period k relative to the year of introduction of women’s suffrage ($k = 0$). At any event-time women’s suffrage demonstrates to have no statistically significant effect on infant mortality rate. Therefore, there is insufficient evidence to reject the null hypothesis of no effect of the introduction of women’s suffrage.

Table 7: Pooled TWFE Estimates: Women’s Suffrage and Infant Mortality Rate
Rubin’s rules, $m = 10$ imputations, 95% CIs clustered by canton

k	Baseline				With Controls			
	$\hat{\beta}_k$	SE	CI low	CI high	$\hat{\beta}_k$	SE	CI low	CI high
−5	−5.663	4.770	−15.012	3.685	−5.829*	1.667	−9.096	−2.562
−4	−1.016	2.130	−5.191	3.159	−1.109	1.580	−4.205	1.988
−3	−0.675	1.681	−3.969	2.620	−0.727	1.459	−3.586	2.132
−2	−0.109	1.377	−2.807	2.589	−0.128	1.256	−2.590	2.334
−1	0.000	—	—	—	0.000	—	—	—
0	0.012	1.108	−2.159	2.183	0.022	1.080	−2.094	2.139
1	1.802	1.529	−1.195	4.799	1.818	1.455	−1.034	4.671
2	1.636	1.541	−1.383	4.656	1.657	1.630	−1.538	4.852
3	−0.334	1.771	−3.804	3.137	−0.309	1.930	−4.092	3.474
4	−1.949	1.979	−5.827	1.930	−1.917	2.213	−6.255	2.420
5	−0.471	1.814	−4.026	3.085	−0.428	1.888	−4.129	3.272
6	0.696	2.061	−3.344	4.735	0.759	1.919	−3.003	4.521
7	0.516	2.271	−3.935	4.968	0.623	2.000	−3.296	4.543
8	1.241	2.768	−4.184	6.666	1.425	1.848	−2.197	5.048
9	3.120	3.680	−4.093	10.333	3.438	2.113	−0.704	7.580
10	5.165	6.934	−8.425	18.755	5.962*	2.603	0.861	11.063

Notes: $k = -1$ is the omitted reference period ($\hat{\beta}_{-1} = 0$ by construction). Controls include pre-treatment $\text{IMR} \times \text{year trend}$. * 95% confidence interval excludes zero. Requires `\usepackage{booktabs}` in preamble.

7.4 Robustness Check

The striking differences in IMR across cantons in the pre-treatment period 1955 (see Table 5), show that cantons start their trajectory from different starting points. This observation supports

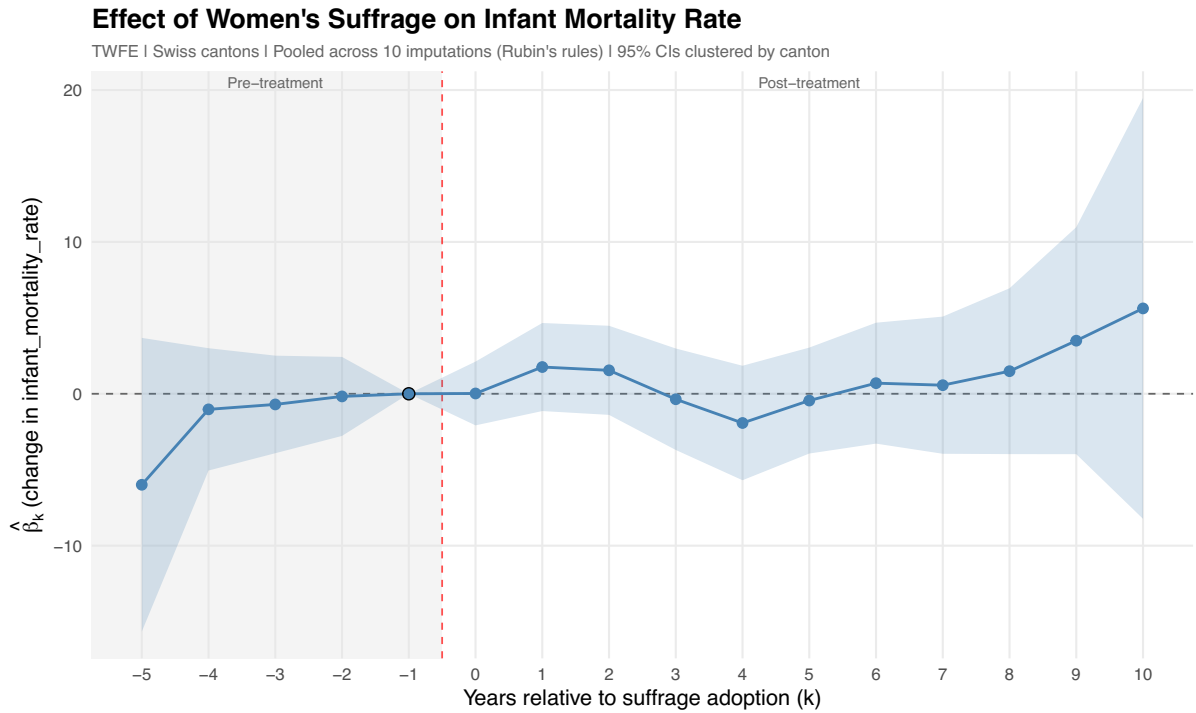


Figure 3: Event Study

adding the pre-treatment IMR interaction term.

7.4.1 DiD Regression

Despite including the interaction term, the estimate remains relatively stable when (6.1 vs 6.9), suggesting that the control variables are not dramatically changing the outcome (Table 6). However, for the extended specification the coefficient is marginally significant at 10 % level. Although the estimated effect is positive and statistically significant at the 10 % level, it should be interpreted cautiously, as the results are sensitive to whether controls are included.

7.4.2 TWFE Event Study

When including the interaction term, the estimated coefficient for the pre-treatment period 5 is statistically significant (Table 8). A significant pre-treatment coefficient indicates that the parallel trend assumption seems to be sensitive to inclusion of control variables. Therefore, the results of the TWFE event study model might be biased and should be interpreted with caution.

For the post treatment period $k = 2$ to $k = 9$ no significant effect of the introduction of women's suffrage on infant mortality rate could be observed. However, the estimated coefficient at event time $k = 10$ indicates a statistically significant positive effect. However, this estimate is the boundary of the event study analysis. Therefore, it is based on a small and unrepresentative subset of early-adopting cantons and should be interpreted with caution as a boundary effect rather than a substantive long-run effect.

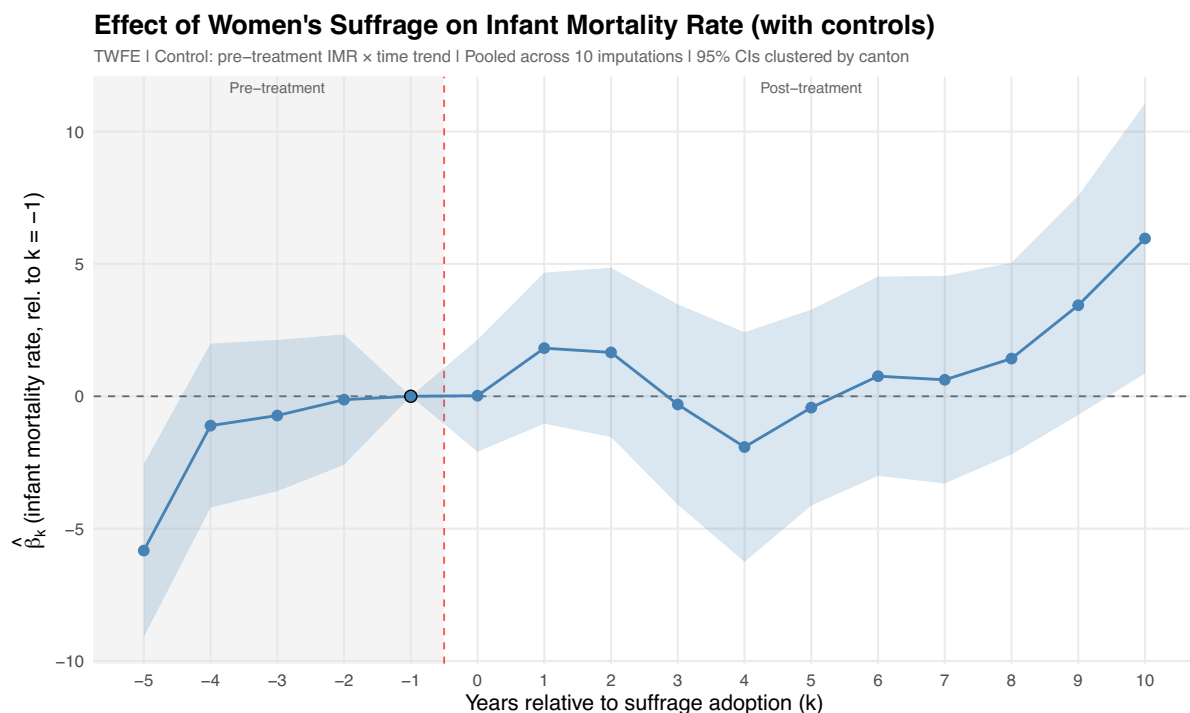


Figure 4: Event Study Covariate

When excluding Appenzell Innerrhoden, the last treated canton in 1990, it leads to a notable change in the TWFE estimates. In the pre-treatment periods, the estimator does not move closely to zero anymore, causing potential threat to the parallel trend assumption. This suggests that the aggregate coefficient is sensitive to the inclusion of late-adopting cantons. This instability indicated potential treatment effect heterogeneity across cohorts and motivates the use of the Callaway and Sant'Anna (2021) estimator.

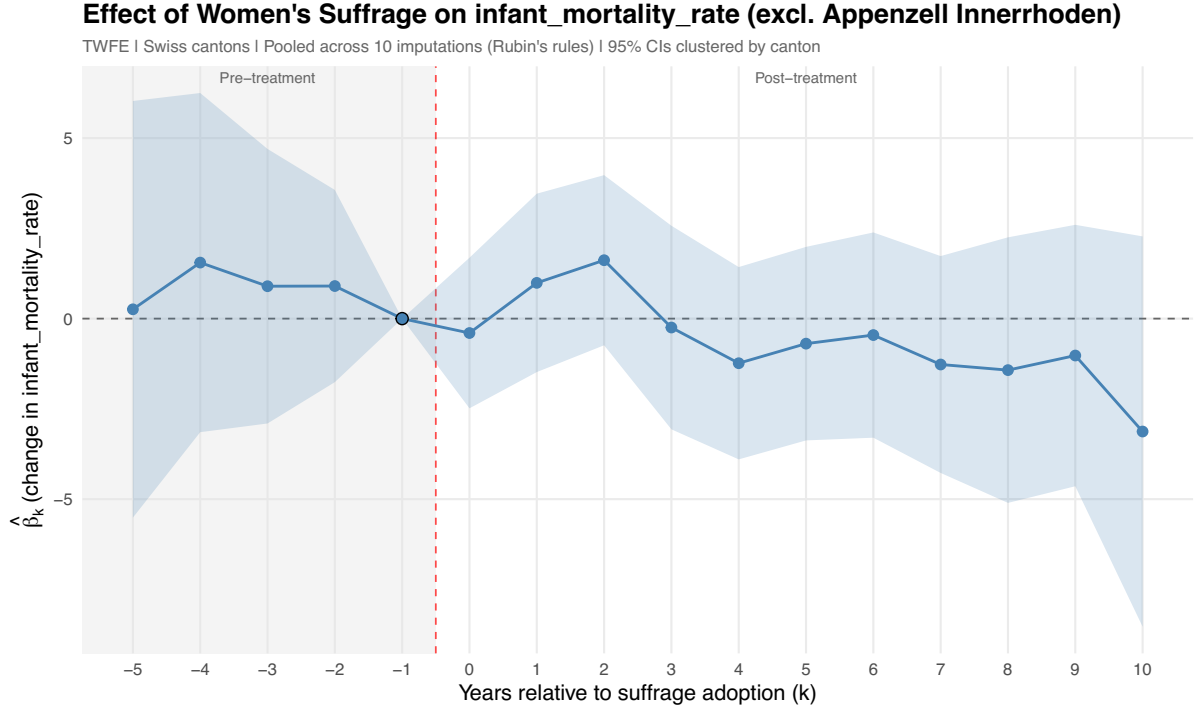


Figure 5: Event Study Robust

7.5 Heterogeneous Effect

As the conventional TWFE estimator is fragile to bias from heterogeneous treatment effect in staggered adoption design, I aggregate the $ATT(g, t)$ by adoption group to explore if there is heterogeneity present across treatment cohorts. [Figure 6](#) presents the group-specific aggregated treatment effects $\hat{\theta}^{sel}(\tilde{g})$ of women's suffrage on the infant mortality rate, estimated using the Callaway and Sant'Anna ([2021](#)) framework and pooled across 10 multiple imputations via Rubin's Rules [\[9\]](#). The cohort-specific summaries reveal significant treatment effect heterogeneity. For the groups that introduced suffrage in 1959, 1969, 1971, and 1972 a negative $ATT(g, t)$ estimate was observed, indicating a reduction in infant mortality rates following the introduction of women's suffrage. The groups that introduced women's suffrage in 1971 and 1972 show a greater decline in infant mortality than the groups that introduced it in 1959 and 1969. The 1970 cohort constitutes an exception, displaying a positive $ATT(g, t)$.

⁹Rubin's Rule is used to combined the results (estimates and standard errors) of the imputation to conduct further analysis (Schafer & Olsen, 1998).

These results show clear treatment heterogeneity. Since TWFE produces a weighted average of group-time treatment effects that can assign negative weights under staggered adoption with heterogeneous effects, the positive aggregate TWFE estimate should be interpreted with caution. However, the findings do not support Hypothesis 2 which assumes that the reduction in infant mortality rate is mainly driven by the early-adopting cantons. The results suggest that the observed reduction is primarily driven by the later-adopting cantons.

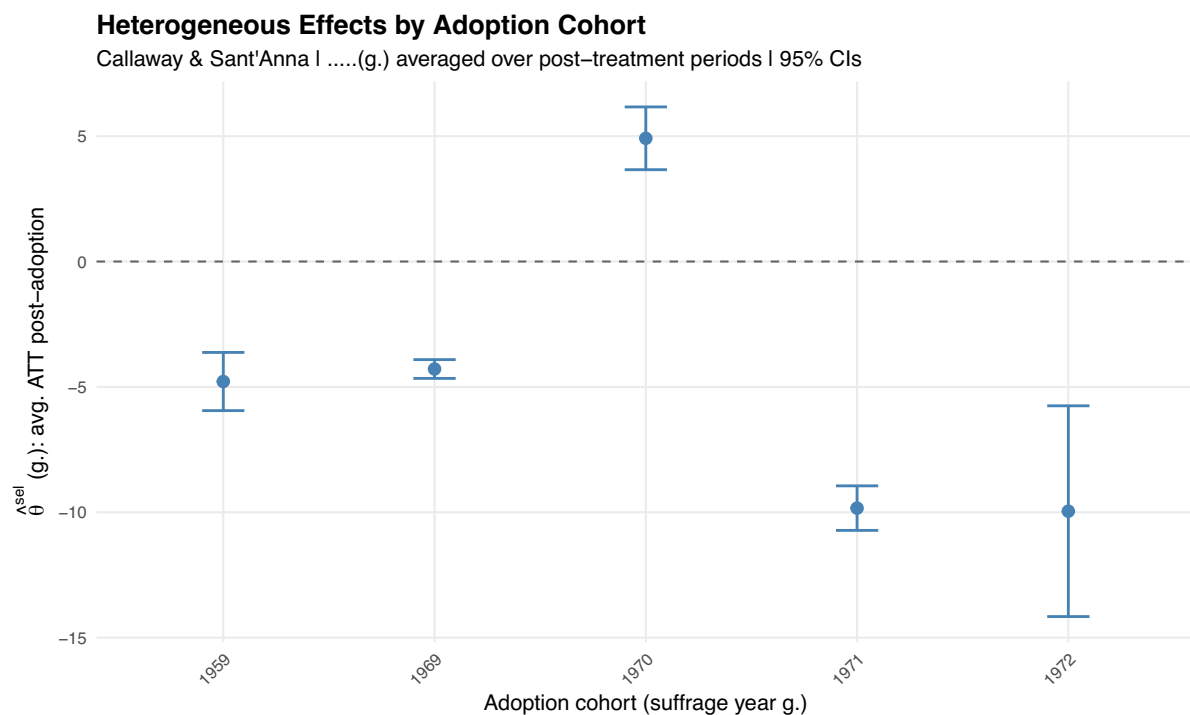


Figure 6: Heterogeneous treatment effect of group

This treatment effect heterogeneity is consistent with the instability of the TWFE event study estimator and further motivates the use of the Callaway and Sant'Anna (2021) estimator (CS estimator), which explicitly allows for cohort-specific treatment effects rather than imposing a single aggregate parameter.

7.6 Event Study in Staggered Treatment Design

For the CS estimator, the assumption of parallel trends was assessed using a joint chi-squared test on all dynamic ATT estimates prior to treatment, pooled across ten multiply-imputed datasets (Kahn-Lang & Lang, 2019). The average chi-squared statistic of 60.144 (df = 70, $p = 0.793$) provides no evidence against the null hypothesis of parallel pre-treatment trends:

$$H_0 : \text{ATT}(k) = 0 \quad \forall k < -1$$

As the previous analysis reveals heterogeneous treatment effects, these must be taken into account to avoid any potential bias. Therefore, the Callaway and Sant'Anna (2021) estimator is used to allow for the staggered nature of the adoption of women's suffrage across cantons. Considering the robustness check that was conducted for the TWFE estimation, the canton Appenzell Innerrhoden was already excluded from the sample, when constructing the Callaway and Sant'Anna (2021) specification.

The Callaway and Sant'Anna (2021) estimator shows a statistically significant negative effect on a 5% significance level for all post-treatment periods, except for the periods $k = 0$ and $k = 2$ (see Table 8). This indicates that there was no immediate effect of adopting women's suffrage on infant mortality rate. These findings suggest that the effect of the introduction of women's suffrage was primarily a long-run effect rather than an immediate response to the policy change.

Compared, to the standard TWFE event study, which showed insignificant treatment effects, the heterogeneity-robust Callaway and Sant'Anna (2021) estimator is statistically significant, indicating that standard regressions were somewhat distorted by forbidden comparisons (see

Figure 7).

Table 8: TWFE and Callaway & Sant'Anna Event Study Estimates

	Two-Way Fixed Effects		Callaway and Sant'Anna (2021)		
k	$\hat{\beta}_k$	SE	$\widehat{ATT}(\ell)$	SE	
-5	-5.991	4.935	-0.724	1.918	
-4	-1.026	2.055	2.631	1.703	
-3	-0.701	1.639	-1.427	1.491	
-2	-0.174	1.327	1.070	1.107	
-1	<i>0.000</i>	<i>0.000</i>	<i>0.000</i>	<i>0.000</i>	
0	0.020	1.071	-2.407	1.778	
1	1.762	1.479	-4.308	1.571	*
2	1.545	1.496	-2.568	1.797	
3	-0.359	1.704	-6.867	2.118	*
4	-1.923	1.923	-10.481	2.358	*
5	-0.446	1.779	-11.434	1.554	*
6	0.700	2.030	-9.947	2.139	*
7	0.566	2.304	-7.587	1.204	*
8	1.488	2.785	-6.365	1.725	*
9	3.496	3.813	-6.175	1.634	*
10	5.620	7.067	-8.058	2.121	*

Notes: * $|z| > 1.96$ (95% level). The reference period $k = -1$ is shown in italics; $\hat{\beta}_{-1} = 0$ by construction.

Standard errors are clustered by canton. Estimates are pooled across 10 imputations via Rubin's rules.

The C&S estimator uses not-yet-treated cantons as the comparison group with no anticipation ($\delta = 0$).

Endpoint bins: $k \leq -5$ and $k \geq 10$.

The visualization of the TWFE vs. the CS estimator (Figure 7) showed a clear difference in their results.

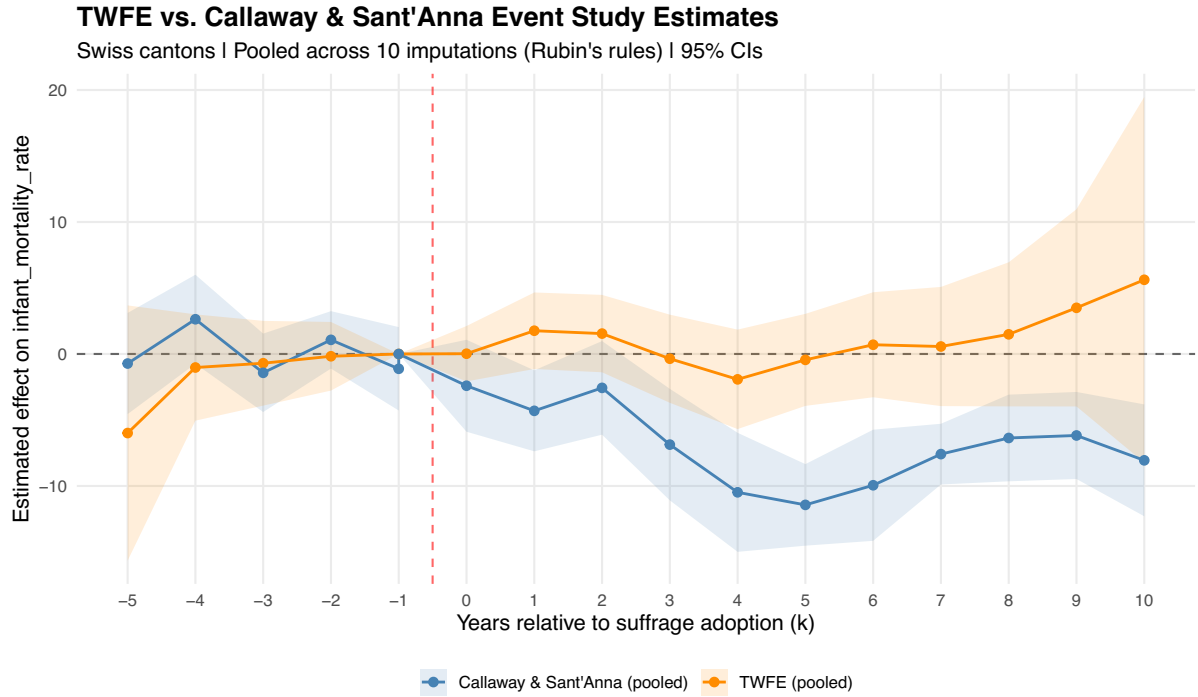


Figure 7: Event Study Comparison

7.7 Mechanism Check

As a mechanism check, the infant mortality rate is replaced by the stillbirth ratio as the dependent variable. Since stillbirths occur prior to birth, this specification helps disentangle whether any reduction in infant mortality is driven by improvements during pregnancy, such as better prenatal care or by changes in conditions during the first year of life. The stillbirth rate exhibits a negative trend beginning three periods after treatment adoption.

Having established a significant relationship between women's suffrage and the infant mor-

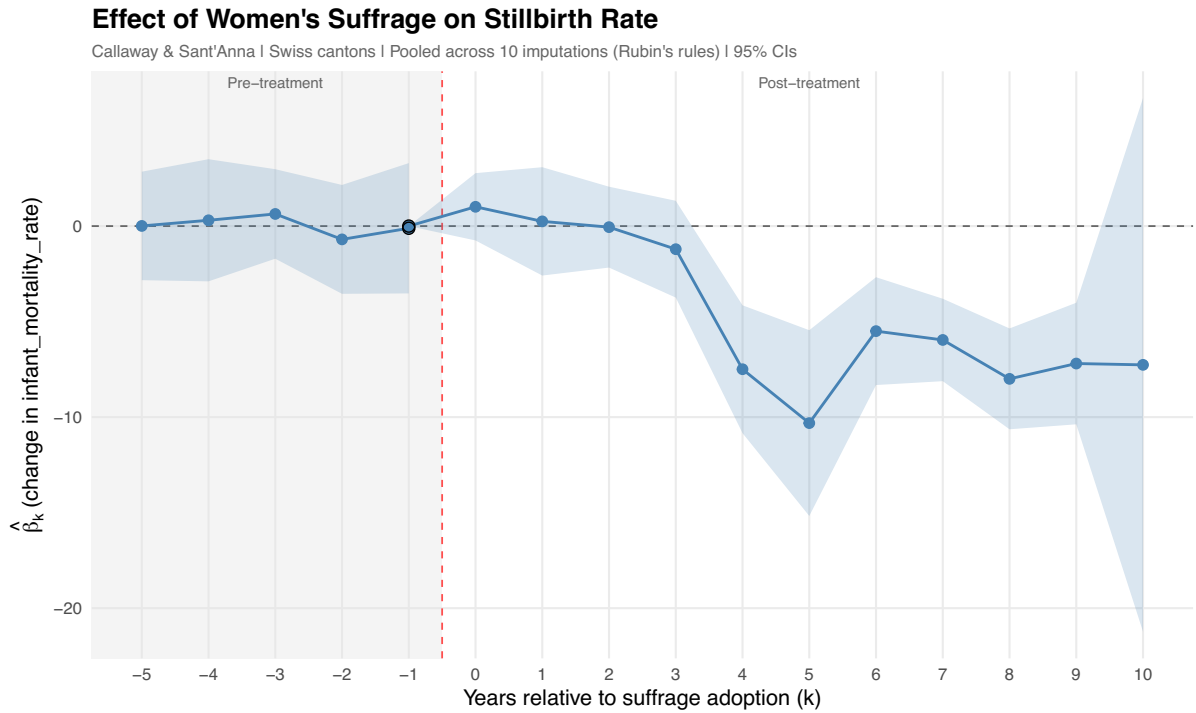


Figure 8: Event Study Stillbirth

tality rate, this section investigates the underlying mechanisms driving this effect, see [Table 9](#) and [Figure 9](#). Theoretically, the political empowerment of women can translate into improved infant health outcomes through several distinct channels such as changes in the financial balance, health index and female secondary share, which works as an indicator of female educational attainment.

The analysis of the treatment effect of women's suffrage on financial balance reveals an initial short-run ($k = 1 - 3$) deterioration in cantonal financial balances. This could be driven by increased social spending, as newly enfranchised women pushed cantons to increase their expenditure on areas such as health care provision and infrastructure. The effect becomes insignificant through the event times $k = 4 - 10$. From $k = 11 - 15$ the estimated effect is again positive and statistically significant, indicating a long-run financial surplus. This financial surplus suggests that the fiscal payoff due to an increase in expenditures, has a positive effect in the long run.

The most direct mechanistic link between infant mortality rate and women's suffrage, is the provision of health care. In order to determine the level of health care provision within a canton, a health index was constructed. When analyzing the effect of women's suffrage on health index, the effect proves to be statistically significant for the observed 15 post-treatment periods. The significant effect is positive for all 15 periods, suggesting improved health care provision following the introduction of women's suffrage. This observation can be linked to the short-run effects observed for the financial balance, as women's votes redirect cantonal expenditures towards issues related to maternal and child health, access to health care, social welfare, and family support programs than men.

The estimates for female secondary school enrollment reveal a long-run, positive effect on the number of female students in secondary schools. The effect is statistically significant at the 5% level in all post-treatment periods, except for $k = 6$, for which the effect is statistically significant at the 10 % level. It is important to note, that the pre-treatment period $k = -1$ is statistically significant, which could reflect some sort of anticipation effect. Cantons that are prior to the treatment debating whether or not to introduce women's suffrage might be more progressive and signaled a policy shift toward gender equality through education.

Table 9: Mechanism Analysis: Callaway & Sant'Anna Dynamic ATT Estimates

k	Financial Balance		Health Index		Female Sec. Share	
	ATT	SE	ATT	SE	ATT	SE
-5	12.38	(16.59)	-0.319*	(0.163)	0.0008	(0.0009)
-4	11.76	(17.64)	-0.437**	(0.173)	0.0008	(0.0010)
-3	-7.18	(13.94)	0.093	(0.209)	0.0010	(0.0010)

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k	Financial Balance		Health Index		Female Sec. Share	
	ATT	SE	ATT	SE	ATT	SE
-2	-12.30	(36.98)	-0.449*	(0.266)	0.0007	(0.0014)
-1	-40.45	(32.84)	-0.034	(0.297)	0.0039**	(0.0016)
0	0.72	(36.54)	0.650**	(0.270)	0.0040**	(0.0018)
1	-64.73*	(37.34)	0.904**	(0.461)	0.0122***	(0.0042)
2	-72.44***	(23.94)	1.270**	(0.549)	0.0205***	(0.0068)
3	-74.07**	(34.32)	1.556**	(0.689)	0.0285***	(0.0094)
4	-61.19	(38.69)	1.539**	(0.700)	0.0365***	(0.0125)
5	-67.82	(48.93)	1.874***	(0.725)	0.0436***	(0.0160)
6	-45.88	(47.79)	2.288***	(0.844)	0.0325*	(0.0170)
7	61.39	(49.04)	3.022***	(1.023)	0.0382**	(0.0173)
8	30.99	(31.30)	4.690***	(1.168)	0.0443**	(0.0186)
9	11.66	(28.55)	5.234***	(1.437)	0.0506***	(0.0193)
10	119.09	(86.46)	4.595***	(1.406)	0.0525**	(0.0209)
11	164.33*	(90.15)	2.944***	(1.086)	0.0596***	(0.0229)
12	188.47**	(81.85)	2.516*	(1.428)	0.0672***	(0.0256)
13	333.84***	(91.04)	3.821**	(1.722)	0.0807***	(0.0312)
14	132.58**	(59.32)	5.534***	(2.055)	0.0904***	(0.0350)
15	337.57***	(109.93)	6.031***	(1.888)	0.0999**	(0.0414)

Notes: Dynamic ATT estimates from Callaway and Sant’Anna (2021) with not-yet-treated control group. Results are pooled across 10 multiply-imputed datasets using Rubin’s rules. $k = 0$ denotes the year of suffrage adoption; $k = -1$ is the normalised reference period (ATT fixed at zero by construction). Standard errors in parentheses are clustered at the canton level.

8 Discussion

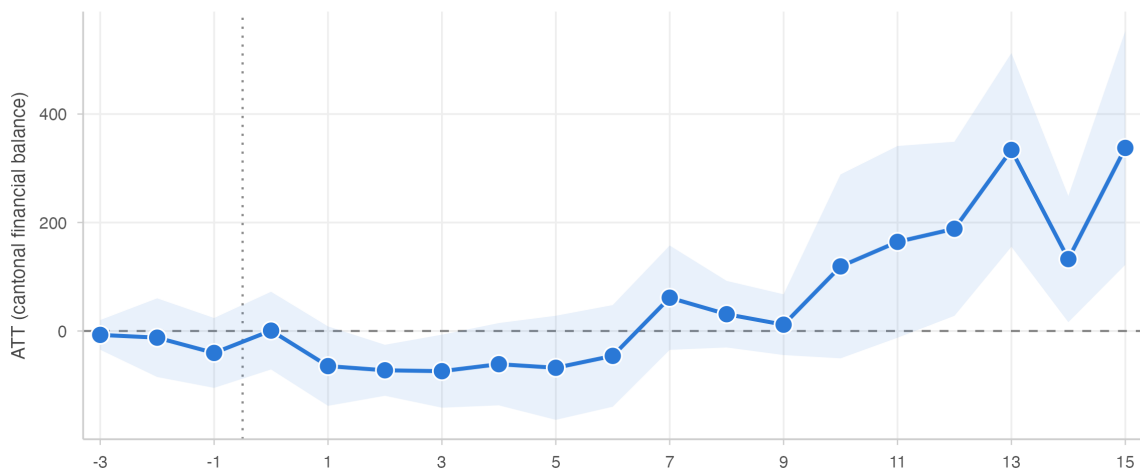
In the case of the simple DID approach a positive insignificant treatment effect was observed. Using the cohort-specific $ATT(g, t)$, a clear difference between the treatment effect was observed. A negative $ATT(g, t)$ was observed for four of the five cohorts, although the magnitude of the estimated effects varies across cohorts. This indicates the presence of treatment heterogeneity supporting Hypothesis 2, which causes the results of the simple DID approach to be distorted. However, contrary to the assumption underlying Hypothesis 2, the treatment effect is larger for later adopting cantons.

Given the evidence of heterogeneous treatment effects across cohorts, the CS estimator is preferred over TWFE, as it explicitly accounts for treatment effect heterogeneity under staggered adoption and avoids the negative weighting bias inherent in the TWFE framework. In comparison, the Callaway and Sant’Anna (2021) approach identifies a negative treatment effect that is statistically significant at the 5 % level. However, the results indicate that the reduction in infant mortality rate caused by the introduction of women’s suffrage is more of a long-term effect than a short-term change. This finding is plausible, as the implementation of new legislation and policies typically requires time before their effects become observable. Furthermore, the results are contingent on the assumption that men’s preferences differ from women’s, thereby resulting in differences in legislative priorities. Observing a negative effect support the assumption stated in Hypothesis 1, that the introduction of women’s suffrage leads to a reduction in infant mortality rate. The results are consistent with the broader literature on female political

Mechanism Analysis — Dynamic ATT (C&S Estimator)

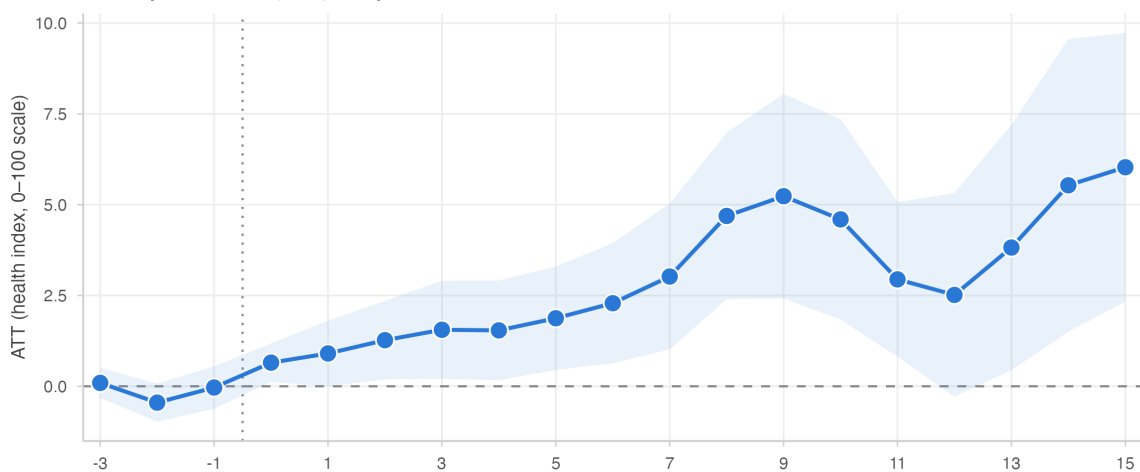
A Financial Balance

Callaway & Sant'Anna (2021) · not-yet-treated control · 95% CI



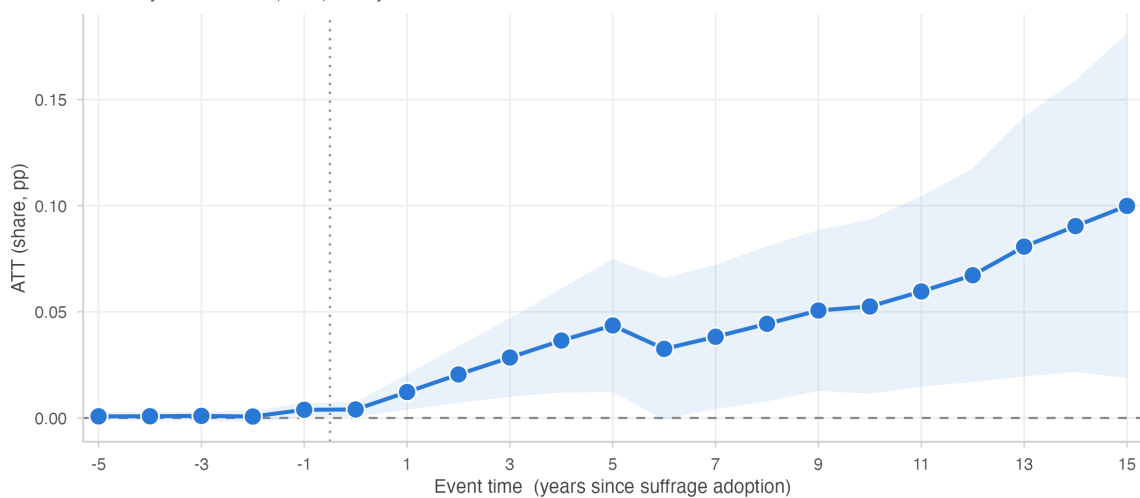
B Health Index

Callaway & Sant'Anna (2021) · not-yet-treated control · 95% CI



C Female Secondary Education Share

Callaway & Sant'Anna (2021) · not-yet-treated control · 95% CI



Pooled across 10 imputed datasets via Rubin's rules.

Figure 9: The effect of women's suffrage introduction on confounding factors

participation and public health outcomes (Bhalotra & Clots-Figueras, 2014; Miller, 2008).

The mechanism analysis was used to explain the channels through which political empowerment translated into a reduction in infant mortality rate. The sustained and significant increase in the health index across all post-treatment periods represents the most direct mechanistic link. Women's suffrage appears to have increased cantonal investment in health care infrastructure, precisely the factors number of physicians per capita, hospitals and available hospital beds. The stillbirth analysis partially provides support for this interpretation. The decline in stillbirths observed several periods after treatment adoption suggests that improvements in prenatal care may have contributed to the subsequent reduction in infant mortality. However, the delayed onset indicates that this mechanism may have required some time for new healthcare infrastructure and policies to become effective.

The positive and persistent effect on female secondary school enrollment points to an operating mechanism through female educational attainment. Notably, Caldwell and McDonald (1982) argued that the step from primary to secondary schooling is more consequential for child health outcomes than the transition from illiteracy to primary education. As secondary schooling equips women with the critical reasoning and health literacy needed to effectively utilize health services. Therefore, an increasing share of female students following enfranchisement suggests a positive effect on children's health.

The short-term deterioration in financial balance indicates an increase in cantonal government expenditures following the introduction of women's suffrage. Increased public expenditure is likely to be directed towards healthcare and family support, as previous research has shown that women tend to favor legislation and policies in these areas (Besnier, 2020).

While the identified mechanism channels are theoretically consistent and empirically supported, the analysis cannot rule out the presence of confounding factors that influenced both

suffrage adoption timing and infant health trajectories.

Furthermore, several limitations of the study should be considered when interpreting the results. In the research I did not account for technical or medical development, leading to a reduction in infant mortality rate. However, national-level confounders like the Great Depression, the postwar economic boom, and the diffusion of antibiotics affect all cantons simultaneously, which helps to isolate the suffrage effect from those aggregate trends, but it is not possible to completely separate those developments from the suffrage effect.

A further limitation concerns the measurement of health index. The variables used to conduct the index, were standardized using [Equation 5](#) to ensure comparability across cantons of varying population sizes. While this standardization is necessary for cross-cantonal comparison, it obscures variations in the spatial distribution of health care infrastructure within individual cantons. For instance, a canton might have a high density of hospital beds in urban areas but has shortages in rural areas where accessibility to emergency obstetric care is low. If this intra-cantonal geographic variation is omitted, the estimated effect of women's suffrage might not capture local differences in access to health care.

A further limitation of the study concerns the omission of factors such as religion, which seem to play a role in the pace of the reduction in infant mortality rate. Research has found a high correlation between infant mortality and confession. Cantons with the majority of the population being protestant show a faster reduction pace than cantons with a Catholic majority (Lorenzetti & Perrenoud, [1997](#)). Furthermore, it is assumed that the stillbirth rate in Catholic cantons was actually considerably higher because stillborn babies were baptized in an emergency and thus registered as live births (Ganguillet, [n.d.](#)).

A further constrain lies in missing information regarding the percentage change of electoral

participation after the introduction. An increase in participation would indicate that women also participated in the election, which is an essential assumption for the research.

Another factor left out in the research, but might be interesting to analyze further is the difference between rural and urban areas, as research found that infant mortality rate tend to be higher in rural areas compared to urban areas (Lorenzetti & Perrenoud, 1997).

Generalizing the findings of introduction of women's suffrage having a negative effect on infant mortality rate to other countries requires that they share key characteristics with Switzerland, such as a federalist structure, comparable levels of economic development, and similar pre-existing health infrastructure. In contexts that differ substantially along these dimensions, the estimated effects might not hold.

9 Conclusion

The standard DiD approach was found to be unsuitable for this analysis, as it does not account for treatment effect heterogeneity. In comparison, the estimator from Callaway and Sant'Anna (2021) provides more reliable results by accounting for treatment heterogeneity and the setting of the staggered treatment adoption. Overall, this study reveals that the introduction of women's suffrage leads to a significant reduction in infant mortality rate in the long-run. However, the effect of women's political enfranchisement is likely to be indirect and is brought about by a change in voting behavior and government spending.

The findings are consistent with previous literature (Besnier, 2020; Bhalotra & Clots-Figueras, 2014; Miller, 2008), which finds a negative effect of women's empowerment on children's health. However, this research extends the existing literature by applying the Callaway and Sant'Anna (2021) estimator. This is substantively important, because the TWFE results sign-

reversed relative to the Callaway and Sant'Anna (2021) estimates, demonstrating that the standard estimator produces misleading results in this setting due to heterogeneous treatment effects across cohorts. This staggered adoption is precisely the setting for which Callaway and Sant'Anna (2021) was designed, making this research one of the first applications of this modern estimator to the women's suffrage and health literature.

Future research could extend the analysis by incorporating factors such as geographical differences between cantons. In this research I briefly mentioned that there is a clear west to east pattern in the introduction timing, but did not go into further detail. This could be of particular interest, as (Miller, 2008) found similar results that western states introduced women's suffrage prior to eastern states in his research.

Appendix

Table A1: Panel Composition by Canton

Canton	Observations	First Year	Last Year
Appenzell Innerrhoden	93	1900	1995
Obwalden	94	1900	1995
Aargau	96	1900	1995
Appenzell Ausserrhoden	96	1900	1995
Basel-Landschaft	96	1900	1995
Basel-Stadt	96	1900	1995
Bern	96	1900	1995
Fribourg	96	1900	1995
Genève	96	1900	1995
Glarus	96	1900	1995
Graubünden	96	1900	1995
Jura	17	1979	1995
Luzern	96	1900	1995
Neuchâtel	96	1900	1995
Nidwalden	96	1900	1995
Schaffhausen	96	1900	1995
Schwyz	96	1900	1995
Solothurn	96	1900	1995
St. Gallen	96	1900	1995
Thurgau	96	1900	1995
Ticino	96	1900	1995
Uri	96	1900	1995
Valais	96	1900	1995
Vaud	96	1900	1995
Zug	96	1900	1995
Zürich	96	1900	1995

Note: Jura excluded (cantonal status only from 1979).

Total canton-year observations: 2,395.

ID	Canton
1	Zürich
2	Bern
3	Luzern
4	Uri
5	Schwyz
6	Obwalden
7	Nidwalden
8	Glarus
9	Zug
10	Fribourg
11	Solothurn
12	Basel-Stadt
13	Basel-Landschaft
14	Schaffhausen
15	Appenzell Ausserrhoden
16	Appenzell Innerrhoden
17	St. Gallen
18	Graubünden
19	Aargau
20	Thurgau
21	Ticino
22	Vaud
23	Valais
24	Neuchâtel
25	Genève
26	Jura
27	Switzerland (national aggregate)

Table A2: Canton identifiers used in the panel dataset

Group	Year of Introduction	Canton
1	1959	Vaud, Neuchtel
2	1960	Genve
3	1966	Basel-Stadt
4	1968	Basel-Landschaft
5	1969	Ticino
5	1970	Valais, Luzern, Zürich
6	1971	Aargau, Fribourg, Schaffhausen, Zug, Glarus, Solothurn, Bern, Thurgau
7	1972	St. Gallen, Uri, Schwyz, Graubünden, Nidwalden, Obwalden
8	1989	Appenzell Ausserrhoden
9	1990	Appenzell Innerrhoden

Table A3: Groups used to assess treatment effect heterogeneity

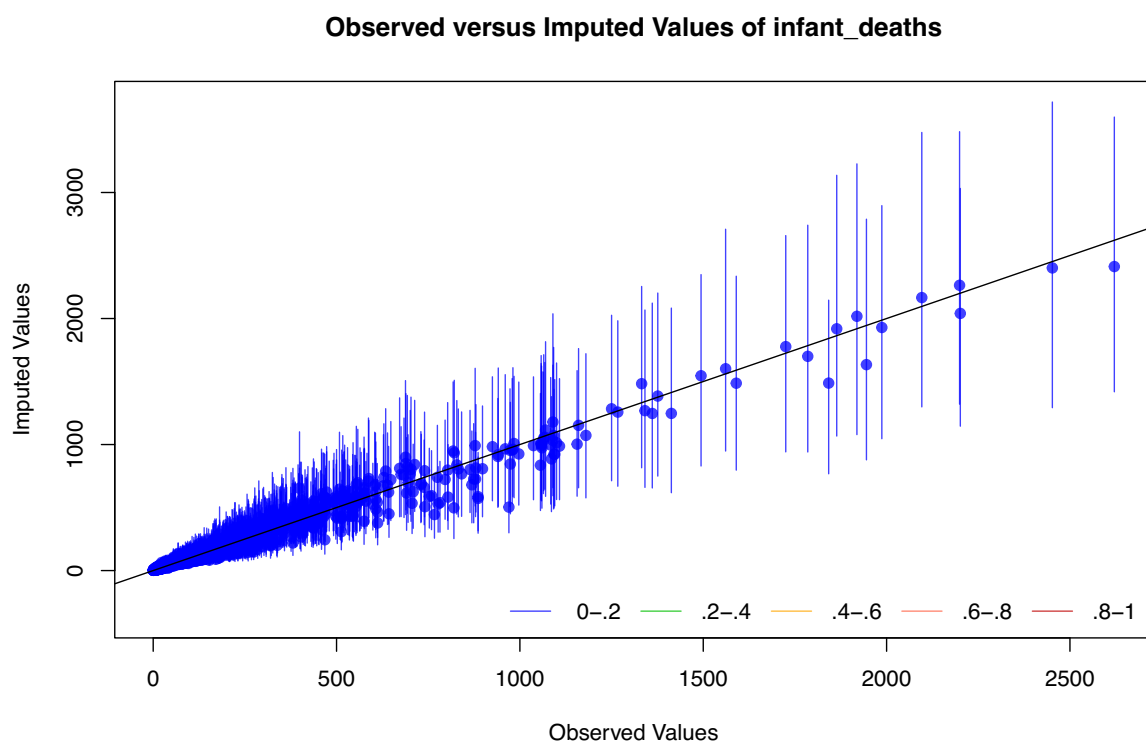


Figure A1: Imputation of missing values for infant deaths

Notes: The plot puts the observed values on the x-axis and the imputed value on the y-axis. If imputation were perfect, every point would satisfy: imputed = observed, which is exactly the equation $y = x$ — the 45° line. The color shows whether the information for the imputed data is mostly drawn on nearby observed data (red) or mostly on the model's structural assumptions like the time trend and cross-sectional patterns (blue).

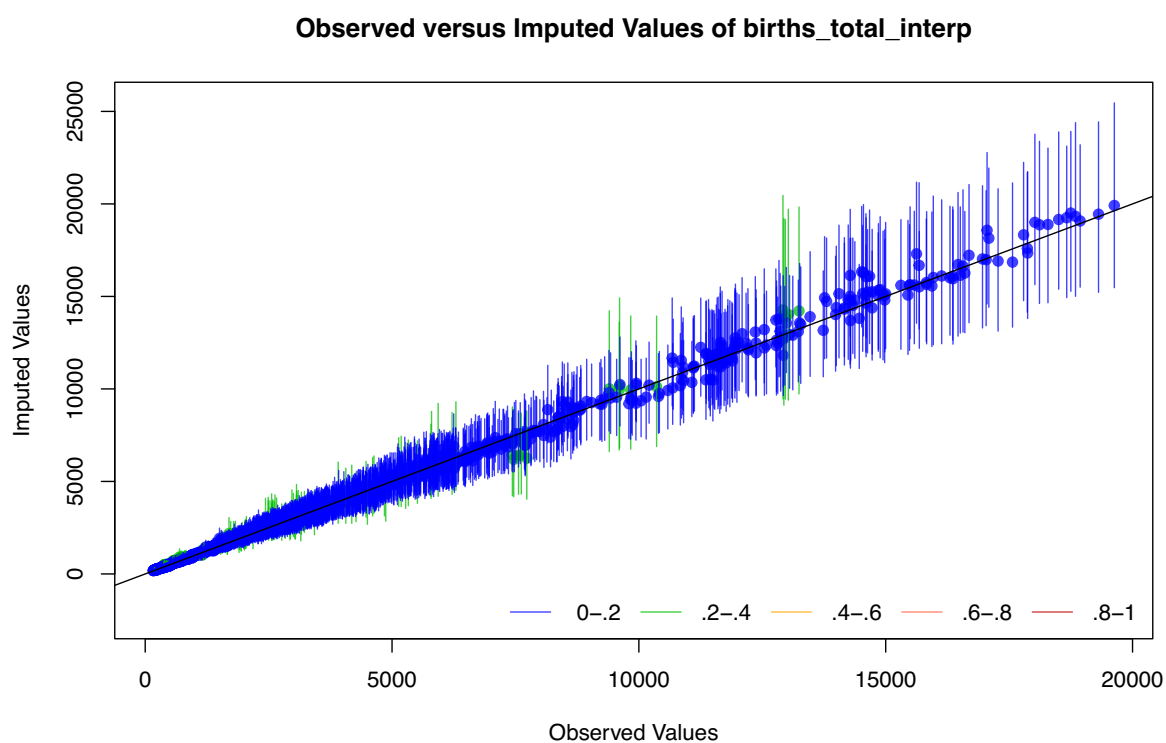


Figure A2: Imputation of missing values for births total

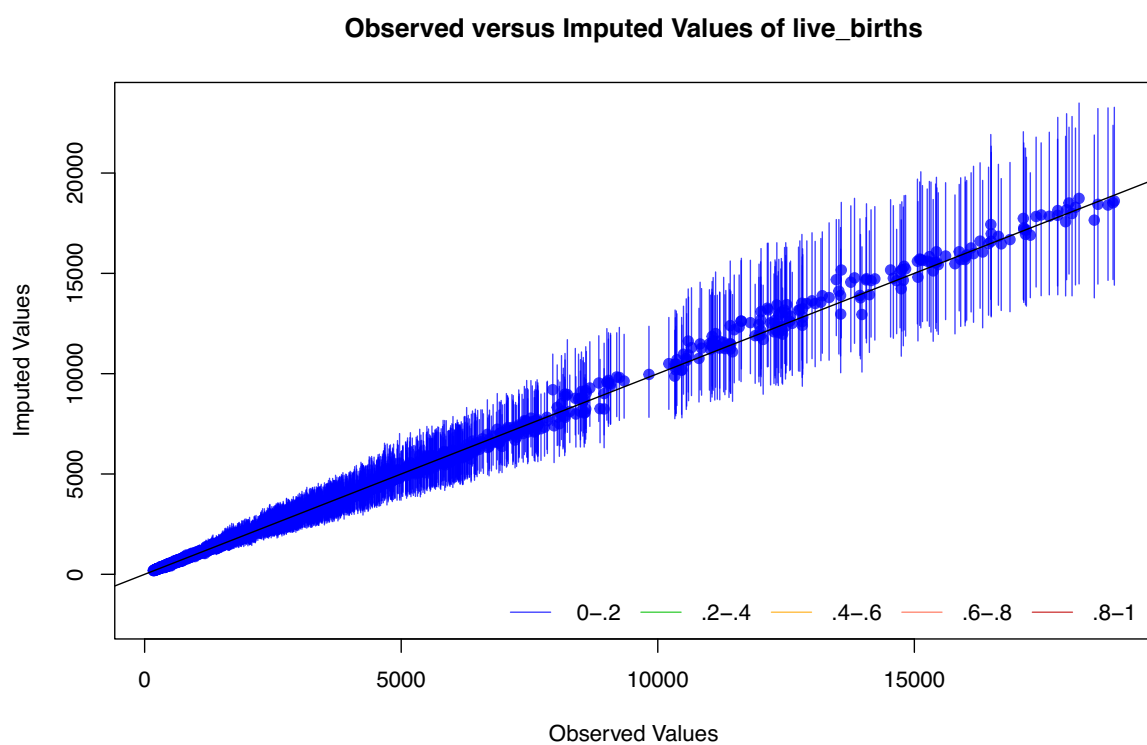


Figure A3: Imputation of missing values for live births

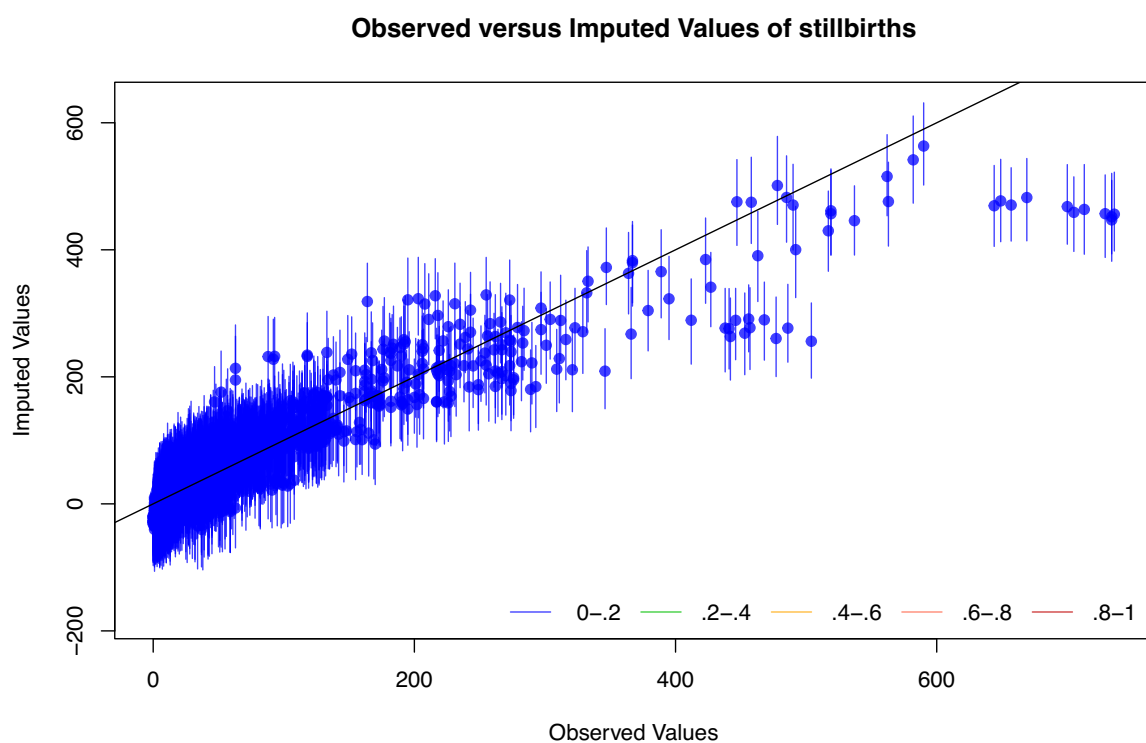


Figure A4: Imputation of missing values for stillbirths

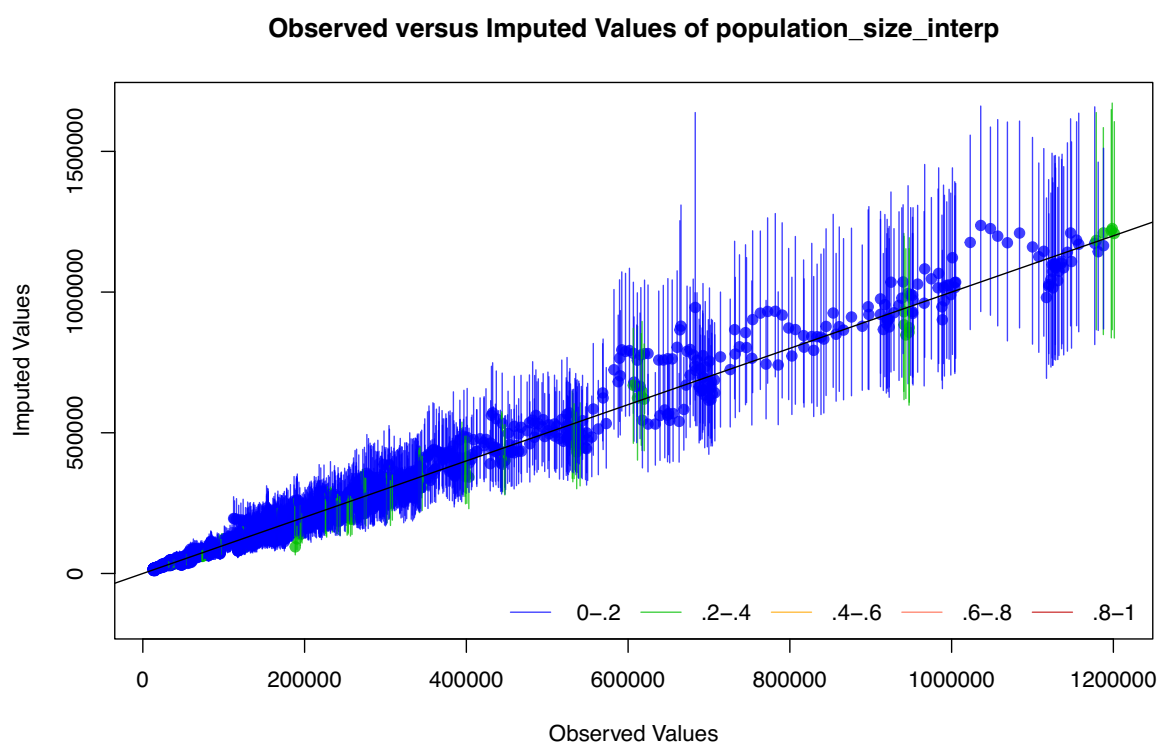


Figure A5: Imputation of missing values for Population

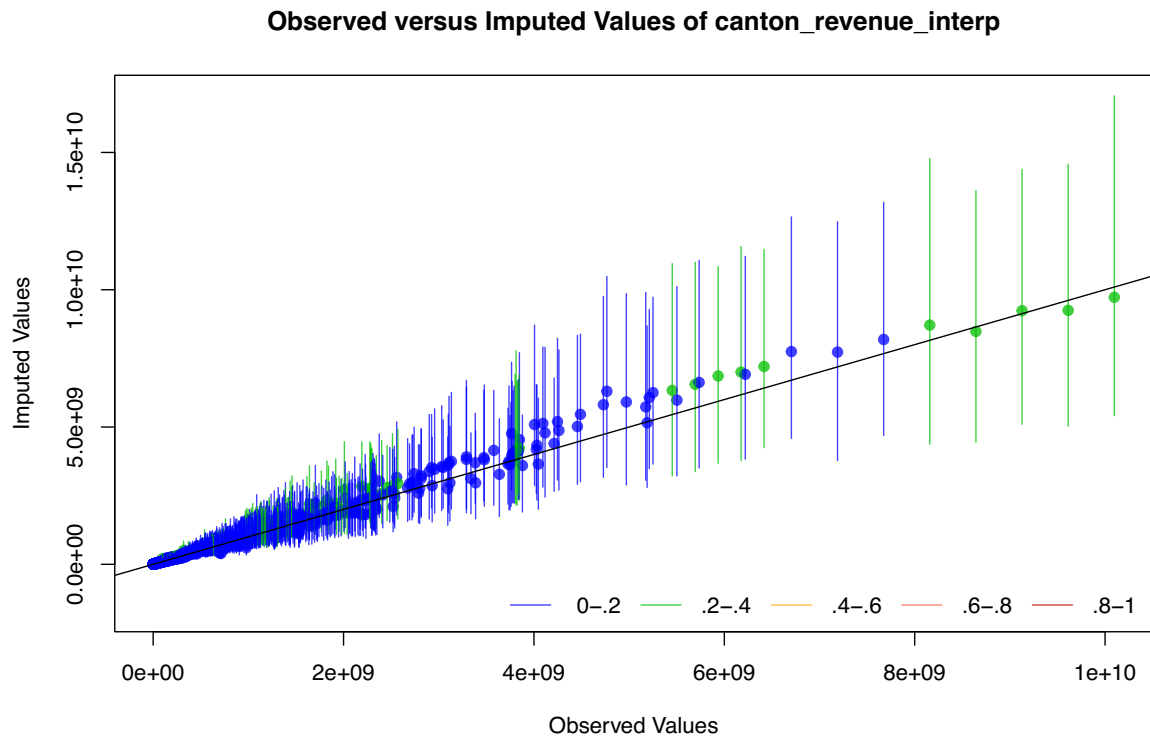


Figure A6: Imputation of missing values for Revenues

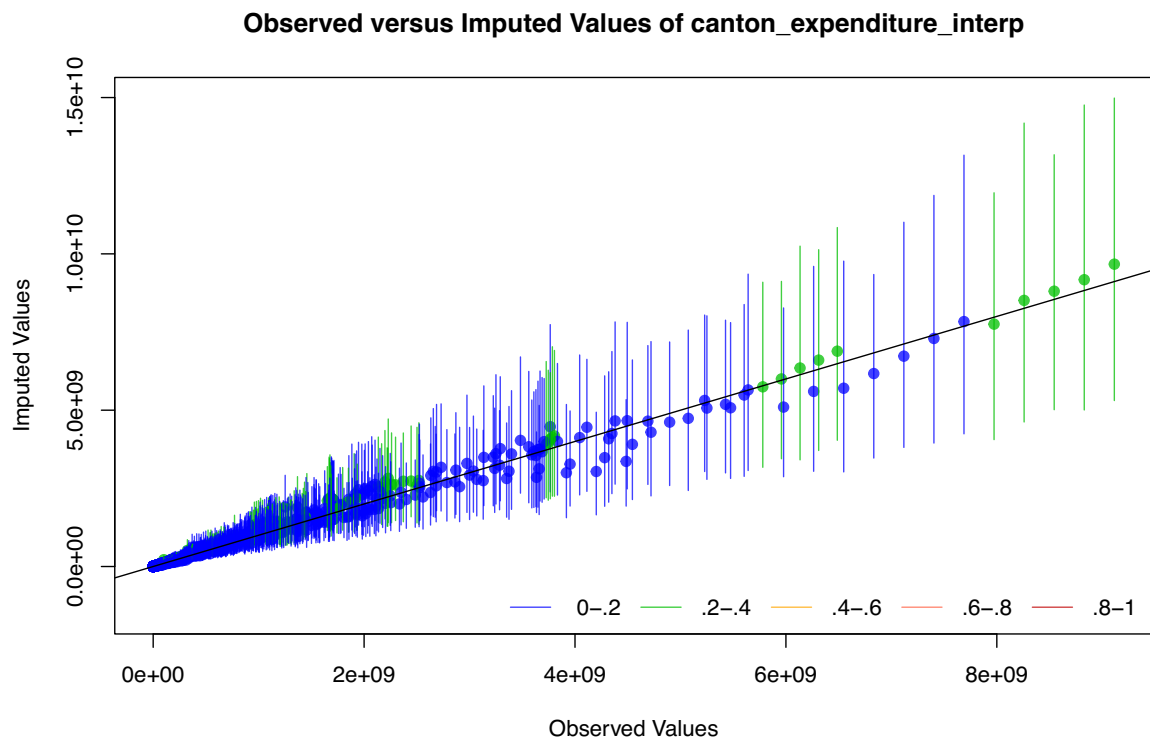


Figure A7: Imputation of missing values for Expenditure

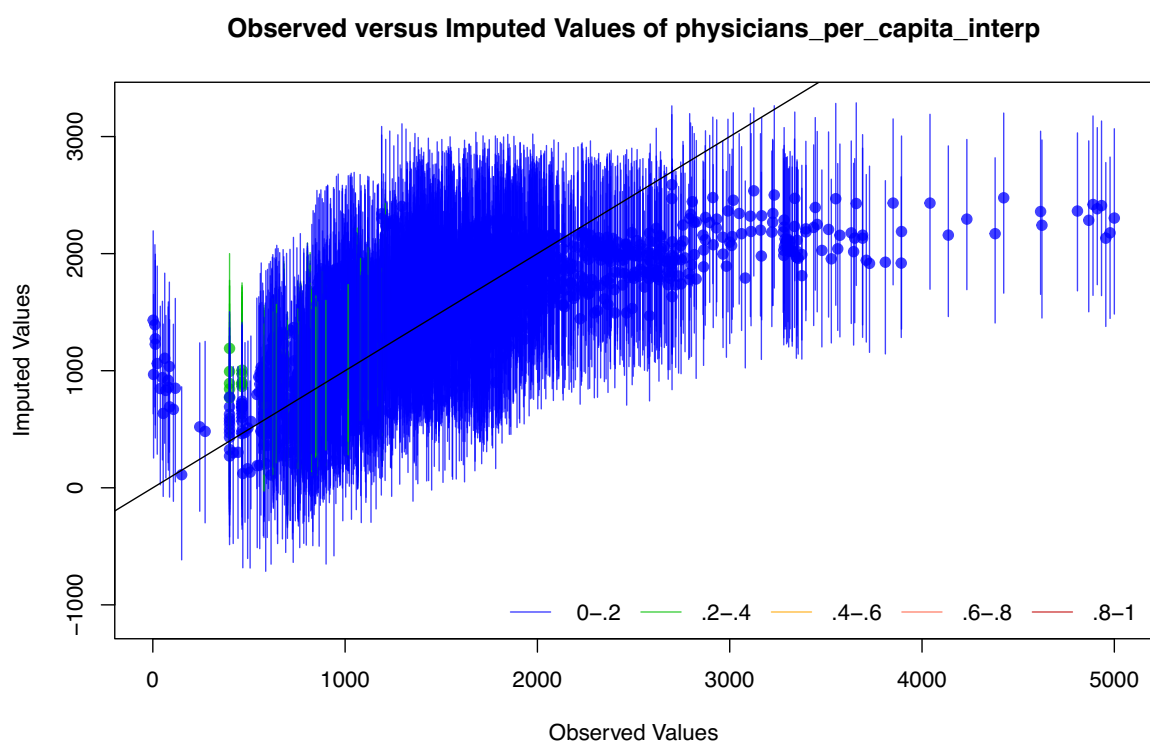


Figure A8: Imputation of missing values for Physicians per capita

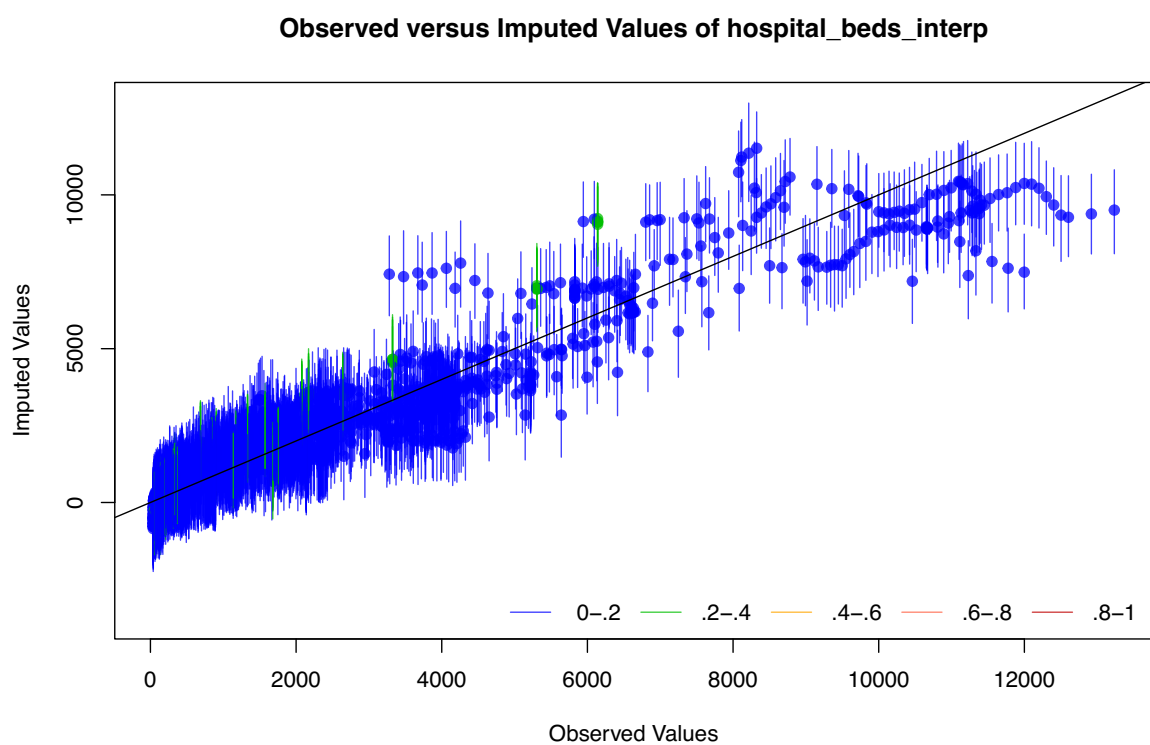


Figure A9: Imputation of missing values for Hospital Beds

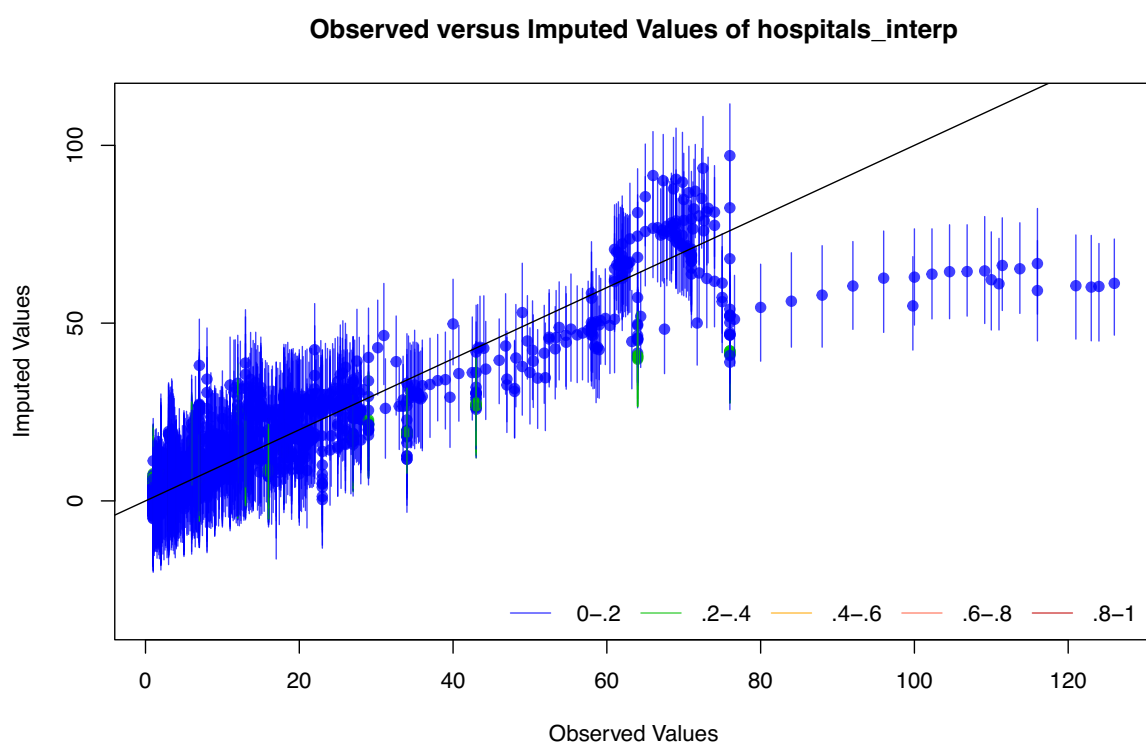


Figure A10: Imputation of missing values for Hospitals

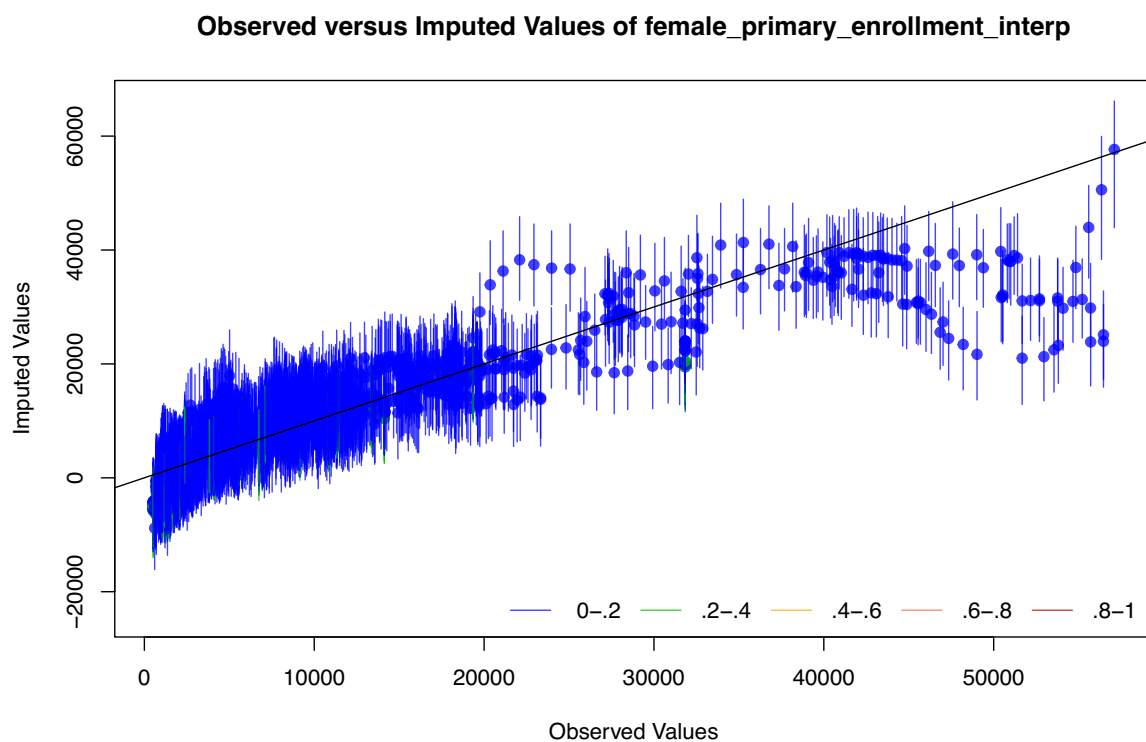


Figure A11: Imputation of missing values for the number female students enrolled in primary schools

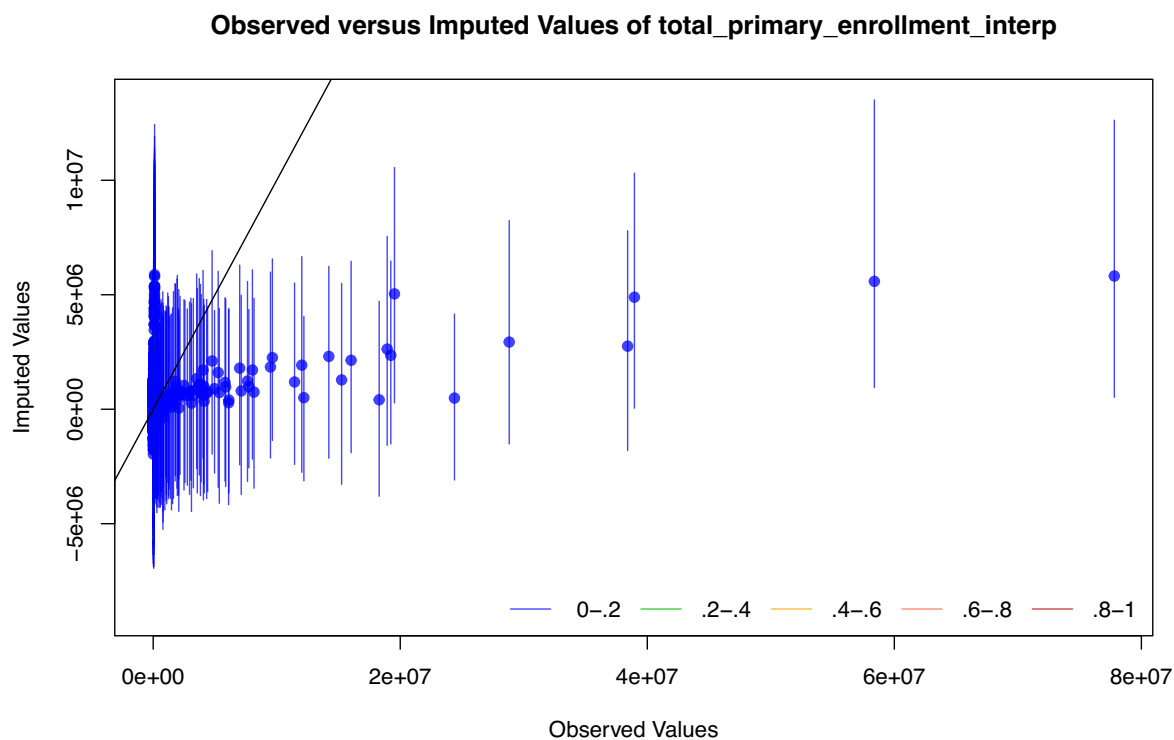


Figure A12: Imputation of missing values for the total number of students enrolled in primary schools

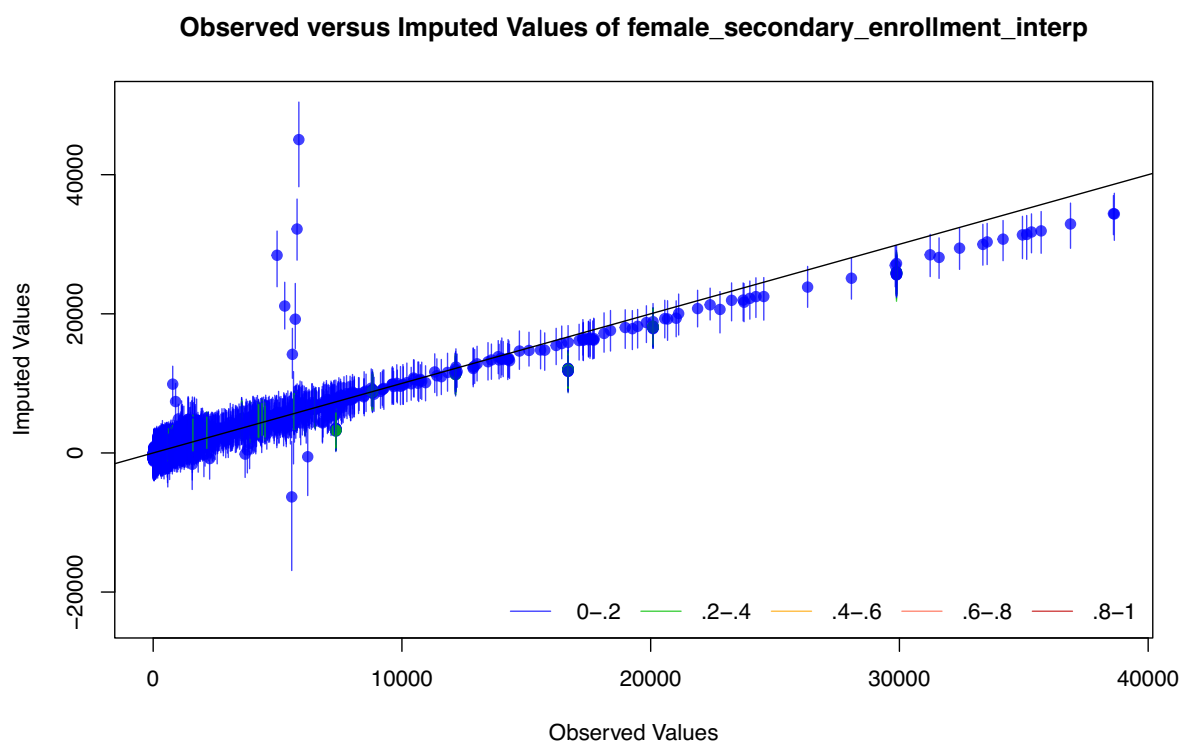


Figure A13: Imputation of missing values for the number female students enrolled in secondary schools

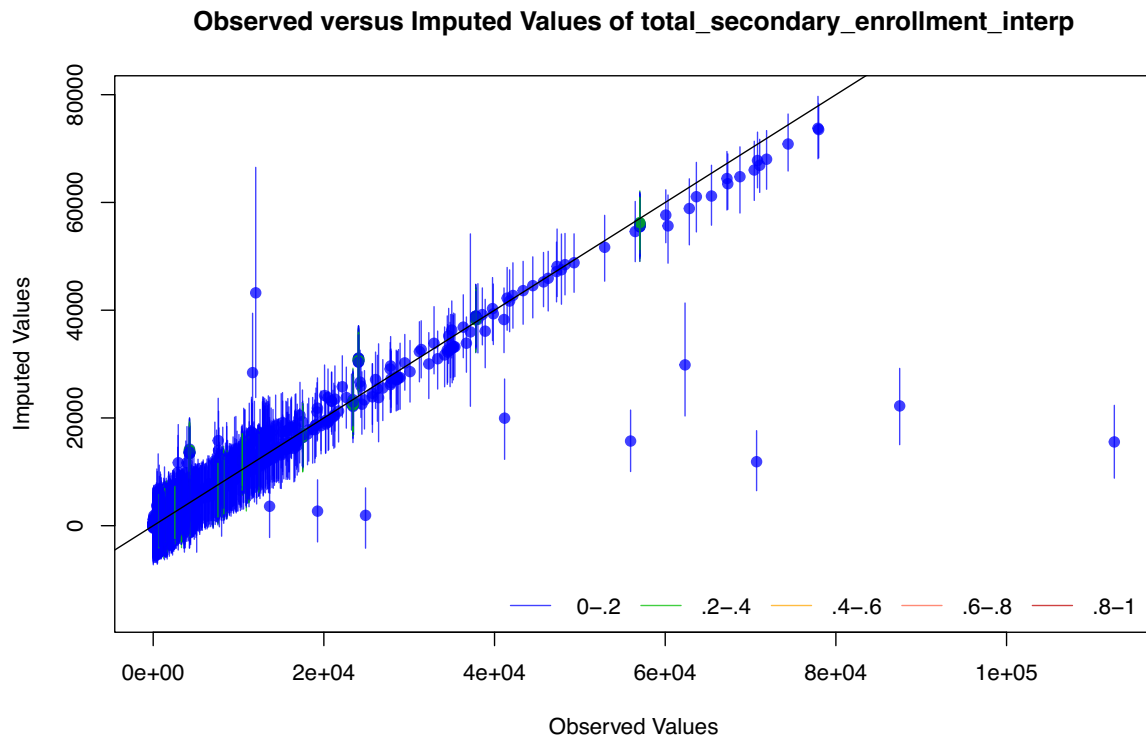


Figure A14: Imputation of missing values for the total number of students enrolled in secondary schools

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