Center for Economic Institutions

Working Paper Series

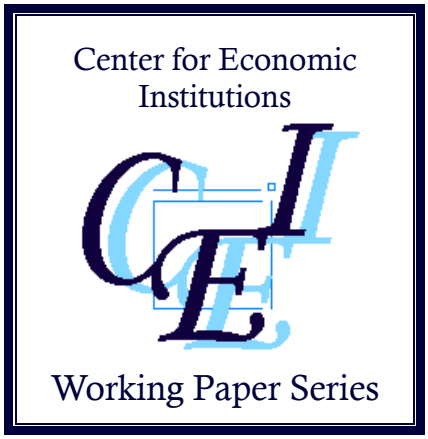
No. 2022-6

"Can Soft Law Improve the Welfare of Sexual Minorities?

The Case of Same-sex Partnership Policy in Japan"

Yuri Sugiyama

December, 2022



Institute of Economic Research
Hitotsubashi University
2-1 Naka, Kunitachi, Tokyo, 186-8603 JAPAN
https://cei.ier.hit-u.ac.jp/English/index.html
Tel:+81-42-580-8405/Fax:+81-42-580-8333

Can Soft Law Improve the Welfare of Sexual Minorities?

The Case of Same-sex Partnership Policy in Japan*

Yuri Sugiyama†

Abstract

Soft law, defined as a set of not legally binding rules, can play a potentially important role in protecting minorities, but it remains empirically unknown whether and how such law works. This study examines how the introduction of soft law affects the welfare of sexual minorities in the context of Japan, where an increasing number of municipalities have adopted a non-binding policy that officially recognizes same-sex relationships ("Same-sex partnership policy"). Using a difference-in-differences and an event study analysis that exploits the variations in the timing of adoption, I find that the same-sex partnership policy reduces the suicide rate of the general population by 5%. I then show that the partnership policy promotes a greater awareness of sexual minorities among residents. Google search data reveal that the number of searches for the word "LGBT" increases after the introduction of the partnership policy, while that for discriminatory words for sexual minorities decreases. Furthermore, original survey data shows the level of subjective happiness of sexual minorities became higher after their municipalities introduced the partnership policy. The survey analysis also suggests that cisgender heterosexuals from the municipalities with the partnership policy became more tolerant toward sexual minorities. Finally, all of these effects are more prominent in the more liberal municipalities. These results altogether imply that soft law can improve the welfare of sexual minorities by increasing the social awareness and acceptance of sexual minorities, especially where people are more likely to accept the new norms proposed by laws.

*This study is a part of the research project, Comprehensive Analysis of Disparity, at the Center for Economic Institutions of Hitotsubashi University. I am truly grateful to my advisors, Kensuke Teshima and Chiaki Moriguchi, for their continuous guidance, support and encouragement. I would also thank Daniel L. Chen, Hidehiko Ichimura, Hiroyuki Okamuro, Kazuya Masuda, Kisho Hoshi, Kohei Kawaguchi, Mari Tanaka, Masaki Iwasaki, Masayuki Sawada, Michihito Ando, Takashi Kurosaki, Yoko Okuyama, Yukichi Mano, Takashi Iida, for their helpful comments and Katsunori Yamada, Saori Kamano, and Yoshiki Ishibashi and the member of Takimoto Seminar at the University of Tokyo for their advice and support for internet surveys, as well as the seminar and conference participants at Hitotsubashi, the 15th Applied Econometrics Conference, Happy Hour Seminar!, Kansai Labor Workshop, the 34th Annual Conference of the European Society for Population, the 2021 Asian Meeting of the Econometric Society, 'Legalisation of Same-sex Marriage: A Global Perspective' Online Conference, the 2021 Delhi Winter School, Asian Law and Economics Association Annual Conference 2022, the Asian & Australasian Society of Labour Economics 2022 Conference. Financial support from the Center for Economic Institutions and the Institute of Economic Research at Hitotsubashi University and JSPS KAKENHI Grant Number JP22J10946 is gratefully acknowledged. The survey design received institutional review board approval from Hitotsubashi University. Any remaining errors are my own.

†Hitotsubashi University, Graduate School of Economics. E-mail: ed215002@g.hit-u.ac.jp

1 Introduction

Soft law, defined as a set of non-legally binding rules, has been employed to protect the rights of minority or socially disadvantaged groups. For example, European institutions have taken some actions that were non-legally binding to encourage each European country to protect the rights of sexual minorities as human rights (Kollman, 2009). Another example is the soft law implemented in 1985 by the Japanese government to pursue equal employment opportunities between men and women (Parkinson, 1989).[1]

Soft law has the advantage of gradually changing citizens' attitudes if the law focuses on controversial matters such as euthanasia or abortion, by bringing dialogue and communication into the community. Contrastingly, hard law may cause conflict or polarization by forcing either side of the issue to comply one-sidedly (Witteveen and Van Klink, 1999). Regarding same-sex marriage, Eskridge (2002) argues that recognizing same-sex couples and increasing awareness about them slowly can achieve legalized same-sex marriage without creating a rift in the community.[2]

Despite its use in the policy fields, it remains empirically unclear whether and how soft law can function as intended. Theoretically, soft law can be effective if it communicates normative beliefs of society to people, and they update their perceived norms (Gersen and Posner, 2008). The role of such law is termed "expressive function", where law affects people's behavior through revealing public attitudes, not through legal sanctions or legitimacy. Law can have stronger expressive effects if people know the law and assume it highly correlates with public attitude toward the relevant subject (McAdams, 2017).[3] Thus, the expressive effects can be heterogenous by conditions influencing people's perceptions, such as initial local norms or the implementation process. However, few empirical studies investigate whether and under what condition soft law is effective through expressive power. One possible reason

[1]This law was officially titled "Law to Promote the Welfare of Female Workers by Providing for Equality of Opportunity and Treatment in Employment."

[2]One example of such is the equal employment opportunities soft law between men and women in Japan. Lawmakers initially relied on soft law because they considered that the gradual approach by soft law could avoid conflicts or backlash effects otherwise possibly caused by hard law (Parkinson, 1989).

[3]Gersen and Posner (2008) elaborate on the mechanism that soft law affects behavior based on insights from the expressive function theory for hard law. Thus, this study invokes the expressive function theory for the incentive mechanism of soft law.

is that few cases allow empirical researchers to exploit variations within one country and identify the causal effects since such soft law is likely to be adopted at an international or national level.[4]

This study explores the effectiveness of soft law in the context of a same-sex partnership policy in Japanese municipalities, which is not legally binding and only recognizes same-sex couples' relationships. This same-sex partnership policy is particularly relevant for studying the effectiveness of soft law and the expressive function as the mechanism for the following three reasons. First, many but not all municipalities have adopted the policy at different times, allowing for exploiting cross-municipality and cross-time variations of introduction in Japan. Second, this policy is implemented in a context where social acceptance levels of sexual minorities are deficient (OECD, 2019), attributable to worse mental health conditions among sexual minorities in Japan (Hidaka, Operario, Takenaka, Omori, Ichikawa, and Shirasaka, 2008).[5] Therefore, there can be much room for updating the beliefs about sexual minorities and improving their welfare. Finally, Japan has not legalized same-sex marriage or anti-discrimination laws based on gender identity and sexual orientation (Amnesty International, 2021). This situation enables the expressive effects of the partnership policy to be identified because the partnership policy itself is not legally binding and can only have expressive power.

This paper then employs a difference-in-differences analysis and an event-study design that exploits the staggered introduction of the same-sex partnership policy to test the hypothesis. First, it finds that it reduces suicide rates by 5% of the general population. Additionally, the effects are more prominent in more liberal municipalities.[6] This paper also perform additional analyses to explain suicide reduction. An age-level analysis reveals that the effect is largest for those in their 20s, consistent with the finding of previous studies where younger sexual minorities are more likely to suffer from mental health issues.

Next, using Google search data, this study finds that the search interests of the term "LGBT" increase after adopting the policy in more liberal municipalities. This result implies

[4]Most academic writing is also confined to the roles of soft law in international relations (Weeks, 2016).

[5]This problem is not unique to Japan (King, Semlyen, Tai, Killaspy, Osborn, Popelyuk, and Nazareth, 2008; Hottes, Bogaert, Rhodes, Brennan, and Gesink, 2016).

[6]I use vote shares of the Liberal Democratic Party, the conservative ruling party, against legislation on same-sex marriage as a proxy for the conservativeness of the municipality.

that residents in more liberal municipalities with the policy may become more aware of sexual minorities. By contrast, the number of Google searches for discriminatory words weakly decreases in response to the policy adoption.

Finally, this study conducted an original survey on both sexual minorities and cisgender heterosexuals to obtain repeated cross-section data about their individual attitudes. The survey analysis reveals that levels of subjective happiness of sexual minorities from the more liberal municipalities improved after those municipalities introduced the partnership policy. The analysis also finds suggestive evidence that cisgender heterosexuals from the more liberal municipalities with the partnership policy became more tolerant toward sexual minorities.

In summary, introducing the same-sex partnership policy reduces quarterly suicide rates by 5% of the general population, and the effects of the policy are more significant in more liberal municipalities. The Google Trends analyses suggest that changes in people's attitudes can lead to suicide reduction, which supports the hypothesis that the partnership policy has an expressive function as the mechanism. The original survey analyses, which directly measure the subjective feelings of sexual minorities and cisgender heterosexuals, complement those results. All these results imply that the same-sex partnership policy can improve the welfare of sexual minorities by changing social attitudes toward sexual minorities, especially where people are more likely to accept the new norms proposed by law.

There is a caveat to this finding. The event study analysis implies that the effects on suicide rates disappeared 1.5 years after the policy implementation. By contrast, the effects on the public attitudes toward sexual minorities measured by Google Trends lasted at least 2 years after the policy implementation. Therefore, continuously reducing suicide rates may need complementary policies to the partnership policy, though this study cannot provide conclusive evidence of such.

This study contributes to the literature regarding the expressive function of law (Sunstein, 1996; Cooter, 1998; Posner, 1998; McAdams, 2000; Dharmapala and McAdams, 2003; Benabou and Tirole, 2011). For example, McAdams (2000) develops an attitudinal theory that law can change citizens' behaviors by informing the underlying attitudes of a community or society, presuming that people care about approval from their community.[7] To my

[7]Posner (1998) formulates a model based on the focal point theory. Dharmapala and McAdams (2003)

knowledge, there is little empirical evidence on whether soft law actually implemented can have expressive effects. Related papers that explore expressive effects of hard law actually implemented are Funk (2007), Galbiati, Henry, Jacquemet, and Lobeck (2020), and Aksoy, Carpenter, De Haas, and Tran (2020).[8] While Funk (2007) and Galbiati et al. (2020) do not analyze the laws regarding the rights of minorities, Aksoy et al. (2020) is the closest to this research. They utilize cross-country variations among European countries and find that the adoption of legalized same-sex marriage makes attitudes of individuals toward sexual minorities more tolerant.[9] However, the law that Aksoy et al. (2020) focused on is not soft law, and they do not investigate the effects on the welfare of sexual minorities.[10] Therefore, this study provides new empirical evidence that soft law can have expressive effects and protect the rights of minority or socially disadvantaged groups.

In addition, little is known about the heterogeneity of expressive effects by the initial local norms. Consistent with theoretical conjecture based on McAdams (2017), Chen and Yeh (2014) experimentally show that a liberal court decision has an expressive effect if people come from liberal communities, but it has a backlash effect if people come from conservative communities. Regarding same-sex marriage, previous studies found heterogeneous effects of the legalization on the attitudes toward sexual minorities by the type of implementation (e.g., Ofosu, Chambers, Chen, and Hehman, 2019; Anderson, Matsuzawa, and Sabia, 2021).[11]

---

pay attention to an information aggregation function of the legislative process based on the implications of the Condorcet jury theorem. They argue that legislation can change citizens' behaviors by providing information to them even if the legislators do not have the expertise or enough information in advance. Regarding the interaction between laws and social norms, Acemoglu and Jackson (2017) study how laws shape social norms. They formulate a model, focusing on the feature that law enforcement partly relies on private citizens' cooperation as whistle-blowing.

[8] As experimental evidence, Tyran and Feld (2006) show people tend to comply with rules that are accepted in a referendum even though the rules are not backed by deterrent sanctions. Galbiati and Vertova (2008) also provide experimental evidence that the obligations expressed by law affect individual behavior by conducting a finitely repeated public goods game.

[9] Funk (2007) exploits a policy change in Switzerland that abolished symbolic fines for not voting and shows that average turnout decreases even though the fines are merely symbolic. Galbiati and Vertova (2008) shows that the lockdown measures in the U.K. change the perceived norms about social distancing.

[10] Technically speaking, legalized same-sex marriage means legal same-sex relationship recognition policy. It is worth noting that since policies in Europe provide at least partial legal rights for same-sex couples, they are different from the same-sex partnership policy in question.

[11] Ofosu et al. (2019) find that states in the U.S. which legalized same-sex marriage following the Supreme Court ruling see an increase in implicit and explicit bias against sexual minorities. Contrastingly, they show that the bias decreases in the states that legalized same-sex marriage by state legislation. Consistent with Ofosu et al. (2019), Anderson et al. (2021) find that legalizing same-sex marriage in the U.S. worsens the mental health of youth sexual minorities, especially in the states where same-sex marriage is less popular

However, those studies do not explicitly examine whether heterogenous effects emerge due to the initial norm toward sexual minorities. Therefore, this study provides new evidence on the expressive power of soft law actually implemented and heterogeneity depending on the initial norms of the community.

This study also contributes to the literature that focuses on health-related issues among sexual minorities (Hatzenbuehler, O'Cleirigh, Grasso, Mayer, Safren, and Bradford, 2012; Kail, Acosta, and Wright, 2015; Raifman, Moscoe, Austin, and McConnell, 2017; Anderson et al., 2021; Carpenter, Eppink, Gonzales, and McKay, 2021). While Raifman et al. (2017) find that legalizing same-sex marriage in the U.S. reduces suicide attempts of high school students who are sexual minorities, Anderson et al. (2021) revisit it and provides evidence against Raifman et al. (2017) by using the same data. Thus, the evidence on the effects of legalized same-sex marriage is mixed. Anderson et al. (2021) argue that their findings are consistent with a story of social backlash against sexual minorities. Contrastingly, same-sex partnership policy as soft law can have the advantage of mitigating conflict between the majority and the minority. Thus, the results of this study can be consistent with previous findings if the partnership policy has the advantage. This study also explores the mechanism behind suicide reduction, which neither Raifman et al. (2017) nor Anderson et al. (2021) address. To the best of my knowledge, this research is the first study to empirically show that even soft laws such as the same-sex partnership policy can increase the welfare of sexual minorities if it can change social attitudes.

Finally, this research is related to the studies investigating how institutions such as legalized same-sex marriage affect socioeconomic outcomes for sexual minorities (Aldén, Edlund, Hammarstedt, and Mueller-Smith, 2015; Sansone, 2019; Chen and van Ours, 2020; Hansen, Martell, and Roncolato, 2020; Delhommer and Hamermesh, 2021). Other studies shed light on the effects on social norms or attitudes toward sexual minorities (Bishin, Hayes, Incantalupo, and Smith, 2016; Fernández, Parsa, and Viarengo, 2019; Valencia, Williams, and Pettis, 2019; Baranov, De Haas, and Grosjean, 2020; Aksoy et al., 2020; Brodeur and Haddad, 2021). This research contributes to the literature by providing evidence that soft law can also improve the welfare of sexual minorities and change social attitudes toward them.

and enacted by court order.

The remainder of the paper is organized as follows. Section 2 introduces the background and the data. In section 3, I describe my empirical strategies. Section 4 then presents the main results. Section 5 discusses among whom suicide rates have declined. Sections 6 and 7 shed light on the heterogeneity of the effects and the mechanisms behind the reduction in suicide rates, respectively. Section 8 provides concluding remarks. Finally, the Appendix includes robustness checks.

2 Background and Data

2.1 Background about sexual minorities

Suicide theory predicts that a person commits suicide when their expected lifetime utility falls below a certain threshold (Hamermesh and Soss, 1974; Becker and Posner, 2004; Campaniello, Diasakos, and Mastrobuoni, 2017). Empirical studies also document the correlation between suicide and subjective happiness (Koivumaa-Honkanen, Honkanen, Viinamaeki, Heikkilae, Kaprio, and Koskenvuo, 2001; Koivumaa-Honkanen, Honkanen, Koskenvuo, and Kaprio, 2003; Bray and Gunnell, 2006). Thus, suicide can be considered an extreme aspect of welfare.

Previous studies note that suicide risk among sexual minorities is higher than among cisgender heterosexuals because of severe life events such as discrimination, hatred, or social isolation. Meta-analyses report that suicide attempts among sexual minorities are 2.5-2.75 times higher than among heterosexuals (King et al., 2008; Hottes et al., 2016). Fish, Rice, Lanza, and Russell (2019) further investigates suicide risk among sexual minority across age groups and potential causes for the risk utilizing a nationally representative U.S. sample of adults. They show that sexual minority adults report suicidal behavior four times as likely as heterosexual adults, and the difference is more prominent among younger ages. Additionally, sexual minority adults who experience anti-LGB discrimination express double the amount of suicidal behaviour, and the pattern remains significantly high between ages 18 and 26. In Japan, Hidaka et al. (2008) show that the suicide risk of young gay males is 5.98 times higher than that of heterosexual males and argue it is attributable to discrimination or

hatred against them. Therefore, combined with the fact that about 3.3% of the population are sexual minorities in Japan (Hiramori and Kamano, 2020), a certain number of people may feel that society does not accept them and suffer mental health issues.

Based on the literature, this study uses suicide rates as the primary outcome to investigate the effects of the partnership policy. Also, heterogeneity analysis examines the age-level effects to test whether the effects are more prominent among young age groups, consistent with previous findings. Then, as the mechanism of suicide reduction, this study hypothesizes that it comes from increased social acceptance and decreased discrimination, following Fish et al. (2019). Finally, since suicide is an extreme case and suicide statistics in the main analysis are not limited to sexual minorities, the original survey asks sexual minorities about their subjective happiness. In addition, the survey asks cisgender heterosexuals about their subjective attitudes toward sexual minorities to complement the mechanism analyses.

2.2 Details of the same-sex partnership policy in Japan

Local governments in Japan have been introducing the same-sex partnership policy to improve the situation for sexual minorities by raising awareness about them. For example, one of the mayors of the municipalities with the partnership policy stated: "*Even though the certification is not legally binding, we hope that it will lead to increasing awareness and social understanding about sexual minorities.*"[12] This statement implies that the partnership policy is expected to have an expressive function because the policy itself is not legally binding, and the policymakers rely on the message of the partnership policy to change behavior.

The partnership policy is mainly implemented in municipalities, the smallest administrative unit in Japan. First, Shibuya City and Setagaya City in Tokyo adopted it on November 5, 2015. As of March 1, 2022, 150 municipalities and 6 prefectures (195 municipalities in the prefecture) out of 1741 municipalities have adopted this policy (Minnano Partnership Seido, nda).[13] The population coverage by the partnership policy is about 45.2%.[14] The

[12]The original sentence can be found in the official minutes of the regular assembly on March 3, 2015 (Shibuya City Assembly, 2015).

[13]Some of the municipalities in the 6 prefectures have independently introduced the partnership policy. Thus, those municipalities are excluded from the count of 195 but included in the count of 150 municipalities.

[14]The author calculates the population coverage based on the population in 2021 from Population, Demographics, and Number of Households Based on the Basic Resident Register provided by the Ministry of

main analysis of suicide reduction focuses on the period between January 1, 2009, and December 31, 2019, because the coronavirus pandemic affected trends of suicide rates (Tanaka and Okamoto, 2021; Sakamoto, Ishikane, Ghaznavi, and Ueda, 2021; Nomura, Kawashima, Yoneoka, Tanoue, Eguchi, Gilmour, Kawamura, Harada, and Hashizume, 2021; Ueda, Nordström, and Matsubayashi, 2022). By December 31, 2019, 30 municipalities and 1 prefecture (44 municipalities in the prefecture) out of 1741 municipalities adopted this policy, which covers 15.6% of the entire population in Japan.[15] Table 1 lists the date of the adoption for each municipality.[16]

Under the same-sex partnership policy, municipalities recognize same-sex couples' relationships by issuing a certificate to the couples. The following points characterize the partnership policy. First, the objective is to create an inclusive society regardless of gender identity and sexual orientation by officially recognizing same-sex couples. Thus, this study hypothesizes that introducing the same-sex partnership policy can broadly affect social attitudes toward sexual minorities. Second, generally, to register the policy requires that at least one partner be a registered resident of the municipality or scheduled to move into the municipality, though the eligibility requirements are slightly different between municipalities. Third, there are no legal effects, such as granting inheritance rights, as opposed to heterosexual marriage. The following paragraphs explain why the partnership policy can be categorized as non-legally binding. Finally, while the municipality only issues a certificate, it may have some practical effects beneficial to couples with the certificate. For example, hospitals can grant hospital visitation rights, or the couples can apply for public housing. Some private companies or organizations also provide services previously targeting a legally married couple, such as family discounts or family care sick leave, to couples with this certificate (Minnano Partnership Seido, ndb). However, this policy cannot oblige private companies or organizations because it is not legally binding.

The same-sex partnership policy has two kinds of institutionalization. First, as of Decem-

Internal Affairs and Communications.

[15]The author calculates the population coverage based on the population in 2019 from Population, Demographics, and Number of Households Based on the Basic Resident Register provided by the Ministry of Internal Affairs and Communications.

[16]This study focuses on municipalities as of the Census in 2015. When using data before 2015, I aggregate all data at the municipality level in 2015 using a municipal code converter that accounts for municipal mergers by Kondo (2019).

ber 31, 2019, in almost all municipalities except three, the main ground is an internal rule of each local government, which binds only the local government.[17] Thus, it does not need approval by the local assembly and can be introduced by the mayor's decision (Tanamura and Nakagawa, 2016). Conversely, the three exceptional municipalities have adopted the same-sex partnership policy based on municipal ordinance, which needs to pass the legislative process in contrast to the case of the internal rule.

Both types of implementation can satisfy the definition of soft law. First, Gersen and Posner (2008) define soft law as a rule issued by a lawmaking authority that does not comply with formalities or understandings necessary for the rule to be legally binding. Thus, the first type of implementation straightforwardly satisfies the definition of soft law since a mayor can adopt the partnership policy based on an internal rule without the legislative process. Second, Gersen and Posner (2008) further argue that laws which satisfy the procedural requirements for legislation but do not have formal legal effects also resemble soft law. Following this classification, partnership policies based on the municipal ordinance are also soft law because the Civil Code does not legalize same-sex marriage, and municipalities cannot independently provide legal protections for same-sex couples. Additionally, the courts do not have a consensus that considers registered couples under the policy as common-law marriage couples as of this writing (Sogabe, 2020).

2.3 Data

2.3.1 Suicide statistics

The data for suicide statistics are obtained from Basic Data on Suicide in the Region, provided by the Ministry of Health, Labor, and Welfare. This study uses the monthly suicide statistics by municipality from January 2009 to December 2019.[18] Note that the data are

[17] The three exceptions are Shibuya City, Toshima City, and Soja City.

[18] The data set contains two types of statistics. The first one is provisional statistics ("Zanteichi"). Provisional suicide statistics for the current month are created based on information available through the middle of the following month. For example, provisional suicide statistics for January would be created with information through mid-February. The second one is finalized statistics ("Kakuteichi"). These statistics are generated by updating the provisional values based on new information in the following year. The Ministry of Health, Labor, and Welfare provided only provisional values for municipality and monthly level statistics until 2020. However, the ministry changed its policy following the increase in suicides due to the coronavirus pandemic and began providing definitive statistics at the municipal and monthly levels from 2020 onward.

not limited to sexual minorities. I discuss how to deal with this issue in Section 5.

2.3.2 Municipality-level socioeconomic status

I use municipality-level population data to calculate the suicide rates per population. The data are from population, demographics, and number of households based on the Basic Resident Register provided by the Ministry of Internal Affairs and Communications. I also use other socioeconomic variables such as the number of employees by industry or population density to control for potential confounding factors. These data are from the Census in 2010.

Table 2 provides the summary statistics of municipality-level characteristics. Columns (1), (2), and (3) report the means and standard deviations of the characteristics for the municipalities that have not introduced the same-sex partnership policy (control group), the municipalities that have introduced the policy (treatment group), and the all, respectively. Column (4) reports the differences in each characteristic between treatment and control groups. Overall, urban municipalities are more likely to introduce the same-sex partnership policy. In the empirical analyses below, in addition to exploiting the differences in timing of the introduction of the policy, I include the characteristics $\times$ time fixed effects to control for heterogeneous trends caused by the different characteristics.

3 Empirical strategy

This section explains the main empirical strategies. I use a difference-in-differences method to quantify the effect of the same-sex partnership policy on suicide rates and employ an event study analysis to visualize the effect.

The analyses mainly uses quarterly suicide data from 2009 to 2019. The monthly data may not be suitable to capture the treatment effects precisely because suicide rates can have seasonal trends and be noisy (Yu, Yang, Kim, Hashizume, Gasparrini, Armstrong, Honda, Tobias, Sera, Vicedo-Cabrera, et al., 2020). Additionally, many municipalities have zero values in their monthly statistics. On the other hand, annual-level data may not be suitable

Accordingly, the ministry replaced provisional statistics for 2019 with finalized statistics on their website to compare the trend with 2020 onward. Nevertheless, this paper continues to use provisional statistics since it would be suitable for comparison over time with 2018 and earlier.

to identify treatment effects because, as shown in Table 1, many municipalities adopted the same-sex partnership policy in the middle of the year. Therefore, this study decided to use quarterly data. A robustness check uses annual and monthly suicide statistics.

3.1 Difference-in-Differences

I exploit variations in the different timings of the introduction of the same-sex partnership policy. First, I employ a difference-in-differences design to quantify the magnitude of the effect on suicide rates. The estimated equation is as follows:

$$SuicideRate_{m,q} = \beta_1 Partnership_{m,q} + \gamma_m + \theta_q + \mathbf{Z}_m^{'}\boldsymbol{\theta}_q(+\lambda_{p,q}) + \epsilon_{m,q} \tag{1}$$

where subscript m, q, and p indicate municipality, quarter, and prefecture, respectively.

$SuicideRate_{m,q}$ is calculated by dividing the number of suicides in municipality m and quarter q by the 100,000 population in municipality m in 2009. $Partnership_{m,q}$ takes 1 after municipality m introduces the same-sex partnership policy in quarter q, and take 0 otherwise.[19] γ_m captures a municipality fixed effect. Including this municipality fixed effect allows for control of all municipality-level confounds that vary from one municipality to another, such as local customs related to attitudes toward suicide. θ_q is a quarter fixed effect and $\lambda_{p,q}$ is a prefecture-quarter fixed effect that captures prefecture specific factors, such as a suicide prevention policy adopted by the prefecture. $\mathbf{Z}_m$ is a set of municipality m socioeconomic variables in 2010, which includes the population density, the share of the population aged 65 years or above, the sex ratio, the ratio of foreign residents, the share of employment in primary industry, and the share of employment in tertiary industry. Thus, $\mathbf{Z}_m^{'}\boldsymbol{\theta}_q$ controls for the differential evolution of suicide rates depending on the initial characteristics of municipalities, such as the effect of the business cycle, which can be different across municipalities with different industrial structures.[20] $\epsilon_{m,q}$ is an error term. All regressions are weighted by the population of each municipality in 2009 because the suicide rate is noisily measured in municipalities with small populations. In addition, standard errors are

[19] Regarding the one prefecture with the same-sex partnership policy, I assume that every municipality in the prefecture introduces the same-sex partnership policy. The same is true for subsequent estimations.

[20] Municipality-level socioeconomic variables that are observed monthly or annually are limited.

clustered by municipalities.

3.2 Event study

I additionally explore how suicide rates respond to the introduction of the same-sex partnership policy utilizing an event study design. Since only 6 municipalities have more than 2 years for the post-treatment period, I focus on 8 quarters before and after the introduction of the partnership policy. The following equation is estimated:

$$\begin{aligned} SuicideRate_{m,q} = \beta_{-8}\mathbb{1}(QuarterSinceTreatment_{m,q} \leq -8) \\ + \sum_{l=-7,\neq -1}^{l=7} \beta_l \mathbb{1}(QuartersSinceTreatment_{m,q} = l) \\ + \beta_8 \mathbb{1}(QuarterSinceTreatment_{m,q} \geq 8) \\ + \gamma_m + \mathbf{Z}_m^{'}\boldsymbol{\theta}_q + \lambda_{p,q} + \epsilon_{m,q} \end{aligned} \tag{2}$$

where subscript m, q, and p indicate municipality, quarter, and prefecture, respectively.

$QuartersSinceTreatment_{m,q} = l$ is a dummy variable assigned 1 if quarter q is equaled to the plus or minus l quarter from the introduction of the same-sex partnership policy, otherwise 0. The other variables are the same as those defined in section 3.1. Since $l = -1$ is omitted, all coefficients are relative to the outcome in the last quarter before the introduction.

4 Results

4.1 Quarterly-level difference-in-differences results

Table 3 shows the regression result estimated in equation (1). Column (1) documents that the effect of introducing the same-sex partnership policy is statistically significant and the magnitude is substantially large. This result indicates that the partnership policy introduction significantly reduces quarterly suicides per 100,000 population by 0.272, corresponding to a 5% reduction in the average quarterly suicide rates. In column (2), the direction of the effect is almost the same as that without prefecture $\times$ quarter fixed effects in column (1).

Using prefecture × quarter fixed effects, I can additionally control for prefecture-quarter level confounds such as suicide prevention adopted at the prefecture level. In columns (3) and (4), I add one-quarter lagged suicide rates to both columns (1) and (2) to control for the prior trends of quarterly suicide rates. Columns (3) and (4) document that the effects remain statistically significant and the magnitudes are quantitatively similar to those without controlling for prior trends. Overall, these estimates show that introducing the same-sex partnership policy reduces suicide rates of the general population.[21]

The Appendix presents robustness checks. First, I adopt alternative specifications to check the validity of assumptions in the two-way fixed effects model in Appendix A.1. I employ a method proposed by Borusyak, Jaravel, and Spiess (2021) to deal with the limitations of the two-way fixed effect model and further control for a municipality-linear trend. Second, Appendix A.2 discusses the existence of outliers that may drive the results by using a synthetic control method. Third, Appendix A.3 investigates confounding effects from other policies. I conduct a test of migration to the municipalities with the partnership policy because those municipalities might have become attractive in general and saw an increase in migration due to other policies such as diversity-oriented or redistribution policies. I additionally include further characteristics of municipalities that may influence the types of policies to be implemented in the baseline specification. In addition, I perform placebo analyses that estimated the effects of the same-sex partnership policy on death rates from major causes other than suicide and municipality-level budget expenditures. Finally, Appendix A.4 addresses the potential concern that the baseline regression may compare apples to oranges because the municipalities with the partnership policy tend to be urban areas, and the number of treatments is relatively small compared to controls. In addition, one treatment has the partnership policy introduced at the prefecture level (a larger administrative and geograph-

[21] I provide the annual-level difference-in-differences results in Table A1 and the monthly-level difference-in-differences results in Table A2, which also document the statistically significant effects.

Since the Census data are available every five years, I also provide the results in which the municipality-level controls are updated up to 2015 in Table A3. It shows that introducing the same-sex partnership policy still has significant effects on suicide reduction though the coefficients become smaller when a prefecture-quarter fixed effect is included. In Table A3, I drop the municipalities that have been designated for "Evacuation order zone," Planned evacuation zone," or "Emergency evacuation preparation zone" due to the accident at Tokyo Electric Power Company (TEPCO)'s Fukushima Daiichi Nuclear Power Station in 2011 (Fukushima Prefecture, 2019) since data in the 2015 Census are affected by those orders.

ical unit than the other treatments), which could have different effects from other units. To address those issues, Appendix A.4 reports three results: (1) difference-in-differences analysis excluding towns and villages and using only cities, (2) difference-in-differences analysis excluding the prefecture-level treatment, and (3) employing matching methods proposed by Imai, Kim, and Wang (2019). The results do not imply that the concern undermines the baseline results.

4.2 Quarterly-level event study result

Next, I show a graphical result of the same-sex partnership policy introduction on quarterly suicide rates in municipalities. Figure 1 shows the estimated coefficients obtained in equation (2) and the 95% confidence intervals. Since one quarter prior to the introduction of the partnership policy is the reference quarter, β_{-1} is 0 by definition, and the other coefficients indicate how the quarterly suicide rates change over time relative to the prior quarter of the event. In Figure 1, the estimates of β_j are almost flat before $q = 0$. This result suggests that suicide rates did not change systematically before introducing the policy. By contrast, after $q = 0$, the coefficients take larger negative values compared with those before $q = 0$, and some of them are statistically different from 0.[22]

In addition, I employ an annual-level event study analysis, which allows me to test for longer prior trends and to examine the effects in the long term. I estimate the annual version of equation (2).[23] Figure A1 shows the estimates of β_j and the 95% confidence intervals. A year before the policy introduction is the reference year. In Figure A1, the estimates of β_j are not statistically different from 0 before year $y = 0$ and show decreasing trends after that. The 95% confidence intervals become wider after year $y = 2$, possibly because only a few treatments have post-treatment periods over 2 years. Overall, the result is consistent with that obtained in the quarterly-level analysis.

I additionally conduct a monthly-level event study analysis to observe the effects over a

[22]This result shows that the effects disappear around 1.5 years after the policy implementation. Section 8 discusses this point.

[23]In the annual-level event study, the estimation window is from 5 years prior to 4 years after the policy introduction since the annual suicide statistics are available from 2009 to 2019, and the first municipalities introduced the same-sex partnership policy in 2015.

shorter period. The estimated equation is the monthly version of equation (2).[24] Figure A2 plots the estimates of β_j and the 95% confidence intervals. The coefficients are almost flat before the policy introduction, implying that there are no differential pre-trends even over short periods. After month $t = 0$, it shows decreasing trends, and some of the coefficients are negative and statistically significant. This result suggests that the effects appeared immediately after the policy introduction.

5 Discussion about whose suicide rates decreased

This section provides suggestive evidence that the main result can come from suicide reduction among sexual minorities since the suicide data in the main analyses do not distinguish between sexual minorities and cisgender heterosexuals. In addition, one may think that a 5% reduction in suicide rates is too significant considering the 3.3% population share of sexual minorities. Although it is challenging to explore whose suicide rates decrease in the data directly, I conduct two analyses to complement the main results. First, I perform a back-of-the-envelope calculation that accounts for the relatively high suicide risks among sexual minorities. The results estimate that introducing the same-sex partnership policy may reduce suicide among sexual minorities by at most 31%, which is not unrealistic.[25]

Second, I conduct age-level analyses because it can be predicted that the effects would

[24]This analysis limits the estimation window to 7 months either side of the policy introduction because more than two-thirds of the treatment municipalities in the data sample have introduced the policy after April 1st, 2019.

[25]Following Iwamoto, Hiramori, Naito, and Nakano (2019), I calculate the quarterly average suicide rates among sexual minorities by the following equation:

$$SuicideRate \times \frac{Share \times RelativeRisk}{Share \times RelativeRisk + (100 - Share)}$$

where SuicideRate is the quarterly average of suicide rates, Share is the share of the population of sexual minorities, and RelativeRisk is relative suicide risk of sexual minorities. I assumed that $RelativeRisk = 6$ based on Hidaka et al. (2008), similar to Iwamoto et al. (2019), and $Share = 3.3$ based on Hiramori and Kamano (2020). In the calculation, $SuicideRate$ is set equal to 5.151, which is the mean of the dependent variable reported in Table 3.

Regarding the above calculation, the number of $RelativeRisk$ from Hidaka et al. (2008) is not a relative risk of completed suicide, but a relative risk of suicide attempts. Although using data about suicide attempts is not a perfect approximation, the literature suggests that patients with suicide attempts are more likely to die by suicide (Bostwick and Pankratz, 2000; Bostwick, Pabbati, Geske, and McKean, 2016). Thus, the above calculation substitutes the relative risk of completed suicide with that of suicide attempts because there are no reliable data about the relative risk of completed suicide to date.

be more likely to appear in the younger generation. Younger sexual minorities are more likely to suffer from mental health issues (Hidaka, 2016). Thus, I conduct age-level analyses to test this prediction. For the age-level analysis, I estimate equation (1) by age group.[26] I create six age groups: under 20, 20-29, 30-39, 40-49, 50-59, 60 or above. Table 4 reports the obtained results. Beginning with column (1), the dependent variable is suicide rates of under 20, 20-29, 30-39, 40-49, 50-59, 60 or above. The coefficient of $Partnership_{m,q}$ in column (2) is statistically significant and the magnitude is large compared with the other age group.

I additionally provide a graphical result of the event study estimation by using quarterly suicide statistics for those under 20, those in their 30s, and those 30 or above. The specification is the same as equation (2).[27] Figure 2 shows all results to scale, including the graph in Figure 1 in the same scale. The top right panel depicts the result for those in the twenties. Notably, the effects are substantially larger than those in the other age groups and there are no observable differential pre-trends since the estimates of β_j are almost flat before $q = 0$.

Furthermore, Section 7.2 provides evidence that introducing the partnership policy increases subjective happiness among sexual minorities. Thus, all these results seem consistent with the hypothesis that the effects come from sexual minorities, though further investigations are needed to identify among whom suicide rates declined clearly.

6 Heterogenous effects by the conservativeness of each municipality

This section explores the heterogeneity of soft law's expressive effects by the community's initial descriptive norms. However, there is no municipality-level data in Japan about the attitudes toward sexual minorities or diversity of gender identity and sexual orientation. Therefore, I use vote shares of the Liberal Democratic Party (LDP), the conservative ruling party, against legislation on same-sex marriage, as a proxy for the conservativeness of the

[26] When estimating the equation, I use inverse variance weighting by the population of each age group in municipality m in 2009.

[27] When estimating the equation, I use inverse variance weighting by the population of each age group in municipality m in 2009.

municipality.[28]

I interact the $Partnership_{m,q}$ variable in equation (1) with the vote shares of non-LDP parties to estimate the heterogenous effects on suicide rates as follows:

$$\begin{aligned} SuicideRate_{m,q} = \beta_1 Partnership_{m,q} + \beta_2 Partnership_{m,q} \times NonLDPshare_m \\ + \gamma_m + \theta_q + \mathbf{Z}_m^{'} \boldsymbol{\theta}_q (+\lambda_{p,q}) + \epsilon_{m,q}. \end{aligned} \tag{3}$$

$NonLDPshare_m$ is the average of non-LDP vote shares at the lower house elections (2009, 2012, 2014, 2017) at municipality m. I then normalize the variable so that the mean is assigned 0 to make it interpretable. The other variables are the same as in equation (1).

The second row in Table 5 reports the coefficients on the interaction term. Since the $NonLDPshare_m$ is zero-mean normalized, the results imply that the effects of introducing the partnership policy become more prominent if the baseline vote share of non-LDP parties at the municipality m is larger than the average. Although the magnitudes are smaller if prefecture-quarter fixed effects are included in columns (2) and (4) compared with columns (1) and (3), the coefficients are negative and statistically significant. Hence, the directions of the effect are similar in all specifications. Overall, these results suggest that the effects of introducing the partnership policy can vary depending on the initial norms about the diversity of gender identity and sexual orientation.

In addition, I perform the event study analysis to see the long-term effects and whether pre-trends of suicide rates differ by the vote share of non-LDP parties. The estimated

[28]The data about the number of votes for each candidate in each municipality is obtained from the Survey on Results of House-of-Representatives General Election and Supreme Court Review.

equation is as follows:

$$
\begin{aligned}
SuicideRate_{m,q} = {} & \alpha_{-8}\mathbb{1}(QuarterSinceTreatment_{m,q} \leq -8) \\
& + \sum_{l=-7,\neq -1}^{l=7} \alpha_l \mathbb{1}(QuartersSinceTreatment_{m,q} = l) \\
& + \alpha_8 \mathbb{1}(QuarterSinceTreatment_{m,q} \geq 8) \\
& + \beta_{-8}\mathbb{1}(QuarterSinceTreatment_{m,q} \leq -8) \times NonLDPshare_m \\
& + \sum_{l=-7,\neq -1}^{l=7} \beta_l \mathbb{1}(QuartersSinceTreatment_{m,q} = l) \times NonLDPshare_m \\
& + \beta_8 \mathbb{1}(QuarterSinceTreatment_{m,q} \geq 8) \times NonLDPshare_m \\
& + \gamma_m + \mathbf{Z}_m^{'}\boldsymbol{\theta}_q + \lambda_{p,q} + \epsilon_{m,q},
\end{aligned}
\tag{4}
$$

where subscript m, q, and p indicate municipality, quarter, and prefecture, respectively. I add interaction terms between time dummies in equation (2) and $NonLDPshare_m$. The other variables are the same as equation (2).

Figure A3 shows only the coefficients on the interaction terms, β. The coefficients are not significantly different from 0 before the policy introduction, which implies that different prior trends between treatments and controls do not exist. By contrast, the coefficients are negative and significantly different from 0 in some periods after the policy introduction, though they are imprecisely estimated in the last three periods. This result can complement the result of the static two-way fixed-effects regression.

7 Mechanism

This section sheds light on the mechanisms behind the reduction of suicide rates in response to the introduction of the same-sex partnership policy, in discussing two points. First, I explore changes in social attitudes toward sexual minorities using data from Google Trends. Second, I exploit data from original internet surveys on individual attitudes of cisgender heterosexuals and sexual minorities to complement the previous results because they all rely on aggregated data.

7.1 Google Trends analysis

One possible explanation for the suicide reduction among sexual minorities is that the majority of residents become aware of sexual minorities via the introduction of the policy, and discrimination against sexual minorities decreases. Some anecdotes support this conjecture. Chi (2016) argues that introducing the policy brings more awareness that sexual minorities exist in Japan. Hiramori and Kamano (2020) provide survey evidence and show that those who personally know sexual minorities are less likely to express hostility.

In order to explore this channel, I use Google search data to show that the awareness of sexual minorities increases after the policy introduction. I also show that the use of discriminatory words in Google searches decreases.

7.1.1 Data

Google Trends provides a scaled internet search volume of terms or topics in a specific period or region, showing the relative popularity of them based on the highest value, ranging from 0 to 100. A value of 100 indicates that the keyword is most popular with 50 indicating that it is half as popular. A value of 0 indicates that there was not enough data for that keyword.[29]

Google Trends data has advantages when researchers explore attitudes related to sexual matters or discrimination. First, it is frequent time series data available for many countries or smaller geographical units such as states and cities from millions of aggregated searches. Second, people tend to express even socially unacceptable thoughts without hesitation because it is online and likely alone, in contrast with traditional surveys (Stephens-Davidowitz, 2018). Thus, some studies utilize Google Trends data to study prejudice or discrimination (Stephens-Davidowitz, 2014; Sansone, 2019).

Despite the advantages, using Google Trends for comparison across regions also has a limitation. The values are generated by random sampling of searches from the specific period and the region. Thus, it can introduce substantial sampling variation depending on submitted queries and the underlying population size of the region (Eichenauer, Indergand, Martínez, and Sax, 2022). Therefore, following Eichenauer et al. (2022), I take the average

[29]The help page explains the detail (`https://support.google.com/trends/answer/4365533?hl=en&ref_topic=6248052`, accessed on December 9, 2022).

of 12 samples drawn for each keyword and normalize the series so that the average and the standard deviation over the period equals 0 and 1, respectively.

This study queries the series for three words ("LGBT," "Homo," and "Rezu" in Japanese) from January 2009 to December 2019 at the prefecture level because municipality level data are not available. "LGBT" is an acronym for lesbian, gay, bisexual, and transgender. The search interest of "LGBT" can be a good indicator of how widespread the social awareness of sexual minorities is since it is generally one of the most common words for sexual minorities. "Homo" and "Rezu" are abbreviations of homosexual and lesbian, respectively. However, these words correspond to "Fag" and "Dike" in English and are considered politically incorrect because of their historical context (Li, 2020). Thus, these terms can reflect the discriminatory attitudes of Google search users.

7.1.2 Empirical strategy

First, I employed a difference-in-differences analysis to quantify the effect of introducing the partnership policy on the search interests. The estimation relies on the following prefecture-level regression because the Google Trends data are available only at the prefecture level, though the partnership policies are mainly adopted at the smaller geographical unit (municipality). Specifically, the estimated equation is as follows:

$$Search_{p,q} = \beta_1 Partnership_{p,q} + \beta_2 Partnership_{p,q} \times NonLDPshare_p + \gamma_p + \theta_q + \mathbf{Z}_p^{'}\boldsymbol{\theta}_q + \epsilon_{p,q}, \tag{5}$$

where subscript p and q indicate prefecture and quarter, respectively. $Search_{p,q}$ is the standardized value of the quarterly average of search interests queried from Google Trends. $Partnership_{p,q}$ is a dummy assigned 1 after the first municipality in prefecture p, quarter q introduces the partnership policy, and 0 otherwise. The other variables are the prefecture version of equation (3).

The results are shown in Table 6. Columns (1), (3), (5), and (7) report the baseline results of the search interest of "LGBT," "Homo," "Rezu," and the mean of "Homo" and "Rezu," respectively. In columns (2), (4), (6), and (8) one-quarter lagged search interest of each word is added, respectively, to control for prior trends. In column (1), the coefficient on

$Partnership_{p,q} \times NonLDPshare_p$ is statistically significant and the magnitude is large. It corresponds to a 0.16 standard deviation increase of the search interest of "LGBT" if the non-LDP vote share at the municipality with the partnership policy is larger by a 10% point than the average. Column (2) also reports a substantially large and statistically significant effect. By contrast, columns (5) and (6) document that the effects of introducing the partnership policy on the search interest of "Rezu" are negative and marginally significant, though the effects on the search interest of "Homo" and the mean of the two words are still negative but not statistically significant in columns (3), (4), (7), and (8).

Table A11 shows the results of replacing the $Partnership_{p,q}$ with the population share of the municipalities with the partnership policy in prefecture p in quarter q to reflect variations of treatment intensity in each prefecture. Since the direction of the effects is the same as in Table 6, the baseline results are robust to different specifications.

I also employ an event study analysis to visualize the effects. I estimate the following equation:

$$\begin{aligned} Search_{p,q} = {} & \alpha_{-8}\mathbb{1}(QuatersSinceTreatment_{p,q} \leq -8) \\ & + \sum_{l=-7,\neq -1}^{l=7} \alpha_l \mathbb{1}(QuatersSinceTreatment_{p,q} = l) \\ & + \alpha_8 \mathbb{1}(QuarterSinceTreatment_{p,q} \geq 8) \\ & + \beta_{-8}\mathbb{1}(QuatersSinceTreatment_{p,q} \leq -8) \times NonLDPshare_p \\ & + \sum_{l=-7,\neq -1}^{l=7} \beta_l \mathbb{1}(QuatersSinceTreatment_{p,q} = l) \times NonLDPshare_p \\ & + \beta_8 \mathbb{1}(QuarterSinceTreatment_{p,q} \geq 8) \times NonLDPshare_p \\ & + \gamma_p + \theta_q + \mathbf{Z}_p^{'}\boldsymbol{\theta}_q + \epsilon_{p,q}, \end{aligned} \tag{6}$$

where subscript p and q indicate prefecture and quarter, respectively. $Search_{p,q}$ is the same as that in equation (5) and the other variables are the prefecture-quarter version of equation (4).

Figure 3 only shows the estimated coefficients on the interaction terms between period dummies and the non-LDP vote share for each word. Panel (A) in Figure 3 documents that

the coefficients increase after the introduction of the partnership policy, $q = 0$. Thus, the results suggest that the introduction of the partnership policy increases social awareness of sexual minorities. In panels (C) and (D) in Figure 3, the coefficients decrease gradually after the policy was introduced, $q = 0$, though all the point estimates are not statistically different from 0 in panel (B).

Combining all of these results, the introduction of the partnership policy may increase social awareness of sexual minorities, especially in more liberal municipalities. By contrast, it may weakly decrease the discriminatory attitudes toward sexual minorities. In addition, these effects appear in relatively short periods, aligning with the literature, as many of the treatment municipalities introduced the partnership policy in 2019 and also implied in Figure 3. First, the expressive function theory predicts that people act according to descriptive norms. Second, previous empirical studies regarding legalized same-sex marriage also find the immediate effects of positive social attitudes toward sexual minorities (e.g., Sansone, 2019; Aksoy, Carpenter, Frank, and Huffman, 2019).[30]

7.2 Survey analyses

7.2.1 Data

I conducted internet surveys twice on cisgender heterosexuals and sexual minorities and created repeated cross-section data. The survey subjects were registered panelists of a survey company, Cross Marketing Inc., in Japan. The first survey period was from March 22, 2021, to March 25, 2021, and the second was from March 11, 2022, to March 17, 2022.[31] I asked the registered panelists about their gender identity and sexual orientation and identified sexual minorities or heterosexual cisgenders in the same manner as Hiramori and Kamano (2020). In the first survey, I recruited 1,500 cisgender heterosexuals from the municipalities that had introduced the same-sex partnership policy by March 1, 2021, and the geographically

[30] Sansone (2019) also uses search volumes of discriminatory words toward sexual minorities from Google Trends, same as this study.

[31] A trial survey was conducted in 2019. Since the low appearance rate of sexual minorities required to send the questionnaire to many registered panelists, it was distributed by first prioritizing the trial participants.

bordering municipalities, respectively, and also 1,500 sexual minorities in the same way.[32] The second survey had two sampling groups. The first group was sampled from the control municipalities in the first survey, consisting of the repeated cross-section data combined with the first survey because some of the control municipalities in the first survey introduced the partnership policy between March 2, 2021, and March 1, 2022.[33] The second group was sampled from the municipalities that were not included in the first survey but have introduced the same-sex partnership policy between March 2, 2021, and March 1, 2022, and the geographically bordering municipalities. I use the second group for the cross-sectional analysis that estimates the effect of the partnership policy on a new outcome not included in the first survey as a robustness check.

Table A12 provides the summary statistics of municipality-level characteristics in the repeated cross-section data (municipalities included in the first and second surveys). This suggests that the treatment and control municipalities have similar characteristics. Table A13 summarizes the characteristics of respondents in the repeated cross-section data who are sexual minorities, and Table A14 summarizes the characteristics of those who are cisgender heterosexuals.

7.2.2 Outcomes

For cisgender heterosexuals, I asked about their comfort level when a sexual minority is their (1) political leader, (2) colleague, and (3) child's partner, following the manner used by Eurobarometer (European Commission, Brussels, 2020). Respondents answered on a scale from 10 (Totally comfortable) to 1 (Totally uncomfortable). In the questionnaire, a sexual minority is classified into three cases: (1) a gay, lesbian, or bisexual person, (2) a transgender person, and (3) an intersex person. Then, I calculated the mean over the type of sexual minorities and created a dummy variable assigned 1 if the calculated value is equal to or greater than 5 and 0 otherwise as an indicator that the respondent is comfortable or moderately

[32] Cross Marketing Inc. uses a proprietary algorithm to drop respondents from the sample who do not respond rationally.

[33] The survey was commissioned to Cross Marketing Inc. with the information on the treatment municipalities as of March 1, 2021. The survey then started on March 22, 2021. However, one municipality (Ageo City, Saitama) introduced the partnership policy on March 16, 2021, just before the survey started. In addition, the municipality (Ageo City, Saitama) is included in the control group because it borders the other treatment municipalities. Therefore, the main analyses exclude the municipality (Ageo City, Saitama).

comfortable with sexual minorities, following Eurobarometer (European Commission, 2019).

For sexual minorities, I first asked about their subjective happiness on the following five-category scale: "Very happy," "A little happy," "A little unhappy," "Unhappy," and "I don't know." Then, I constructed a dummy variable assigned 1 if the respondents answered "Very happy" or "A little happy" and 0 if the answer is "A little unhappy" or "Unhappy." I dropped the observations answering "I don't know." The literature suggests a positive association between lower subjective happiness and committing suicide (Bray and Gunnell, 2006). Therefore, if the main results of suicide reduction come from the reduction among sexual minorities, their subjective level of happiness is higher in the municipalities with the policy.

Next, I use four measurements to assess the degree of social inclusion of sexual minorities: Political participation, participation in various social associations, outness about sexual orientation, and experienced discrimination or hatred. Political participation is measured by whether the respondents have voted at the national or local elections in the last 4 years (between 2017 and 2020 in the first survey and between 2018 and 2021 in the second survey). Regarding participation in various social associations, I asked the respondents how actively they participated in social associations such as sports or recreational organizations regarding the categories: "Active member," "Inactive member," or "Don't belong." This question is based on the questionnaire used in the World Values Survey (Inglehart, Haerpfer, Moreno, Welzel, Kizilova, Diez-Medrano, Lagos, Norris, Ponarin, Puranen, et al., 2014). Then, I coded them in order from 3 to 1 and calculated the mean. The outness about sexual orientation is based on the Outness Inventory constructed by Mohr and Fassinger (2000) and translated into Japanese to assess the degree to which the respondents are open about their sexual orientation. In the questionnaire, the respondents answered how openly they talk about their sexual orientation to specific figures or types of figures such as their mother or work peers on a scale from 7 (Openly talked) to 1 (Never talked). In the regression, I use the mean over the figures. Finally, following Eurobarometer (European Commission, Brussels, 2020), I asked about their experienced discrimination or hatred in the last year and constructed a dummy variable assigned 1 if they have not faced discrimination or hatred and 0 otherwise.

In addition, in the second survey, I asked cisgender heterosexuals and sexual minorities whether they assumed their municipality had introduced the same-sex partnership policy. This variable is used to check the validity of the assumption of the expressive function theory where people know the implementation of the law, though the analysis relies on cross-sectional data.

7.2.3 Estimation

Using the above outcomes, I estimate the effect of introducing the same-sex partnership policy by the municipality-level difference-in-differences analysis. The estimated equation is as follows:

$$\begin{aligned} Outcome_{i,m,y} = {} & \beta_1 Partnership_m + \beta_2 After_y + \beta_3 NonLDPshare_m \\ & + \beta_4 Partnership_m \times After_y + \beta_5 Partnership_m \times NonLDPshare_m \\ & + \beta_6 After_y \times NonLDPshare_m \\ & + \beta_7 Partnership_m \times After_y \times NonLDPshare_m \\ & + \beta_8 After_y \times \boldsymbol{Z}_{m,2015}(+B_m + B_m \times After_y) + \epsilon_{i,m,y} \end{aligned} \tag{7}$$

where i, m, and y indicate respondent, municipality, and year (2021 or 2022), respectively. $Partnership_m$ is assigned 1 if municipality m has introduced the same-sex partnership policy between March 2, 2021, and March 1, 2022, and 0 otherwise. $After_y$ is assigned 1 in the second survey data, and 0 otherwise. $NonLDPshare_m$ is the same as in equation (3). $\boldsymbol{Z}_{m,2015}$ is a set of municipality m socioeconomic variables in 2015, which includes the same factors as in equation (3).[34] B_m is a bordering municipality fixed effect. The inclusion of the bordering municipality fixed effect allows me to control for regional characteristics, such as local customs related to attitudes toward suicide, at a fine level. Note that all the outcome variables are coded so that the coefficient in OLS would be positive if the comfort level of cisgender heterosexuals, the subjective level of happiness of sexual minorities, and the levels of social inclusion of sexual minorities are higher in the municipalities with the policy.

[34] In this estimation, I use the variables in 2015. I also use inverse variance weighting by the population in municipality m in 2015.

7.2.4 Results

Table 7 shows the results regarding sexual minorities. In even numbered columns, a bordering municipality fixed effect is included. The coefficients on the triple interaction term, $Partnership_m \times After_y \times NonLDPshare_m$, in columns (1) and (2) are the positive and statistically significant. These results imply that introducing the partnership policy increases the subjective happiness of sexual minorities, especially in the municipalities with a higher non-LDP vote share. Nevertheless, the other results related to the social inclusion of sexual minorities in columns (3) to (10) show that introducing the partnership policy does not have statistically significant effects on the degree of their social inclusion.

Table 8 shows the results regarding cisgender heterosexuals. Same as Table 7, a bordering municipality fixed effect is included in even numbered columns. The coefficients on the triple interaction term, $Partnership_m \times After_y \times NonLDPshare_m$, in columns (5) and (6) are positive and statistically significant. These results imply that introducing the partnership policy makes cisgender heterosexuals feel more comfortable when their child's partner is a sexual minority. Although the coefficients are not statistically significant in columns (1) to (4), the effects are positive if the bordering municipality fixed effect is included. Overall, these results weakly suggest that cisgender heterosexuals may become more comfortable with sexual minorities after the partnership policy was introduced in more liberal municipalities.

Appendix Table A15 reports the result of a cross-section analysis, which regresses a dummy variable assigned 1 if respondents assume their municipality has introduced the same-sex partnership policy and 0 otherwise on $Partnership_m$. Table A15 documents that the introduction of the partnership policy is positively correlated with the awareness among sexual minorities and cisgender heterosexuals in the municipality. This result is consistent with the assumption of the expressive function theory.[35]

[35]Another possible way to estimate the effects on awareness about the partnership policy is using Google Trends. Unfortunately, it is impossible to do the same difference-in-differences analysis as Section 7.1 because the search volumes of words related to the partnership policy are almost zero in many prefectures, especially those without the partnership policy, between January 1, 2009, and December 31, 2019. For example, Google Trends returns the search volumes of "same-sex partnership policy" only for Tokyo, where 6 municipalities have introduced the policy. When querying the term "same-sex partnership", it returns the volumes for Hokkaido, Kanagawa, Osaka, and Tokyo, where 1, 4, 5, and 6 municipalities have introduced the policy, respectively. Regarding the term "partnership policy", it returns the volumes for Okinawa, where 1 municipality has introduced the policy. This pattern suggests that people in municipalities with the partnership policy are more likely to use the related words in Google because all the prefectures with non-zero interests

8 Conclusion

Today, protecting minority rights, especially the rights of sexual minorities, is at the center of policy interests. However, different types of policy implementations may have different consequences. Thus, it is important to examine what kind of implementations have practical effects. This study examines the effect of soft law on the welfare of sexual minorities in the context of the same-sex partnership policy adopted by municipalities in Japan.

Using the difference-in-differences estimation and the event study analysis, I find evidence that introducing the same-sex partnership policy decreases suicide rates of the general population by 5%. The effects are more prominent in more liberal municipalities. I use vote shares of the Liberal Democratic Party, the conservative ruling party, against legislation on same-sex marriage, as a proxy for the conservativeness of the municipality.

Exploring the mechanism, I first show that the number of Google searches for the word "LGBT" increases after the introduction of the partnership policy, especially where the vote share of non-LDP is higher. By contrast, I document that the number of Google searches for discriminatory words weakly decrease after the introduction of the partnership policy, especially where the vote share of non-LDP is higher. These results imply that introducing the partnership policy may improve the awareness of sexual minorities and reduce discriminatory attitudes toward them in more liberal municipalities.

Analyses using original survey data complement those results. I find consistent evidence that introducing the partnership policy increases the subjective happiness of sexual minorities, especially where the vote share of non-LDP is higher. This result can support the hypothesis that the suicide reduction comes from sexual minorities. However, the analysis does not find statistically significant effects that sexual minorities are more integrated into society after introducing the partnership policy. Regarding cisgender heterosexuals, I find suggestive evidence that introducing the partnership policy makes them more tolerant toward sexual minorities in more liberal municipalities, consistent with the Google search analyses.

The results have a remark. The event study analysis reveals that the effects on suicide

have municipalities with the partnership policy. Nevertheless, the data is not enough to do formal analyses.

rates disappeared 1.5 years after the policy implementation, in contrast to the Google Trend analyses. The effects on the attitudes toward sexual minorities from the Google Trend analyses remain for at least 2 years. However, this study cannot shed light on the reasons behind the short-lived effects.

There is room for improvement in this research. First, the findings may be specific to the Japanese context because Japanese people tend to obey the government even if the government does not force them to do so. Thus, it is important to investigate whether this result applies to different contexts or what conditions are necessary to make soft law effective, particularly the relationships between the expressive power of soft law and the dominant norms in the community. Further studies may advance an understanding of these points. Second, in the main analysis, suicide statistics are not limited to sexual minorities due to the lack of available statistics. Third, the effects on suicide rates are short-lived in this analysis. Exploring whether the soft law approach is insufficient to reduce suicide rates continuously or whether the short-lived effects are unique to the setting of this study will be future tasks. Finally, since the survey analyses rely on repeated cross-sectional data, individual-specific unobservables not captured in the current specification may cause bias in the results. Since the number of municipalities planning to adopt the same-sex partnership policy is increasing, it may be possible to construct individual-level panel data before and after the implementation, allowing one to analyze how they change their attitudes more precisely. This is also an important task for future research.

References

Abadie, A., A. Diamond, and J. Hainmueller (2010). Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. *Journal of the American statistical Association 105*(490), 493–505.

Acemoglu, D. and M. O. Jackson (2017). Social norms and the enforcement of laws. *Journal of the European Economic Association 15*(2), 245–295.

Aksoy, C. G., C. S. Carpenter, R. De Haas, and K. D. Tran (2020). Do laws shape attitudes?

Evidence from same-sex relationship recognition policies in Europe. *European Economic Review 124*, 103399.

Aksoy, C. G., C. S. Carpenter, J. Frank, and M. L. Huffman (2019). Gay glass ceilings: Sexual orientation and workplace authority in the UK. *Journal of Economic Behavior & Organization 159*, 167–180.

Aldén, L., L. Edlund, M. Hammarstedt, and M. Mueller-Smith (2015). Effect of registered partnership on labor earnings and fertility for same-sex couples: Evidence from Swedish register data. *Demography 52*(4), 1243–1268.

Amnesty International (2021, June 1). Japan: Government must deliver an LGBTI bill that ensures zero tolerance of discrimination. `https://www.amnesty.org/en/latest/news/2021/06/japan-government-must-deliver-an-lgbti-bill-that-ensures-zero-tolerance-of-discrimination/`. Accessed on September 28, 2021.

Anderson, D. M., K. Matsuzawa, and J. J. Sabia (2021). Marriage equality laws and youth mental health. *The Journal of Law and Economics 64*(1), 29–51.

Baranov, V., R. De Haas, and P. Grosjean (2020). Queens of the desert: Convictism and marital attitudes across Australia. In *AEA Papers and Proceedings*, Volume 110, pp. 457–62.

Becker, G. S. and R. A. Posner (2004). Suicide: An economic approach. Working paper, University of Chicago.

Benabou, R. and J. Tirole (2011). Laws and norms. Working Paper 17579, National Bureau of Economic Research.

Bishin, B. G., T. J. Hayes, M. B. Incantalupo, and C. A. Smith (2016). Opinion backlash and public attitudes: Are political advances in gay rights counterproductive? *American Journal of Political Science 60*(3), 625–648.

Borusyak, K., X. Jaravel, and J. Spiess (2021). Revisiting event study designs: Robust and efficient estimation. *Unpublished working paper, version May 19*, 2021.

Bostwick, J. M., C. Pabbati, J. R. Geske, and A. J. McKean (2016). Suicide attempt as a risk factor for completed suicide: even more lethal than we knew. *American journal of psychiatry 173*(11), 1094–1100.

Bostwick, J. M. and V. S. Pankratz (2000). Affective disorders and suicide risk: a reexamination. *American Journal of Psychiatry 157*(12), 1925–1932.

Bray, I. and D. Gunnell (2006). Suicide rates, life satisfaction and happiness as markers for population mental health. *Social psychiatry and psychiatric epidemiology 41*(5), 333–337.

Brodeur, A. and J. Haddad (2021). Institutions, attitudes and LGBT: Evidence from the Gold Rush. *Journal of Economic Behavior & Organization 187*, 92–110.

Callaway, B. and P. H. Sant'Anna (2020). Difference-in-differences with multiple time periods. *Journal of Econometrics*.

Campaniello, N., T. M. Diasakos, and G. Mastrobuoni (2017). Rationalizable suicides: Evidence from changes in inmates' expected length of sentence. *Journal of the European Economic Association 15*(2), 388–428.

Carpenter, C. S., S. T. Eppink, G. Gonzales, and T. McKay (2021). Effects of Access to Legal Same-Sex Marriage on Marriage and Health. *Journal of Policy Analysis and Management 40*(2), 376–411.

Chen, D. L. and S. Yeh (2014). The construction of morals. *Journal of Economic Behavior & Organization 104*, 84–105.

Chen, S. and J. C. van Ours (2020). Symbolism matters: The effect of same-sex marriage legalization on partnership stability. *Journal of Economic Behavior & Organization 178*, 44–58.

Chi, N. (2016). What is at the End of the Rainbow?: Prospects and Challenges for Sexual Minorities in Japan. *Annals of Public Policy 10*, 75–85.

Cooter, R. (1998). Expressive law and economics. *The Journal of Legal Studies 27*(S2), 585–607.

De Chaisemartin, C. and X. d'Haultfoeuille (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review 110*(9), 2964–96.

Delhommer, S. M. and D. S. Hamermesh (2021). Same-Sex Couples and the Gains to Marriage: The Importance of the Legal Environment. *Journal of Policy Analysis and Management*.

Dharmapala, D. and R. H. McAdams (2003). The Condorcet jury theorem and the expressive function of law: a theory of informative law. *American Law and Economics Review 5*(1), 1–31.

Eichenauer, V. Z., R. Indergand, I. Z. Martínez, and C. Sax (2022). Obtaining consistent time series from google trends. *Economic Inquiry 60*(2), 694–705.

Eskridge, W. N. J. (2002). *Equality practice: Civil unions and the future of gay rights*. New York: Routledge.

European Commission (2019). Factsheet on the social acceptance of lgbti people in the eu - 2019. Eurobarometer on the social acceptance of LGBTIQ people in the EU - 2019. `https://ec.europa.eu/info/policies/justice-and-fundamental-rights/combatting-discrimination/lesbian-gay-bi-trans-and-intersex-equality/eurobarometer-social-acceptance-lgbtiq-people-eu-2019_en`. Accessed on November 27, 2022.

European Commission, Brussels (2020). Eurobarometer 91.4 (2019). GESIS Data Archive, Cologne. ZA7575 Data file Version 1.0.0, https://doi.org/10.4232/1.13429.

Fernández, R., S. Parsa, and M. Viarengo (2019). Coming out in america: Aids, politics, and cultural change. Working Paper 25697, National Bureau of Economic Research.

Fish, J. N., C. E. Rice, S. T. Lanza, and S. T. Russell (2019). Is young adulthood a critical period for suicidal behavior among sexual minorities? results from a us national sample. *Prevention Science 20*(3), 353–365.

Fukushima Prefecture (2019, March 3). Transition of evacuation designated zones. https://www.pref.fukushima.lg.jp/site/portal-english/en03-08.html. Accessed on August 16, 2021.

Funk, P. (2007). Is there an expressive function of law? An empirical analysis of voting laws with symbolic fines. *American Law and Economics Review 9*(1), 135–159.

Galbiati, R., E. Henry, N. Jacquemet, and M. Lobeck (2020). How laws affect the perception of norms: Empirical evidence from the lockdown. Working paper 684710, SSRN.

Galbiati, R. and P. Vertova (2008). Obligations and cooperative behaviour in public good games. *Games and Economic Behavior 64*(1), 146–170.

Gersen, J. E. and E. A. Posner (2008). Soft law: lessons from congressional practice. *Stan. L. Rev. 61*, 573.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*.

Hamermesh, D. S. and N. M. Soss (1974). An economic theory of suicide. *Journal of Political Economy 82*(1), 83–98.

Hansen, M. E., M. E. Martell, and L. Roncolato (2020). A labor of love: The impact of same-sex marriage on labor supply. *Review of Economics of the Household 18*(2), 265–283.

Hatzenbuehler, M. L., C. O'Cleirigh, C. Grasso, K. Mayer, S. Safren, and J. Bradford (2012). Effect of same-sex marriage laws on health care use and expenditures in sexual minority men: A quasi-natural experiment. *American journal of public health 102*(2), 285–291.

Hidaka, Y. (2016, March). Gei Baisekushual Dansei No Kenkō Repōto [Gay and Bisexual Men's Health Report]. https://www.health-issue.jp/Health_Report_2015.pdf. Accessed on August 30, 2021.

Hidaka, Y., D. Operario, M. Takenaka, S. Omori, S. Ichikawa, and T. Shirasaka (2008). Attempted suicide and associated risk factors among youth in urban Japan. *Social psychiatry and psychiatric epidemiology 43*(9), 752–757.

Hiramori, D. and S. Kamano (2020, March 4). Asking about Sexual Orientation and Gender Identity in Japan: Case Report on the Osaka City Residents' Survey and Related Preparatory Studies. Working paper, SocArXiv.

Hottes, T. S., L. Bogaert, A. E. Rhodes, D. J. Brennan, and D. Gesink (2016). Lifetime prevalence of suicide attempts among sexual minority adults by study sampling strategies: A systematic review and meta-analysis. *American journal of public health 106*(5), e1–e12.

Imai, K., I. S. Kim, and E. H. Wang (2019). Matching methods for causal inference with time-series cross-sectional data. *American Journal of Political Science*.

Inglehart, R., C. Haerpfer, A. Moreno, C. Welzel, K. Kizilova, J. Diez-Medrano, M. Lagos, P. Norris, E. Ponarin, B. Puranen, et al. (2014). World values survey: Round six-country-pooled datafile version. *Madrid: JD Systems Institute*, 12.

Iwamoto, T., D. Hiramori, S. Naito, and S. Nakano (2019). Seiteki Mainoriti No Jisatsu Utsu Ni Yoru Shakaitekisonshitsu No Shisan To Hitoujisha Tono Shūnyūkakusa Ni Kansuru Sābei [A survey on the estimation of social loss due to suicide and depression among sexual minorities and the income gap with non-minorities]. JILPT Discussion Paper 5, The Japan Institute for Labour Policy and Training.

Kail, B. L., K. L. Acosta, and E. R. Wright (2015). State-level marriage equality and the health of same-sex couples. *American Journal of Public Health 105*(6), 1101–1105.

King, M., J. Semlyen, S. S. Tai, H. Killaspy, D. Osborn, D. Popelyuk, and I. Nazareth (2008). A systematic review of mental disorder, suicide, and deliberate self harm in lesbian, gay and bisexual people. *BMC psychiatry 8*(1), 1–17.

Koivumaa-Honkanen, H., R. Honkanen, M. Koskenvuo, and J. Kaprio (2003). Self-reported happiness in life and suicide in ensuing 20 years. *Social psychiatry and psychiatric epidemiology 38*(5), 244–248.

Koivumaa-Honkanen, H., R. Honkanen, H. Viinamaeki, K. Heikkilae, J. Kaprio, and M. Koskenvuo (2001). Life satisfaction and suicide: A 20-year follow-up study. *American Journal of Psychiatry 158*(3), 433–439.

Kollman, K. (2009). European institutions, transnational networks and national same-sex unions policy: when soft law hits harder. *Contemporary politics 15*(1), 37–53.

Kondo, K. (2019, March). Municipality-level panel data and municipal mergers in japan. Technical Paper 19-T-001, RIETI.

Li, K. (2020, July 13). Before You Call Me 'Gaijin'. *nippon.com*,. https://www.nippon.com/en/japan-topics/g00859/. Accessed on December 27, 2020.

McAdams, R. H. (2000). An attitudinal theory of expressive law. *Or. L. Rev. 79*, 339.

McAdams, R. H. (2017). *The Expressive Powers of Law: Theories and Limits*. Cambridge: Harvard University Press.

Ministry of Health, Labour and Welfare (n.d.). Summary of vital statistics. https://www.mhlw.go.jp/english/database/db-hw/populate/index.html. Accessed on October 29, 2021.

Minnano Partnership Seido (n.d.a). Nihon Zenkoku Partnership Seido Ichiran [List of Partnership Policy in Japan]. https://minnano-partnership.com/partnership/all. Accessed on November 14, 2022.

Minnano Partnership Seido (n.d.b). Partnership Seido De Dekirukoto [What you can do with the partnership policy]. https://minnano-partnership.com/partnership/merit. Accessed on November 14, 2022.

Mohr, J. and R. Fassinger (2000). Measuring dimensions of lesbian and gay male experience. *Measurement and evaluation in counseling and development 33*(2), 66–90.

National Council of Japan Nuclear Free Local Authorities (2020). Hikakusengen Jichitai Ichiran [List of Local Governments that have Declared themselves Nuclear Free]. http://www.nucfreejapan.com/uploads/2020/10/20200930_%E9%9D%9E%E6%A0%B8%E5%AE%A3%E8%A8%80%E8%87%AA%E6%B2%BB%E4%BD%93%E4%B8%80%E8%A6%A7.pdf. Accessed on December 10, 2022.

Nomura, S., T. Kawashima, D. Yoneoka, Y. Tanoue, A. Eguchi, S. Gilmour, Y. Kawamura, N. Harada, and M. Hashizume (2021). Trends in suicide in japan by gender during the covid-19 pandemic, up to september 2020. *Psychiatry research 295*, 113622.

OECD (2019). *Society at a Glance 2019: OECD Social Indicators*. Paris: OECD Publishing.

Ofosu, E. K., M. K. Chambers, J. M. Chen, and E. Hehman (2019). Same-sex marriage legalization associated with reduced implicit and explicit antigay bias. *Proceedings of the National Academy of Sciences 116*(18), 8846–8851.

Parkinson, L. (1989). Japan's equal employment opportunity law: An alternative approach to social change. *Columbia Law Review 89*(3), 604–661.

Posner, E. A. (1998). Symbols, signals, and social norms in politics and the law. *The Journal of Legal Studies 27*(S2), 765–797.

Raifman, J., E. Moscoe, S. B. Austin, and M. McConnell (2017). Difference-in-differences analysis of the association between state same-sex marriage policies and adolescent suicide attempts. *JAMA pediatrics 171*(4), 350–356.

Robbins, M. W. and S. Davenport (2021). microsynth: Synthetic control methods for disaggregated and micro-level data in R. *Journal of Statistical Software 97*(2), 1–31.

Sakamoto, H., M. Ishikane, C. Ghaznavi, and P. Ueda (2021). Assessment of suicide in japan during the covid-19 pandemic vs previous years. *JAMA network open 4*(2), e2037378–e2037378.

Sansone, D. (2019). Pink work: Same-sex marriage, employment and discrimination. *Journal of Public Economics 180*, 104086.

Shibuya City Assembly (2015). Shibuya-ku Heisei 27th March regular assembly (1st meeting) March 3 No.3. `https://ssp.kaigiroku.net/tenant/shibuya/SpMinuteView.html?power_user=false&tenant_id=394&council_id=1126&schedule_id=3&view_years=2015%23#`. Accessed on August 13, 2021.

Sogabe, M. (2020). Status of same-sex marriage legislation in japan. *NTU L. Rev. 15*, 1.

Stephens-Davidowitz, S. (2014). The cost of racial animus on a black candidate: Evidence using google search data. *Journal of Public Economics 118*, 26–40.

Stephens-Davidowitz, S. (2018). *Everybody lies: What the internet can tell us about who we really are*. Bloomsbury Publishing.

Sun, L. and S. Abraham (2020). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*.

Sunstein, C. R. (1996). On the expressive function of law. *University of Pennsylvania law review 144*(5), 2021–2053.

Tanaka, T. and S. Okamoto (2021). Increase in suicide following an initial decline during the covid-19 pandemic in japan. *Nature human behaviour 5*(2), 229–238.

Tanamura, M. and S. Nakagawa (Eds.) (2016). *Dousei Pātonāshippu Seido: Sekai No Doukou Nihon No Jichitai Ni Okeru Dounyū No Jissai To Tenbou [Same-sex partnership policy: Global trends, the reality of adoption in Japanese municipalities and the prospects]*. Tokyo: Nihon Kajo Shuppan.

Tyran, J.-R. and L. P. Feld (2006). Achieving compliance when legal sanctions are non-deterrent. *scandinavian Journal of Economics 108*(1), 135–156.

Ueda, M., R. Nordström, and T. Matsubayashi (2022). Suicide and mental health during the covid-19 pandemic in japan. *Journal of Public Health 44*(3), 541–548.

Valencia, Z., B. Williams, and R. Pettis (2019). Pride and prejudice: Same-sex marriage legalization announcements and lgbt hate-crimes. *Available at SSRN 3362835*.

Weeks, G. (2016). *Soft Law and Public Authorities : Remedies and Reform (Hart Studies in Comparative Public Law)*. Oxford: Hart Publishing Ltd.

Witteveen, W. and B. Van Klink (1999). Why is soft law really law? a communicative approach to legislation. *Regel Maat 14*(3), 126–140.

Yu, J., D. Yang, Y. Kim, M. Hashizume, A. Gasparrini, B. Armstrong, Y. Honda, A. Tobias, F. Sera, A. M. Vicedo-Cabrera, et al. (2020). Seasonality of suicide: a multi-country multi-community observational study. *Epidemiology and psychiatric sciences 29*.

Table 1: Dates of introduction of the same-sex partnership policy

Municipality	Date
Setagaya Ku, Tokyo	2015-11-05
Shibuya Ku, Tokyo	2015-11-05
Iga Shi, Mie	2016-04-01
Takarazuka Shi, Hyogo	2016-06-01
Naha Shi, Okinawa	2016-07-08
Sapporo Shi, Hokkaido	2017-06-01
Fukuoka Shi, Fukuoka	2018-04-01
Osaka Shi, Osaka	2018-07-09
Nakano Ku, Tokyo	2018-08-20
Oizumi Machi, Gumma	2019-01-01
Chiba Shi, Chiba	2019-01-29
Toshima Ku, Tokyo	2019-04-01
Edogawa Ku, Tokyo	2019-04-01
Fuchu Shi, Tokyo	2019-04-01
Yokosuka Shi, Kanagawa	2019-04-01
Odawara Shi, Kanagawa	2019-04-01
Sakai Shi, Osaka	2019-04-01
Hirakata Shi, Osaka	2019-04-01
Soja Shi, Okayama	2019-04-01
Kumamoto Shi, Kumamoto	2019-04-01
Kanuma Shi, Tochigi	2019-06-03
Miyazaki Shi, Miyazaki	2019-06-10
Ibaraki Prefecture	2019-07-01
Kitakyushu Shi, Fukuoka	2019-07-01
Nishio Shi, Aichi	2019-09-01
Nagasaki Shi, Nagasaki	2019-09-02
Sanda Shi, Hyogo	2019-10-11
Katano Shi, Osaka	2019-11-22
Kawasaki Shi, Kanagawa	2019-12-01
Kamakura Shi, Kanagawa	2019-12-04
Daito Shi, Osaka	2019-12-04
Mitoyo Shi, Kagawa	2020-01-01
Amagasaki Shi, Hyogo	2020-01-06
Osaka Prefecture	2020-01-22
Saitama Shi, Saitama	2020-04-01
Minato Ku, Tokyo	2020-04-01
Bunkyo Ku, Tokyo	2020-04-01
Sagamihara Shi, Kanagawa	2020-04-01
Zushi Shi, Kanagawa	2020-04-01
Niigata Shi, Niigata	2020-04-01
Hamamatsu Shi, Shizuoka	2020-04-01
Nara Shi, Nara	2020-04-01
Yamatokoriyama Shi, Nara	2020-04-01
Takamatsu Shi, Kagawa	2020-04-01
Koga Shi, Fukuoka	2020-04-01
Kijo Cho, Miyazaki	2020-04-01
Kawagoe Shi, Saitama	2020-05-01
Toyoake Shi, Aichi	2020-05-01
Itami Shi, Hyogo	2020-05-15
Ashiya Shi, Hyogo	2020-05-17
Hayama Machi, Kanagawa	2020-07-01
Inabe Shi, Mie	2020-07-01
Tondabayashi Shi, Osaka	2020-07-01
Okayama Shi, Okayama	2020-07-01
Kawanishi Shi, Hyogo	2020-08-01
Kyoto Shi, Kyoto	2020-09-01
Kaizuka Shi, Osaka	2020-09-01
Sakado Shi, Saitama	2020-10-01
Koganei Shi, Tokyo	2020-10-20
Tochigi Shi, Tochigi	2020-11-01
Kitamoto Shi, Saitama	2020-11-01
Matsudo Shi, Chiba	2020-11-01
Kokubunji Shi, Tokyo	2020-11-15
Konosu Shi, Saitama	2020-12-01
Hirosaki Shi, Aomori	2020-12-10
Gumma Prefecture	2020-12-21
Shibukawa Shi, Gumma	2020-12-21
Miura Shi, Kanagawa	2021-01-01
Yoshinogawa Shi, Tokushima	2021-01-01
Higashikagawa Shi, Kagawa	2021-01-01
Hiroshima Shi, Hiroshima	2021-01-04
Akashi Shi, Hyogo	2021-01-08
Okegawa Shi, Saitama	2021-02-01
Yokohama Shi, Kanagawa	2021-02-01
Tokushima Shi, Tokushima	2021-02-01
Kochi Shi, Kochi	2021-02-01
Ina Machi, Saitama	2021-03-01
Yamato Shi, Kanagawa	2021-03-01
Kameoka Shi, Kyoto	2021-03-01
Ageo Shi, Saitama	2021-03-16
Annaka Shi, Gumma	2021-04-01
Gyoda Shi, Saitama	2021-04-01
Honjo Shi, Saitama	2021-04-01
Koshigaya Shi, Saitama	2021-04-01
Miyoshi Machi, Saitama	2021-04-01
Adachi Ku, Tokyo	2021-04-01
Kunitachi Shi, Tokyo	2021-04-01
Fujisawa Shi, Kanagawa	2021-04-01
Chigasaki Shi, Kanagawa	2021-04-01
Matsumoto Shi, Nagano	2021-04-01
Fuji Shi, Shizuoka	2021-04-01
Toyohashi Shi, Aichi	2021-04-01
Nishinomiya Shi, Hyogo	2021-04-01
Inagawa Cho, Hyogo	2021-04-01
Tenri Shi, Nara	2021-04-01
Ikoma Shi, Nara	2021-04-01
Kitajima Cho, Tokushima	2021-04-01
Tonosho Cho, Kagawa	2021-04-01
Shodoshima Cho, Kagawa	2021-04-01
Tadotsu Cho, Kagawa	2021-04-01
Usuki Shi, Oita	2021-04-01
Nichinan Shi, Miyazaki	2021-04-01
Ibusuki Shi, Kagoshima	2021-04-01
Nobeoka Shi, Miyazaki	2021-04-26
Urayasu Shi, Chiba	2021-05-01
Chiyoda Machi, Gumma	2021-06-01
Nagaokakyo Shi, Kyoto	2021-06-01
Higashimatsuyama Shi, Saitama	2021-07-01
Minamiashigara Shi, Kanagawa	2021-07-01
Oi Machi, Kanagawa	2021-07-01
Kanazawa Shi, Ishikawa	2021-07-01
Toyota Shi, Aichi	2021-07-16
Saga Prefecture	2021-08-27
Mie Prefecture	2021-09-01
Nikko Shi, Tochigi	2021-09-01
Iruma Shi, Saitama	2021-09-01
Ube Shi, Yamaguchi	2021-09-01
Miyoshi Shi, Tokushima	2021-09-01
Shintomi Cho, Miyazaki	2021-09-01
Kuki Shi, Saitama	2021-10-01
Moroyama Machi, Saitama	2021-10-01
Kawajima Machi, Saitama	2021-10-01
Matsuda Machi, Kanagawa	2021-10-01
Hikone Shi, Shiga	2021-10-01
Muko Shi, Kyoto	2021-10-01
Bizen Shi, Okayama	2021-10-01
Akitakata Shi, Hiroshima	2021-10-01
Karatsu Shi, Saga	2021-10-01
Ozu Machi, Kumamoto	2021-10-01
Urasoe Shi, Okinawa	2021-10-01
Sayama Shi, Saitama	2021-10-11
Naka Cho, Tokushima	2021-11-01
Tokigawa Machi, Saitama	2021-12-01
Koshu Shi, Yamanashi	2021-12-01
Kurashiki Shi, Okayama	2021-12-01
Maniwa Shi, Okayama	2021-12-01
Zentsuji Shi, Kagawa	2021-12-01
Ebino Shi, Miyazaki	2021-12-01
Hakusan Shi, Ishikawa	2021-12-10
Funabashi Shi, Chiba	2021-12-16
Soka Shi, Saitama	2021-12-20
Tokorozawa Shi, Saitama	2022-01-01
Hanno Shi, Saitama	2022-01-01
Hidaka Shi, Saitama	2022-01-01
Mihara Shi, Hiroshima	2022-01-01
Kagoshima Shi, Kagoshima	2022-01-01
Gamagori Shi, Aichi	2022-01-04
Yoshikawa Shi, Saitama	2022-02-01
Ichikawa Shi, Chiba	2022-02-01
Tama Shi, Tokyo	2022-02-01
Ayase Shi, Kanagawa	2022-02-01
Samukawa Machi, Kanagawa	2022-02-01
Aomori Prefecture	2022-02-07
Ebetsu Shi, Hokkaido	2022-03-01
Mima Shi, Tokushima	2022-03-01
Kamimine Cho, Saga	2022-03-01

Table 2: Municipality-level characteristics in 2010

	Control	Treatment	All	Difference
Avg. quarterly suicides per 100,000 population	6.649	5.999	6.621	-0.649***
	[4.287]	[1.532]	[4.209]	(0.207)
Population	63497.269	300100.068	73553.907	236602.799***
	[134571.553]	[594948.111]	[185658.594]	(69239.806)
Population density	973.878	2731.897	1048.601	1758.019***
	[2272.351]	[4583.773]	[2439.534]	(535.751)
Population share under 15	12.613	12.930	12.626	0.317
	[2.266]	[1.770]	[2.248]	(0.213)
Population share of those aged 15 to 64	59.092	63.927	59.298	4.834***
	[5.225]	[3.064]	[5.243]	(0.378)
Population share over 64	28.052	22.643	27.822	-5.409***
	[7.016]	[3.762]	[6.994]	(0.470)
Share of employment in primary industry	11.839	5.382	11.564	-6.456***
	[10.701]	[5.881]	[10.621]	(0.732)
Share of employment in secondary industry	26.270	27.658	26.329	1.388
	[8.273]	[8.854]	[8.300]	(1.049)
Share of employment in tertiary industry	61.892	66.960	62.107	5.068***
	[10.292]	[11.901]	[10.412]	(1.406)
Sex ratio	51.673	50.737	51.633	-0.936***
	[1.770]	[1.296]	[1.762]	(0.157)
Share of foreign residents	0.811	1.598	0.844	0.787***
	[0.902]	[1.789]	[0.969]	(0.209)
Observations	1667	74	1741	1741

Notes: For each characteristic, the followings are reported: Column (1): mean and standard deviation for the municipalities that have not introduced the same-sex partnership policy (control group); Column (2): mean and standard deviation for the municipalities that have introduced the same-sex partnership policy (treatment group); Column (3): mean and standard deviation for the all; Column (4): difference between the treatment and the control groups and the standard error. Standard deviations and standard errors are presented in square brackets and parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: The effects on quarterly suicide rates

Dependent variable is:	Quarterly suicide rates per 100,000 population			
	(1)	(2)	(3)	(4)
$\text{Partnership}_{m,q}$	-0.272**	-0.222**	-0.286**	-0.240**
	(0.139)	(0.110)	(0.135)	(0.112)
$\text{SuicideRate}_{m,q-1}$			-0.009**	-0.014***
			(0.004)	(0.004)
R^2	0.192	0.215	0.189	0.212
Observations	76,604	76,604	74,863	74,863
Mean dep. var.	5.151	5.151	5.151	5.151
Municipality F.E.s	✓	✓	✓	✓
Quarter F.E.s	✓	✓	✓	✓
Prefecture-Quarter F.E.s		✓		✓
Controls × Quarter F.E.s	✓	✓	✓	✓

Notes: The table reports coefficients on the dummy that takes 1 after the introduction of the same-sex partnership policy at municipality m in quarter q, and one-quarter lagged quarterly suicide rates. All specifications include municipality fixed effects, quarter fixed effects, and controls × quarter fixed effects. Controls are municipality-level socioeconomic factors in 2010 that contain the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. All regressions are weighted by the population in each municipality in 2009. Standard errors clustered at the municipality level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table 4: The effects on quarterly suicide rates by age group

Dependent variable is:	Quarterly suicide rates per 100,000 population					
	Under 20	20s	30s	40s	50s	60+
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Partnership}_{m,q}$	-0.014	-0.482**	-0.198	-0.359	-0.237	-0.285*
	(0.060)	(0.225)	(0.217)	(0.265)	(0.302)	(0.163)
R^2	0.036	0.053	0.064	0.054	0.079	0.108
Observations	76,604	76,604	76,604	76,604	76,604	76,604
Mean dep. var.	0.527	4.209	4.901	6.335	6.324	6.980
Municipality F.E.s	✓	✓	✓	✓	✓	✓
Quarter F.E.s	✓	✓	✓	✓	✓	✓
Controls × Quarter F.E.s	✓	✓	✓	✓	✓	✓

Notes: The table reports coefficients on the dummy that takes 1 after the introduction of the same-sex partnership policy at municipality m in quarter q, and one-quarter lagged quarterly suicide rates. All specifications include municipality fixed effects, quarter fixed effects, and controls × quarter fixed effects. Controls are municipality-level socioeconomic factors in 2010 which contains the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. All regressions are weighted by the population of each age group in each municipality in 2009. Standard errors clustered at the municipality level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table 5: The heterogenous effects on quarterly suicide rates by vote share of non-LDP parties

Dependent variable is:	Quarterly suicide rates per 100,000 population			
	(1)	(2)	(3)	(4)
$\text{Partnership}_{m,q}$	0.056	0.006	0.028	-0.021
	(0.139)	(0.124)	(0.136)	(0.126)
$\text{Partnership}_{m,q}$	-2.774**	-1.873**	-2.654**	-1.799**
$\times$Vote share of Non-LDP_m	(1.181)	(0.813)	(1.153)	(0.819)
$\text{SuicideRate}_{m,q-1}$			-0.009**	-0.014***
			(0.004)	(0.004)
R^2	0.192	0.215	0.189	0.212
Observations	76,604	76,604	74,863	74,863
Mean dep. var.	5.151	5.151	5.151	5.151
Municipality F.E.s	✓	✓	✓	✓
Quarter F.E.s	✓	✓	✓	✓
Prefecture-Quarter F.E.s		✓		✓
Controls $\times$ Quarter F.E.s	✓	✓	✓	✓

Notes: The table reports coefficients on the dummy that takes 1 after the introduction of the same-sex partnership policy at municipality m in quarter q, the interaction term with average vote shares of non-LDP parties at municipality m in lower house elections (2009, 2012, 2014, 2017), and one-quarter lagged quarterly suicide rates. All specifications include municipality fixed effects, quarter fixed effects, and controls $\times$ quarter fixed effects. Controls are municipality-level socioeconomic factors in 2010 which contains the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. All regressions are weighted by the population in each municipality in 2009. Standard errors clustered at the municipality level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table 6: The effects on the Google search interest

Dependent variable is:	Normalized quarterly average of search interest of							
	LGBT		Homo		Rezu		Mean of Homo and Rezu	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{Partnership}_{p,q}$	-0.008	-0.010	-0.143*	-0.123*	-0.018	-0.009	-0.088	-0.069
	(0.043)	(0.043)	(0.077)	(0.067)	(0.065)	(0.054)	(0.064)	(0.054)
$\text{Partnership}_{p,q}$	1.634***	1.632***	-0.108	-0.191	-1.545*	-1.281*	-0.916	-0.827
$\times$Vote share of Non-LDP$_p$	(0.265)	(0.268)	(0.969)	(0.863)	(0.843)	(0.686)	(0.845)	(0.734)
$\text{Search}_{p,q-1}$		0.012		0.115***		0.171***		0.143***
		(0.027)		(0.037)		(0.033)		(0.044)
R^2	0.935	0.935	0.761	0.766	0.859	0.862	0.871	0.873
Observations	2,068	2,021	2,068	2,021	2,068	2,021	2,068	2,021
Prefecture F.E.s	✓	✓	✓	✓	✓	✓	✓	✓
Quarter F.E.s	✓	✓	✓	✓	✓	✓	✓	✓
Controls $\times$ Quarter F.E.s	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The table reports coefficients on the dummy that takes 1 after the first municipality in prefecture p introduces the same-sex partnership policy in quarter q, the interaction term with average vote shares of non-LDP parties at prefecture p in lower house elections (2009, 2012, 2014, 2017), and one-quarter lagged search interest of each word. All specifications include prefecture fixed effects, quarter fixed effects, and controls $\times$ quarter fixed effects. Controls are prefecture-level socioeconomic factors in 2010 which contains the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. All regressions are weighted by the population in each prefecture in 2009. Standard errors clustered at the prefecture level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table 7: The effects on subjective happiness and social inclusing of sexual minorities

Dependent variable is:	Subjective Happiness		Social participation		Voting experience		Levels of outness		Discrimination or hatred	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\text{Partnership}_m \times \text{After}_y$	-0.053	-0.108*	0.003	-0.029	0.006	0.002	-0.278	-0.447*	0.018	0.008
	(0.056)	(0.064)	(0.037)	(0.042)	(0.057)	(0.080)	(0.188)	(0.231)	(0.029)	(0.037)
$\text{Partnership}_m \times \text{After}_y$	1.012**	1.059*	0.127	0.299	-0.537	-0.284	-0.159	1.666	-0.312	-0.499
$\times$Vote share of Non-LDP$_m$	(0.505)	(0.602)	(0.541)	(0.657)	(0.431)	(0.606)	(1.723)	(2.178)	(0.269)	(0.307)
R^2	0.010	0.017	0.010	0.020	0.013	0.019	0.015	0.022	0.011	0.022
Observations	2,710	2,710	2,972	2,972	2,958	2,958	2,463	2,463	2,574	2,574
Bordered municipalities F.E.s		✓		✓		✓		✓		✓
Bordered municipalities-After$_y$ F.E.s		✓		✓		✓		✓		✓
Controls $\times$ After$_y$ F.E.s	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

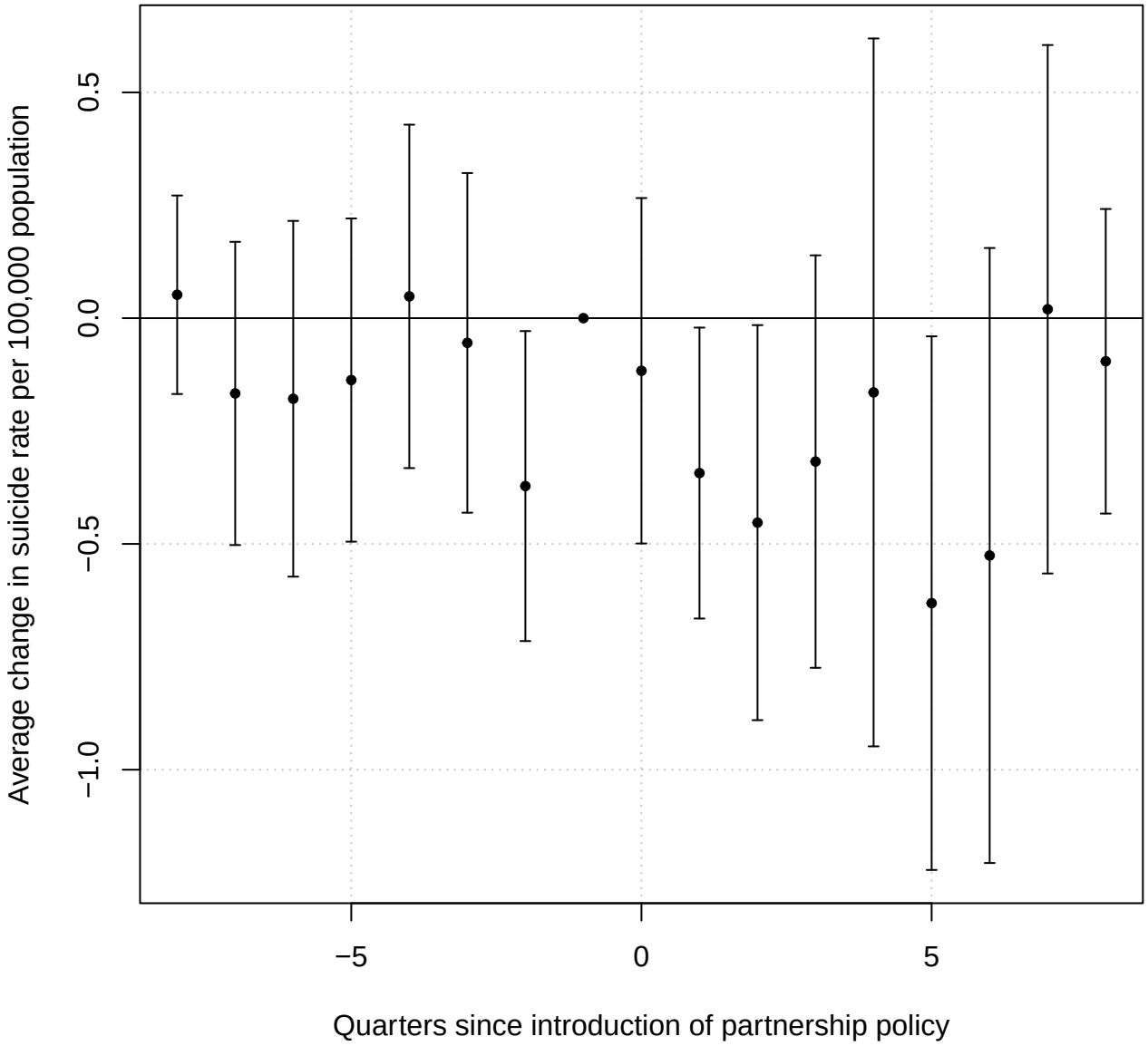
Notes: The table reports coefficients on the dummy that takes 1 if municipality m introduced the same-sex partnership policy, the dummy that takes 1 in 2022, average vote shares of non-LDP parties at municipality m in lower house elections (2009, 2012, 2014, 2017), and those interaction terms. All specifications include controls $\times$ after 2022 dummy. Controls are municipality-level socioeconomic factors in 2015, including population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. All regressions are weighted by the population in each municipality in 2015. Standard errors clustered at the municipality level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table 8: The effects on attitudes toward sexual minorities among cisgender heterosexuals

Dependent variable is:	Political leader		Colleague		Child's partner	
	(1)	(2)	(3)	(4)	(5)	(6)
Partnership$_m$ × After$_y$	-0.022	-0.057	-0.046	-0.112*	-0.057	-0.140***
	(0.034)	(0.041)	(0.043)	(0.060)	(0.050)	(0.048)
Partnership$_m$ × After$_y$	-0.292	0.265	-0.338	0.433	0.771*	1.261***
×Vote share of Non-LDP$_m$	(0.281)	(0.402)	(0.391)	(0.466)	(0.445)	(0.428)
R^2	0.006	0.010	0.007	0.011	0.008	0.015
Observations	2,968	2,968	2,962	2,962	2,963	2,963
Mean dep. var.	0.668	0.668	0.708	0.708	0.532	0.532
Bordered municipalities F.E.s		✓		✓		✓
Bordered municipalities-After$_y$ F.E.s		✓		✓		✓
Controls × After$_y$ F.E.s	✓	✓	✓	✓	✓	✓

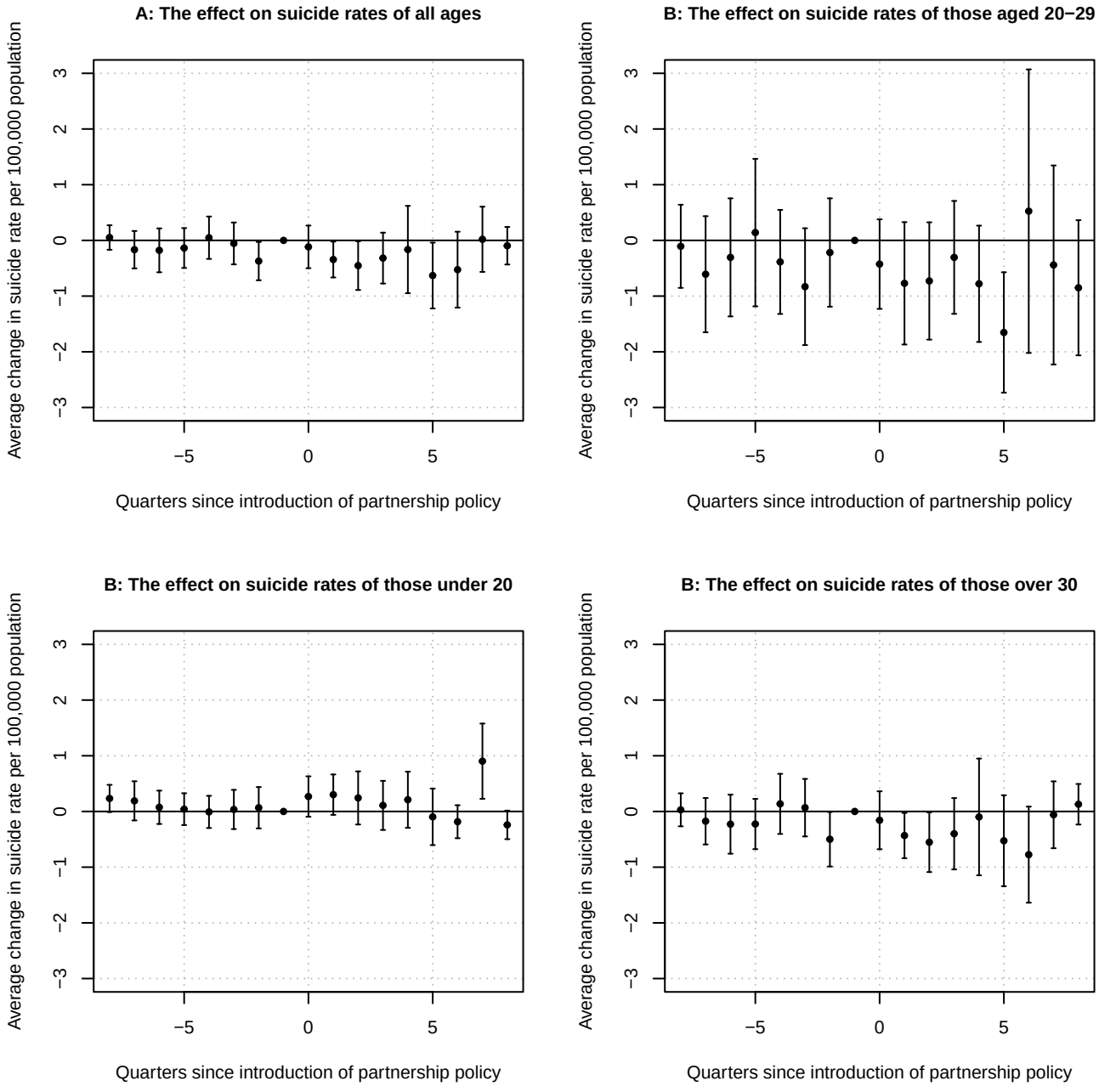
Notes: The table reports coefficients on the dummy that takes 1 if municipality m introduced the same-sex partnership policy, the dummy that takes 1 in 2022, average vote shares of non-LDP parties at municipality m in lower house elections (2009, 2012, 2014, 2017), and those interaction terms. The dependent variable is the dummy that takes 1 if their comfort level is equal to or greater than 5 and takes 0 otherwise (Categorized as "Comfortable" or "Moderately comfortable" in Eurobarometer). All specifications include controls × after 2022 dummy. Controls are municipality-level socioeconomic factors in 2015, including population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. All regressions are weighted by the population in each municipality in 2015. Standard errors clustered at the municipality level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Figure 1: Quarterly-level event study



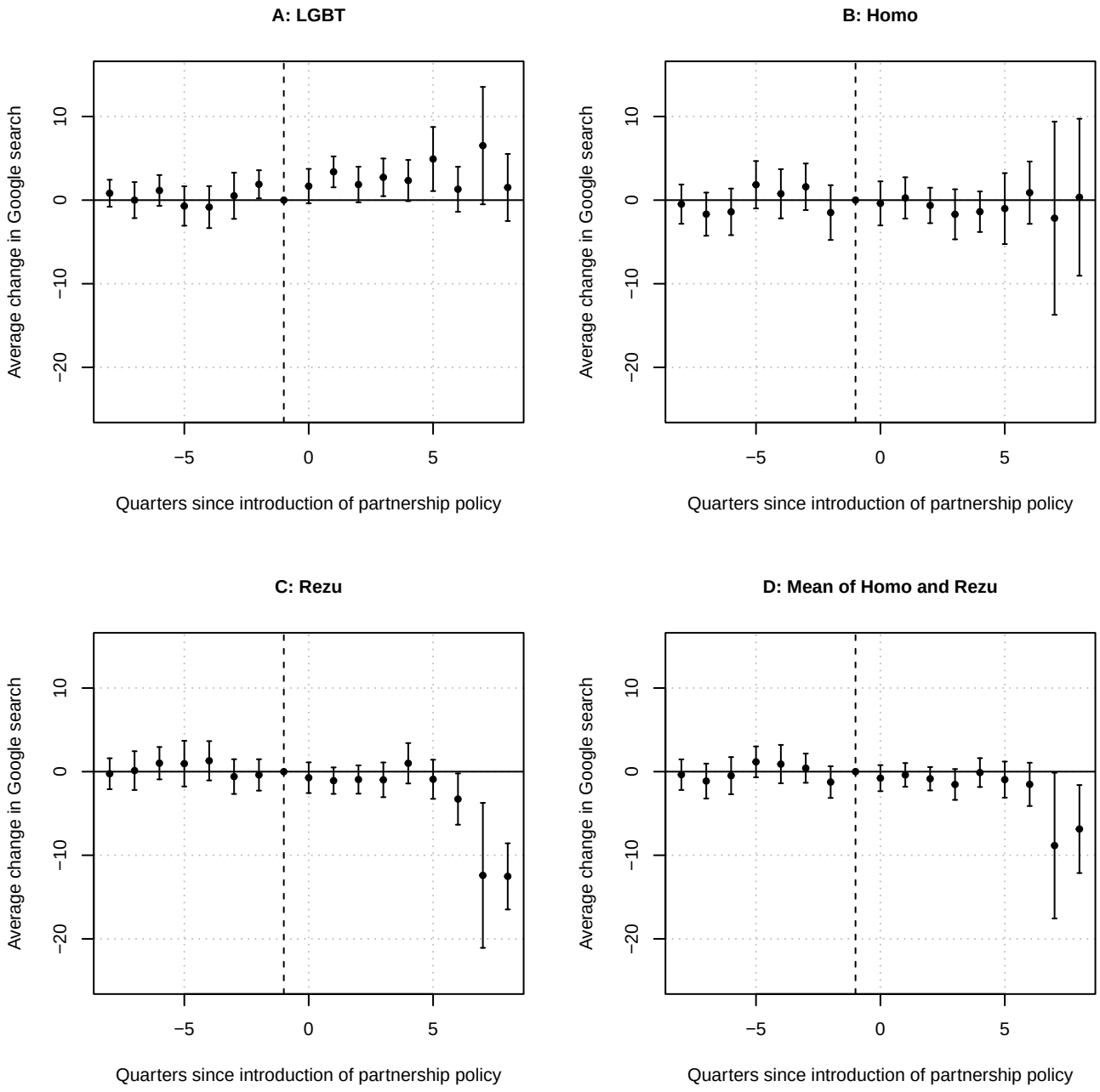
Notes: This figure shows event study estimates by equation (2) and the corresponding 95 percent confidence intervals using the cluster-robust standard error at the municipality level. $q = -1$ is a reference quarter. The dependent variable is the quarterly suicide rates per 100,000 population. The independent variables are dummies that take 1 if quarter q equals the quarter of the introduction plus or minus l. The estimation includes municipality fixed effects, quarter fixed effects, and controls $\times$ quarter fixed effects. Controls are the municipality-level socioeconomic factors in 2010 that contain the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. The regression is weighted by the population in each municipality in 2009.

Figure 2: Result of event study by age group



Notes: This figure shows event study estimates by age group and the corresponding 95 percent confidence intervals using the cluster-robust standard error at the municipality level. $q = -1$ is a reference quarter. The dependent variable is the age-level quarterly suicide rates per 100,000 population of each age group. The independent variables are dummies that take 1 if quarter q equals the quarter of the introduction plus or minus l. The estimation includes municipality fixed effects, quarter fixed effects, and controls $\times$ quarter fixed effects. Controls are the municipality-level socioeconomic factors in 2010 that contain the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. The regression is weighted by the population of each age group in each municipality in 2009.

Figure 3: Result of event study using Google search interests



Notes: This figure shows event study estimates by equation (6) and the corresponding 95 percent confidence intervals using the cluster-robust standard error at the prefecture level. $q = -1$ is a reference quarter. The dependent variables are the normalized Google search interest of each word. The independent variables are dummies that take 1 if quarter q equals the quarter of the introduction plus or minus l, and the interaction terms with average vote shares of non-LDP parties at municipality m in lower house elections (2009, 2012, 2014, 2017). This figure plots only the estimated coefficients on the interaction terms. The estimation includes prefecture fixed effects, quarter fixed effects, and controls × quarter fixed effects. Controls are the prefecture-level socioeconomic factors in 2010 that contain the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. The regression is weighted by the population in each prefecture in 2009.

9 Appendix

A.1 Violation of assumptions in two-way fixed effects model

The two-way fixed effects regression that I employed in Section 3.1 assumes parallel trends of the outcome before the treatment intervention and no anticipation effects. So far, the results of the event study suggest that there are no differential pre-trends and the assumptions hold. However, Goodman-Bacon (2021) has further pointed out that the obtained average treatment effects on the treated (ATT) by the two-way fixed effects regression is a weighted sum of the following three groups: (1) treatment group versus never-treated group, (2) early treated group as treated versus late treated group as control before the late treated group is treated, and (3) early treated group as control versus late treated group as treated after the late treated group is treated. Even worse, if the evolution of treatment effects varies across time, which seems likely to happen, the weights can be negative. From another point of view, the conventional two-way fixed effects estimation implicitly assumes the treatment effect homogeneity (Borusyak et al., 2021).

Similar issues also arise in the context of event-study regression. The coefficients of each lag and lead can be a weighted average of treatment effects and the weights can be negative (Sun and Abraham, 2020). This means that the conventional test for the parallel trends and no anticipation effects by the event-study regression may not be reliable.

For the solution, Sun and Abraham (2020) together with Callaway and Sant'Anna (2020) and De Chaisemartin and d'Haultfoeuille (2020) estimate cohort-specific average treatment effects on the treated, that is, the average treatment effect for the cohort of units treated at a particular time. While this estimation use data only from one time period before a treatment, Borusyak et al. (2021) exploit all pre-periods and thus are more efficient. They estimate individual treatment effects by using all untreated units based on the conventional parallel trends and no anticipation effects assumptions. Instead, they provide a way of testing those assumptions separating from the estimation of the dynamics of treatment effects to avoid the contaminations from treatment effects at other time periods. Therefore, in this section, I employ the alternative method proposed by Borusyak et al. (2021) and address those issues.

Following Borusyak et al. (2021), I first estimate the municipality fixed effect and quarter fixed effect including controls by using only untreated observations:

$$SuicideRate_{m,q}(0) = \gamma_m + \theta_q + \mathbf{Z}'_{m,2010}\boldsymbol{\theta}_q(+\lambda_{p,q}) + \epsilon_{m,q} \tag{8}$$

where subscript m, q, and p indicate municipality, quarter, and prefecture, respectively. The other variables are the same as those defined in section 3.1.

Then, I extrapolate it to impute the potential outcomes for treated observations and obtain the individual treatment effect:

$$\hat{\tau}_{m,q} = SuicideRate_{m,q} - \widehat{SuicideRate}_{m,q}(0) \tag{9}$$

where $SuicideRate_{m,q}$ is the observed outcome of the treated observations and $\widehat{SuicideRate}_{m,q}(0)$ is the estimated potential outcome for the treated observations obtained by using the estimated fixed effects in equation (8).

Finally, I estimate the ATT by calculating the weighted sum of the individual treatment effects. Since researchers can obtain the individual treatment effects directly, one does not need the treatment effect homogeneity assumption. However, this approach cannot distinguish the individual treatment effect $\tau_{m,q}$ and the residual $\epsilon_{m,q}$ because $\tau_{m,q}$ is defined as the difference between the outcome and the counterfactual. Thus, researchers need to calculate some weighted average of estimated treatment effects over a large group of observations and take the difference from the individual treatment effect as the residual. How choose the averages depends on a balance between avoiding overfitting by narrowing the group and overly inflating residuals by broadening the group. This analysis calculates the standard error using the weighted average of estimated treatment effects over cohort-by-two-quarters groups.

Table A4 reports the estimated ATT and the coefficient of $Partnership_{m,q}$ from the two-way fixed effect estimation in equation (1) for comparison. The results in columns (1) and (2) are the same as those in columns (1) and (2) in Table 3. In column (3), I add a municipality-specific linear time trend to the specification in column (2) to control for potential confounding factors linearly. The result in column (4) is from the alternative method, and in column (5), prefecture-quarter fixed effects are added. Similar to column

(3), a municipality-specific linear time trend is included in column (6). The magnitudes do not change so much from columns (1) to (4) and from (2) to (5), respectively.[36] These results suggest that the main estimation results in Table 3 suffer little from the potential identification problems of the two-way fixed effect regressions. The reason can be because the number of the never-treated municipalities is huge compared with that of the treated municipalities, and the setting becomes closer to an original 2-by-2 difference-in-differences.[37] Thus, the estimated ATTs in Table 3 are less likely to suffer from the issues that recent works pointed out. When including a municipality-specific linear time trend in column (3), the magnitude becomes smaller and loses significance. However, in column (6), the magnitude is very close to columns (4) and (5), though it is imprecisely estimated. This result suggests that the treatment effects still exist after controlling for the municipality-specific linear time trend by the alternative method. It could be attributed to the fact that estimating the municipality-specific linear time trend using both pre-and post-treatment data in the two-way fixed effect model contaminates the treatment effects with the estimates of the linear trend.

Figure A4 graphically shows the treatment effects of each quarter and the 95 percent confidence intervals. In Figure A4, since all of the pre-treatment coefficients except $q = -2$ are not statistically different from zero, the graphical evidence also supports that there are no clear differential pre-trends. After $q = 0$, some of the coefficients are negative and statistically different from zero, and they are in a downward trend overall. Therefore, the results are consistent with those obtained in Section 4.

A.2 Presence of outliers

This section applies a synthetic control method to each municipality to closely see the trend of suicide rates for the following reasons. First, only a few municipalities, such as those that have introduced the same-sex partnership policy as a municipal ordinance, could drive the baseline results. Second, the synthetic control method can provide an unbiased estimator

[36] The number of observations dropped in columns (5) and (6) because all of the municipalities in Ibaraki Prefecture are dropped after they get treatment in order to estimate the prefecture-quarter fixed effects.

[37] The number of the never-treated municipalities is 1667, whereas that of the treated municipalities is 74, where I count each of the municipalities in Ibaraki Prefecture as treated.

even when unobserved confounders can vary with time, in contrast to the difference-in-differences (two-way fixed effects) model that assumes to be constant across time, including individual and time fixed effects only separately (Abadie, Diamond, and Hainmueller, 2010). Thus, this analysis complements the difference-in-differences estimation and event study analysis that provide evidence on the average treatment effects of introducing the partnership policy.

Following Abadie et al. (2010), the synthetic control estimator is defined as follows:

$$Y_{i,t}^{I} = Y_{i,t}^{N} + \alpha_{i,t} D_{i,t}, \tag{10}$$

where i and t refer to unit and time, respectively. $D_{i,t}$ is an indicator variable that takes 1 if unit i got treated in t and takes 0 otherwise. $Y_{i,t}^{I}$ is the observed outcome for unit i and time t if the unit i got treated. $Y_{i,t}^{N}$ is the counterfactual outcome for unit i and time t in the absence of the treatment. If unit $i = 1$ got treated, what one would like to estimate is $\alpha_{1,t} = Y_{1,t}^{I} - Y_{1,t}^{N}$ for the post-treatment period. The synthetic control method then aims to estimate unobserved $Y_{1,t}^{N}$ by a particular weighted average of controls, supposing $Y_{1,t}^{N}$ is generated as follows:

$$Y_{it}^{N} = \delta_t + \boldsymbol{\theta_t} \boldsymbol{Z_i} + \boldsymbol{\lambda_t} \boldsymbol{\mu_i} + \epsilon_{it}, \tag{11}$$

where $\boldsymbol{Z_i}$ is a vector of observed covariates (not affected by the treatment), and $\boldsymbol{\theta_t}$ is a vector of the associated unknown parameters. With weights such that $w_j \geq 0$ for $j = 2, ..., J+1$ (potential controls) and $w_2 + ... + w_{J+1} = 1$, the weighted average of the outcome is

$$\sum_{j=2}^{J+1} Y_{jt} = \delta_t + \boldsymbol{\theta_t} \sum_{j=2}^{J+1} w_j \boldsymbol{Z_j} + \boldsymbol{\lambda_t} \sum_{j=2}^{J+1} w_j \boldsymbol{\mu_j} + \sum_{j=2}^{J+1} w_j \epsilon_{jt}. \tag{12}$$

The weights are constructed such that

$$\begin{aligned}
&\sum_{j=2}^{J+1} w_j Y_{j1} = Y_{11}, \\
&\sum_{j=2}^{J+1} w_j Y_{j2} = Y_{12}, \\
&\sum_{j=2}^{J+1} w_j Y_{jT_0} = Y_{1T_0} (T_0 \text{ is the last time in the pre-treatment period}), \\
&\sum_{j=2}^{J+1} w_j \boldsymbol{Z_j} = \boldsymbol{Z_1}.
\end{aligned} \tag{13}$$

Abadie et al. (2010) show that $\sum_{j=2}^{J+1} Y_{jt}$ would be equivalent to Y_{it}^N under certain conditions.

I apply the synthetic control methods for 9 municipalities with at least a one-year post-treatment period using municipality-quarter level data same as the main analyses. The matched outcome is quarterly suicide rates per 100,000 population. Time-varying predictors are age-level suicide rates per 100,000 population of each age group (under 20, 20s, 30s, 40s, 50s, 60 or above). Then I create matching based on the outcome and the predictors in the prior 8 quarters to the policy introduction.[38]

Figure A5 shows the outcome trajectories of treatment municipalities and the corresponding synthetic controls for 4 quarters after the policy introduction. The left graph in each panel shows the trend of the treatment municipality (solid red line), the synthetic controls (dotted black line), and all cases (dash-dotted green line). The right graph in each panel shows the difference between the treatment and controls. In all cases, the synthetic controls produce an exact match for the treatment, which implies that synthetic control groups for respective municipalities are reasonably comparable in the pre-treatment period. Overall, 6 of the 9 municipalities see a decrease in the quarterly suicide rates per 100,000 population at 4 quarters after the policy introduction relative to their synthetic control groups. Thus, this result can also support the validity of the baseline results.

[38] I use the microsynth package in R for estimation (Robbins and Davenport, 2021).

A.3 Confounding effects from other policies

Another concern is that the municipalities with the same-sex partnership policy may introduce other policies which have been introduced at the same time as the same-sex partnership policy and have reduced suicide rates as well. Since the graphical results of quarterly-level event study analysis in Figures 1 and A4 show that suicide rates decrease in response to the introduction of the same-sex partnership policy, if the above concern is the case, the municipalities with the same-sex partnership policy would adopt such policies in the same relative quarter. Still, it is possible that municipalities tend to implement many policies as a policy package.

In order to address this concern, I perform the three analyses that can provide evidence against the existence of those policies. First, I estimate effects of the introduction of the same-sex partnership policy on the net population inflow into the municipalities. If the municipalities with the same-sex partnership policy had introduced the other diversity-oriented policies or redistribution policies, the municipalities would have become more attractive and the net population inflow would have increased at the same time as the introduction of the same-sex partnership policy.

I obtain data about migration between municipalities from Population, Demographics, and Number of Households Based on the Basic Resident Register compiled by the Ministry of Internal Affairs and Communications. I use the annual-level data from 2012 to 2019 because the monthly-level data are not available. The estimated equation is as follows:

$$Inflow_{m,y} = \beta_1 Partnership_{m,y} + \gamma_m + \theta_y + \mathbf{Z}_m^{'}\boldsymbol{\theta}_y(+\lambda_{p,y}) + \epsilon_{m,y}, \tag{14}$$

where subscript m and y indicate municipality and year, respectively. $Inflow_{m,y}$ is calculated by dividing the net population inflow to the municipality m in year y by the 100 population in municipality m in 2009. The other variables are the annual version of equation (1).

Table A5 shows the result. In even columns, the prefecture-quarter fixed effect is included. The first two columns document the effects on the net inflow of people of all ages, and the last two columns document those on the net inflow of people in their twenties. In all columns, the effects are not statistically different from 0. Thus, these results suggest that

the municipalities with the partnership policy did not become more attractive in general at the same time as introducing the policy.

Second, I add additional initial characteristics interacted with quarter fixed effects to the quarterly-level estimation in section 3.1 to control for potential differential trends based on the initial characteristics of municipalities. These characteristics may relate to types of policies which could be adopted in the municipalities with the same-sex partnership policy and have effects on suicide rates. In this analysis, I use the following three variables. One is vote shares of the Liberal Democratic Party (LDP) at the 2014 lower house election, which can indicate regional political preferences because the LDP is a conservative party and an only major party which express opposition to the same-sex marriage or partnership policy.[39] I also construct an indicator variable which takes 1 if the municipality is a member of the National Council of Japan Nuclear Free Local Authorities (Nucfree), which can capture a regional political ideology because being a member of the National Council of Japan Nuclear Free Local Authorities can be considered as a left-wing region.[40] Finally, I include suicide rates in 2009, which can capture an initial condition of suicide rates in municipalities. It can potentially affect the adoption of suicide prevention measures.

The results are presented in Table A6. Column (1) documents the baseline result estimated in Table 3. In column (2), the LDP vote shares $\times$ quarter fixed effects are added. In column (3), indicators of a member of the Nucfree $\times$ quarter fixed effects are added. In column (4), suicide rates in 2009 $\times$ quarter fixed effects are added. In column (5), all of the three variables are included. When including suicide rates in 2009, I limit the estimation periods to 2012-2019 to avoid endogeneity bias. Overall, the results do not change from the baseline, which provides suggestive evidence against the importance of other policies.

Third, I perform placebo analyses to see the effects on various outcomes that are not supposed to be affected by the adoption of the same-sex partnership policy. For this purpose, I use the annual death rates by top five death causes (cancer, heart diseases other than hypertensive diseases, senility, cerebrovascular diseases, and pneumonia) according to the

[39]The data about the number of votes for each candidate in each municipality is obtained from the Survey on Results of House-of-Representatives General Election and Supreme Court Review.

[40]The list of municipalities is obtained from National Council of Japan Nuclear Free Local Authorities (2020).

Ministry of Health Labour and Welfare as outcome variables (Ministry of Health, Labour and Welfare, nd). In the estimation, the death rates are calculated by dividing the annual number of deaths of each cause in the municipality by the 100,000 population in the municipality in 2009.[41] The estimated equation is the annual version of equation (1). Table A7 reported the results. Column (1) documents the death rates by all death causes other than suicide. None of the death rates by other causes significantly correlate with the introduction of the partnership policy. These results suggest that other concurrent policies are not likely to explain the baseline results on suicide rates.

Finally, I estimate the effects on the municipality-level expenditure on social welfare, sanitation, and labor administration.[42] Social welfare expenditure includes the public spending on operating welfare facilities for citizens and livelihood relief. Sanitation expenditure includes the public spending on policies related to public health or the living environment. Labor administration expenditure includes the public spending on unemployment relief measures. In the estimation, I use the percentage share of each expenditure by dividing that number by the total amount of spending in the fiscal year. The estimated equation is the annual version of equation (1). The results are presented in Table A8. While the social welfare expenditure is negatively correlated with the adoption of the same-sex partnership policy, the share of the expenditure on each purpose does not increase in response to the adoption of the same-sex partnership policy. These placebo analyses also suggest that the municipalities with the same-sex partnership policy would not introduce some policies that can also affect suicide rates at the same time.

A.4 Unbalance between treatments and controls

This section addresses the potential concern that the baseline regression may compare apples to oranges for the following reasons. First, Table 2 shows that treatment municipalities tend to have higher population density, the population share of those aged 15 to 64, and share of employment in tertiary industry, which implies that treatments are concentrated

[41]The data source is the Vital Statistics Survey from 2009 to 2019 compiled by the Ministry of Health Labour and Welfare.

[42]The data source is the Survey on Local Public Finance from 2009 to 2019 compiled by the Ministry of Internal Affairs and Communications.

in urban areas. In addition, since the number of treatments is tiny compared to controls, municipalities in rural areas can dominate the control group. Therefore, the control group in the baseline regression may not produce suitable counterfactuals, though the baseline regression controls for differential trends based on the above factors. Second, one treatment has introduced the partnership policy at the prefecture level (a larger administrative and geographical unit than the other treatments), which consists of 44 municipalities (over half of the total treatment municipalities in the baseline analysis). The prefecture-level treatment could have different effects than other units and influence the overall results because people feel distant from prefecture governments compared to municipal governments.[43]

To alleviate the concern empirically, I perform three analyses. The first analysis employs the same difference-in-differences design as equation (1) using only data from cities, excluding towns and villages. Table A9 reports the result. Although the magnitudes tend to be a little smaller than Table 3, the estimated coefficients remain negative and marginally significant. The second analysis also uses the same difference-in-differences design as equation (1) by excluding the prefecture-level treatment (Ibaraki prefecture). Table A10 shows the result. The magnitudes are close to those in Table 3 and the estimated coefficients are statistically significant. These two results suggest that the baseline result is robust to the selection of controls and treatments.

The third analysis employs a matching method for panel data proposed by Imai et al. (2019), which can produce counterfactual outcomes by the weighted average of controls. For example, if a researcher chooses inverse probability weighting, it performs weighted difference-in-differences using all data. On the other hand, suppose a researcher uses matching methods such as Mahalanobis matching or propensity score matching. In that case, it produces counterfactual outcomes by averaging the outcomes of matched controls (the assigned weights are all equal to 1). Specifically, the ATT estimator is described as follows:

[43]The expressive function theory predicts that the expressive power can be more substantial if the law is enacted at the local level because it can be assumed to more precisely reflect the local norm about which people care most.

$$\hat{\delta}(F,L) = \frac{1}{\sum_{i=1}^{N}\sum_{t=L+1}^{T-F}} \sum_{i=1}^{N}\sum_{t=L+1}^{T-F} D_{it} \Big\{ (Y_{i,t+F} - Y_{i,t-1}) - \sum_{i' \in \mathcal{M}_{it}} w_{it}^{i'} (Y_{i',t+F} - Y_{i',t-1}) \Big\}, \tag{15}$$

where i and t refers to units and time, respectively. $w_{it}^{i'}$ is a weight such that $w_{it}^{i'} \geq 0$ and $\sum_{i' \in \mathcal{M}_{it}} w_{it}^{i'} = 1$. $\mathcal{M}_{it}$ means a matched set for unit i and time t, and X_{it} indicates treatment status of unit i at time t. D_{it} is defined as $D_{it} = X_{it}(1 - X_{i,t-1}) \times \mathbf{1}\{\mid \mathcal{M}_{it} \mid > 0\}$. Thus, D_{it} takes 1 only if unit i at time t changes the treatment status from t-1 and has at least one matched control unit.

F and L represent lead and lag, respectively. Although researchers can select the F based on their interests, L is related to an identification assumption because units with different past treatment statuses could have different trends of an outcome if the treatment status could switch on and off. Nevertheless, since this research uses the case of staggered adoptions, the selection of L does not cause serious issues. Therefore, I select $F = 4$ (quarter) to see the trend of the treatment effects and $L = -1$ (quarter). The covariates used for matching are the same as all of the previous regressions.

Figure A6 shows the estimated results. Panels (A), (B), and (C) plot the estimated coefficients when using Mahalanobis matching up to 10 matches, when using propensity score matching up to 10 matches, and when using inverse probability weighting, respectively. Although the confidence intervals are wide and across 0 in Panel (C), all of the coefficients are negative and tend to fall between 0 and -1, similar to the event study result in Figure 1. This result can untangle the concern about unbalance between treatments and controls and the small number of treatments in the baseline regression.

Table A1: The effects on annual suicide rates

Dependent variable is:	Annual suicide rates per 100,000 population			
	(1)	(2)	(3)	(4)
$\text{Partnership}_{m,y}$	-1.041**	-0.774**	-1.147***	-0.960**
	(0.441)	(0.380)	(0.399)	(0.387)
$\text{SuicideRate}_{m,y-1}$			-0.043***	-0.061***
			(0.009)	(0.008)
R^2	0.479	0.502	0.460	0.482
Observations	19,151	19,151	17,410	17,410
Mean dep. var.	20.604	20.604	20.604	20.604
Municipality F.E.s	✓	✓	✓	✓
Year F.E.s	✓	✓	✓	✓
Prefecture-Year F.E.s		✓		✓
Controls × Year F.E.s	✓	✓	✓	✓

Notes: The table reports coefficients on the dummy that takes 1 after the introduction of the same-sex partnership policy at municipality m in year y, and one-year lagged annual suicide rates. All specifications include municipality fixed effects, year fixed effects, and controls × year fixed effects. Controls are municipality-level socioeconomic factors in 2010 that contain the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. All regressions are weighted by the population in each municipality in 2009. Standard errors clustered at the municipality level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A2: The effects on monthly suicide rates

Dependent variable is:	Monthly suicide rates per 100,000 population			
	(1)	(2)	(3)	(4)
$\text{Partnership}_{m,t}$	-0.098**	-0.084**	-0.100**	-0.086**
	(0.047)	(0.037)	(0.046)	(0.038)
$\text{SuicideRate}_{m,t-1}$			-0.002	-0.003
			(0.002)	(0.002)
R^2	0.079	0.104	0.078	0.103
Observations	229,812	229,812	228,071	228,071
Mean dep. var.	1.717	1.717	1.717	1.717
Municipality F.E.s	✓	✓	✓	✓
Month F.E.s	✓	✓	✓	✓
Prefecture-Month F.E.s		✓		✓
Controls × Month F.E.s	✓	✓	✓	✓

Notes: The table reports coefficients on the dummy that takes 1 after the introduction of the same-sex partnership policy at municipality m in month t, and one-month lagged monthly suicide rates. All specifications include municipality fixed effects, month fixed effects, and controls × month fixed effects. Controls are municipality-level socioeconomic factors in 2010 that contain the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. All regressions are weighted by the population in each municipality in 2009. Standard errors clustered at the municipality level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A3: The effects on quarterly suicide rates adding controls in 2015

Dependent variable is:	Quarterly suicide rates per 100,000 population			
	(1)	(2)	(3)	(4)
$\text{Partnership}_{m,q}$	-0.241**	-0.199**	-0.254**	-0.218**
	(0.120)	(0.099)	(0.118)	(0.101)
$\text{SuicideRate}_{m,q-1}$			-0.010**	-0.014***
			(0.004)	(0.004)
R^2	0.193	0.216	0.190	0.213
Observations	75,812	75,812	74,089	74,089
Mean dep. var.	5.151	5.151	5.151	5.151
Municipality F.E.s	✓	✓	✓	✓
Quarter F.E.s	✓	✓	✓	✓
Prefecture-Quarter F.E.s		✓		✓
Controls × Quarter F.E.s	✓	✓	✓	✓

Notes: The table reports coefficients on the dummy that takes 1 after the introduction of the same-sex partnership policy at municipality m in quarter q, and one-quarter lagged quarterly suicide rates. All specifications include municipality fixed effects, quarter fixed effects, controls in 2010 × quarter fixed effects from 2009 to 2014, and controls in 2015 × quarter fixed effects from 2015 to 2019. Controls are municipality-level socioeconomic factors that contain the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. All regressions are weighted by the population in each municipality in 2009. Standard errors clustered at the municipality level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A4: The effects on quarterly suicide rates: Alternative specifications

Dependent variable is:	Quarterly suicide rates per 100,000 population					
	Two-way fixed effects model			Borusyak et al. (2021)		
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	-0.272**	-0.222**	-0.169	-0.278**	-0.259*	-0.251
	(0.139)	(0.110)	(0.119)	(0.134)	(0.133)	(0.166)
Observations	76,604	76,604	76,604	76604	76516	76516
Mean dep. var.	5.151	5.151	5.151	5.151	5.153	5.153
Municipality F.E.s	✓	✓	✓	✓	✓	✓
Quarter F.E.s	✓	✓	✓	✓	✓	✓
Controls × Quarter F.E.s	✓	✓	✓	✓	✓	✓
Prefecture-Quarter F.E.s		✓	✓		✓	✓
Municipality-specific linear trend			✓			✓

Notes: The table reports the average treatment effects on treated of the introduction of the same-sex partnership policy. The results in columns (1) and (2) are the same as those in columns (1) and (2) of Table 3. Column (3) includes municipality-specific linear trends. Columns (4) to (6) report the results from a method proposed by Borusyak et al. (2021). Appendix A.1 explains the estimation procedure. All specifications include municipality fixed effects, quarter fixed effects, and controls × quarter fixed effects. Controls are municipality-level socioeconomic factors in 2010 that contain the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. All regressions are weighted by the population in each municipality in 2009. Standard errors clustered at the municipality level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A5: The effects on annual net population inflow

Dependent variable is:	Net population inflow per 100 population			
	All ages		Twenties	
	(1)	(2)	(3)	(4)
Partnership$_{m,y}$	0.041	0.024	0.235	0.174
	(0.037)	(0.042)	(0.166)	(0.194)
R^2	0.734	0.745	0.851	0.856
Observations	13,920	13,920	13,920	13,920
Mean dep. var.	-0.184	-0.184	-1.081	-1.081
Municipality F.E.s	✓	✓	✓	✓
Year F.E.s	✓	✓	✓	✓
Prefecture-Year F.E.s		✓		✓
Controls × Year F.E.s	✓	✓	✓	✓

Notes: This table reports coefficients on the dummy that takes 1 after the introduction of the same-sex partnership policy at municipality m in year y. All specifications include municipality fixed effects, year fixed effects, and controls × year fixed effects. Controls are municipality-level socioeconomic factors in 2010 that contain the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. All regressions are weighted by the population in each municipality in 2009. Standard errors clustered at the municipality level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A6: The effects on quarterly suicide rates including additional controls

Dependent variable is:	Quarterly suicide rates per 100,000 population				
	(1)	(2)	(3)	(4)	(5)
$\text{Partnership}_{m,q}$	-0.272**	-0.252**	-0.273**	-0.241**	-0.226**
	(0.139)	(0.128)	(0.139)	(0.105)	(0.102)
R^2	0.192	0.193	0.193	0.148	0.149
Observations	76,604	76,604	76,604	55,712	55,712
Mean dep. var.	5.151	5.151	5.151	4.605	4.605
Municipality F.E.s	✓	✓	✓	✓	✓
Quarter F.E.s	✓	✓	✓	✓	✓
Controls × Quarter F.E.s	✓	✓	✓	✓	✓
LDP vote share in 2009 × Quarter F.E.s		✓			✓
Member of Nucfree × Quarter F.E.s			✓		✓
Suicide rates in 2009 × Quarter F.E.s				✓	✓

Notes: The table reports coefficients on the dummy that takes 1 after the introduction of the same-sex partnership policy at municipality m in quarter q. All specifications include municipality fixed effects, quarter fixed effects, and controls × quarter fixed effects. Controls are municipality-level socioeconomic factors in 2010 that contain the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in primary industry, and the share of employment in tertiary industry. All regressions are weighted by the population in each municipality in 2009. Standard errors clustered at the municipality level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A7: The effects on annual death rates from other death causes

Dependent variable is:	Death rates per 100,000 population					
	All death causes except suicide	Canser	Heart diseases	Senility	Cerebrovascular diseases	Pneumonia
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Partnership}_{m,y}$	6.845	0.851	1.067	0.259	0.152	1.434
	(6.155)	(2.168)	(1.620)	(1.652)	(0.972)	(1.527)
R^2	0.894	0.826	0.847	0.855	0.830	0.819
Observations	19,151	19,151	19,151	19,151	19,151	19,151
Mean dep. var.	-0.184	-1.081	-0.184	-1.081	-0.184	-1.081
Municipality F.E.s	✓	✓	✓	✓	✓	✓
Year F.E.s	✓	✓	✓	✓	✓	✓
Controls × Year F.E.s	✓	✓	✓	✓	✓	✓

Notes: This table reports coefficients on the dummy that takes 1 after the introduction of the same-sex partnership policy at municipality m in year y. All specifications include municipality fixed effects, year fixed effects, and controls × year fixed effects. Controls are municipality-level socioeconomic factors in 2010 that contain the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. All regressions are weighted by the population in each municipality in 2009. Standard errors clustered at the municipality level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A8: The effects on the municipality-level expenditure

Dependent variable is:	Share of each expenditure in a total amount of spending (%)		
	Social welfare	Sanitation	Labor administration
	(1)	(2)	(3)
Partnership$_{m,y}$	-0.598***	0.064	0.007
	(0.190)	(0.097)	(0.009)
R^2	0.916	0.659	0.823
Observations	19,151	19,151	19,151
Mean dep. var.	13.994	4.417	0.172
Municipality F.E.s	✓	✓	✓
Year F.E.s	✓	✓	✓
Controls × Year F.E.s	✓	✓	✓

Notes: The table reports coefficients on the dummy that takes 1 after the introduction of the same-sex partnership policy at municipality m in year y. All specifications include municipality fixed effects, year fixed effects, controls × year fixed effects. Controls are municipality-level socioeconomic factors in 2010 that contain the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. All regressions are weighted by the population in each municipality in 2009. Standard errors clustered at the municipality level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A9: The effects on quarterly suicide rates excluding towns and villages

Dependent variable is:	Quarterly suicide rates per 100,000 population			
	(1)	(2)	(3)	(4)
$\text{Partnership}_{m,q}$	-0.267*	-0.196*	-0.278**	-0.216*
	(0.139)	(0.116)	(0.135)	(0.117)
$\text{SuicideRate}_{m,q-1}$			0.000	-0.008
			(0.007)	(0.006)
R^2	0.296	0.341	0.291	0.336
Observations	35,772	35,772	34,959	34,959
Mean dep. var.	4.943	4.943	4.943	4.943
Municipality F.E.s	✓	✓	✓	✓
Quarter F.E.s	✓	✓	✓	✓
Prefecture-Quarter F.E.s		✓		✓
Controls × Quarter F.E.s	✓	✓	✓	✓

Notes: The table reports coefficients on the dummy that takes 1 after the introduction of the same-sex partnership policy at municipality m in quarter q, and one-quarter lagged quarterly suicide rates, excluding towns and villages from the data. All specifications include municipality fixed effects, quarter fixed effects, and controls × quarter fixed effects. Controls are municipality-level socioeconomic factors in 2010 that contain the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. All regressions are weighted by the population in each municipality in 2009. Standard errors clustered at the municipality level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A10: The effects on quarterly suicide rates excluding prefecture-level treatments

Dependent variable is:	Quarterly suicide rates per 100,000 population			
	(1)	(2)	(3)	(4)
$\text{Partnership}_{m,q}$	-0.280*	-0.226**	-0.294**	-0.244**
	(0.150)	(0.110)	(0.146)	(0.113)
$\text{SuicideRate}_{m,q-1}$			-0.009**	-0.013***
			(0.004)	(0.004)
R^2	0.193	0.217	0.190	0.214
Observations	74,668	74,668	72,971	72,971
Mean dep. var.	5.157	5.157	5.157	5.157
Municipality F.E.s	✓	✓	✓	✓
Quarter F.E.s	✓	✓	✓	✓
Prefecture-Quarter F.E.s		✓		✓
Controls × Quarter F.E.s	✓	✓	✓	✓

Notes: The table reports coefficients on the dummy that takes 1 after the introduction of the same-sex partnership policy at municipality m in quarter q, and one-quarter lagged quarterly suicide rates, excluding a prefecture-level treatment (Ibaraki prefecture) from the data. All specifications include municipality fixed effects, quarter fixed effects, and controls × quarter fixed effects. Controls are municipality-level socioeconomic factors in 2010 that contain the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. All regressions are weighted by the population in each municipality in 2009. Standard errors clustered at the municipality level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A11: The effects on the Google search interest: Alternative specifications

Dependent variable is:	Normalized quarterly average of search interest of							
	LGBT		Homo		Rezu		Mean of Homo and Rezu	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population Share$_{p,q}$	0.074	0.069	-0.363	-0.321	0.096	0.120	-0.180	-0.134
	(0.099)	(0.100)	(0.381)	(0.345)	(0.172)	(0.135)	(0.288)	(0.244)
Population Share$_{p,q}$	5.009***	5.023***	-1.959	-2.038	-4.769*	-4.105*	-3.657	-3.345
×Vote share of Non-LDP$_p$	(0.663)	(0.651)	(3.205)	(2.841)	(2.755)	(2.215)	(2.428)	(2.112)
Search$_{p,q-1}$		0.009		0.115***		0.173***		0.143***
		(0.027)		(0.037)		(0.033)		(0.044)
R^2	0.935	0.935	0.761	0.766	0.859	0.862	0.871	0.873
Observations	2,068	2,021	2,068	2,021	2,068	2,021	2,068	2,021
Prefecture F.E.s	✓	✓	✓	✓	✓	✓	✓	✓
Quarter F.E.s	✓	✓	✓	✓	✓	✓	✓	✓
Controls × Quarter F.E.s	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The table reports coefficients on the population share of the municipalities in prefecture p that have introduced the same-sex partnership policy in quarter q, the interaction term with average vote shares of non-LDP parties at prefecture p in lower house elections (2009, 2012, 2014, 2017), and one-quarter lagged search interest of each word. All specifications include prefecture fixed effects, quarter fixed effects, and controls × quarter fixed effects. Controls are prefecture-level socioeconomic factors in 2010 which contains the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. All regressions are weighted by the population in each prefecture in 2009. Standard errors clustered at the prefecture level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A12: Municipality-level characteristics in 2015

	Control	Treatment	All	Difference
Population	109944.609	122588.389	112112.114	12643.780
	[204050.445]	[134885.182]	[193829.554]	(22281.232)
Population density	2429.252	2165.320	2384.006	-263.931
	[4211.075]	[2678.185]	[3987.994]	(448.074)
Population share under 15	1253.264	1272.478	1256.558	19.213
	[220.491]	[174.550]	[213.192]	(27.395)
Population share of those aged 15 to 64	5903.454	5972.833	5915.347	69.379
	[481.094]	[350.954]	[461.653]	(56.282)
Population share over 64	2843.282	2754.690	2828.095	-88.592
	[602.627]	[456.211]	[580.477]	(72.427)
Share of employment in primary industry	642.797	594.586	634.532	-48.210
	[805.948]	[810.747]	[805.683]	(121.083)
Share of employment in secondary industry	2635.219	2621.726	2632.906	-13.493
	[792.594]	[696.299]	[775.906]	(106.702)
Share of employment in tertiary industry	6721.984	6783.687	6732.562	61.703
	[1087.364]	[908.882]	[1057.828]	(140.811)
Sex ratio	51.266	51.346	51.280	0.080
	[1.439]	[1.339]	[1.420]	(0.203)
Share of foreign residents	1.240	1.056	1.209	-0.184
	[1.450]	[0.740]	[1.356]	(0.135)
Observations	261	54	315	315

Notes: This table reports the summary statistics of municipalities as follows: Column (1): mean and standard deviation for the municipalities that have not introduced the same-sex partnership policy by March 1, 2022 included in the first and second surveys (control group); Column (2): mean and standard deviation for the municipalities that have introduced the same-sex partnership policy between March 2, 2021, and March 1, 2022 included in the first and second surveys (treatment group); Column (3): mean and standard deviation for the all; Column (4): the difference between the treatment and the control groups and the standard error. Standard deviations and standard errors are presented in square brackets and parentheses. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A13: Summary statistics: Sexual minorities

	Control	Treatment	All	Difference
Age	43.520	43.766	43.587	0.246
	[11.892]	[11.975]	[11.913]	(0.470)
# of household	2.462	2.524	2.479	0.062
	[1.370]	[1.358]	[1.367]	(0.068)
Educational background				
Compulsory education	0.047	0.062	0.051	0.015
	[0.211]	[0.240]	[0.220]	(0.010)
High school	0.298	0.352	0.313	0.054**
	[0.458]	[0.478]	[0.464]	(0.025)
Junior coullege/Higher vocational school	0.128	0.130	0.128	0.003
	[0.334]	[0.337]	[0.335]	(0.014)
University	0.412	0.348	0.395	-0.064**
	[0.492]	[0.477]	[0.489]	(0.027)
Master course	0.062	0.044	0.057	-0.018*
	[0.241]	[0.206]	[0.232]	(0.009)
Doctoral course	0.031	0.036	0.033	0.004
	[0.175]	[0.186]	[0.178]	(0.008)
Other degree	0.022	0.028	0.024	0.007
	[0.146]	[0.166]	[0.152]	(0.006)
None	0.083	0.111	0.091	0.027**
	[0.276]	[0.314]	[0.287]	(0.013)
Household income				
(Unit: 1,000 Yen)				
-100	0.077	0.086	0.080	0.009
	[0.267]	[0.281]	[0.271]	(0.012)
100-199	0.097	0.097	0.097	-0.000
	[0.296]	[0.296]	[0.296]	(0.011)
200-399	0.215	0.230	0.219	0.015
	[0.411]	[0.421]	[0.414]	(0.017)
400-599	0.188	0.189	0.188	0.002
	[0.390]	[0.392]	[0.391]	(0.015)
600-799	0.139	0.121	0.134	-0.019
	[0.346]	[0.326]	[0.341]	(0.013)
800-999	0.087	0.071	0.083	-0.016
	[0.282]	[0.258]	[0.276]	(0.012)
1,000-1,199	0.044	0.042	0.043	-0.002
	[0.204]	[0.200]	[0.203]	(0.008)
1,200-1,399	0.021	0.020	0.021	-0.002
	[0.144]	[0.139]	[0.143]	(0.007)
1,400-	0.049	0.033	0.044	-0.015*
	[0.215]	[0.179]	[0.206]	(0.009)
Occupation				
Employee	0.294	0.274	0.289	-0.020
	[0.456]	[0.446]	[0.453]	(0.024)
Managerial occupation	0.075	0.049	0.068	-0.026***
	[0.263]	[0.216]	[0.252]	(0.009)
Top management	0.032	0.015	0.027	-0.017**
	[0.176]	[0.121]	[0.163]	(0.006)
Government, education	0.050	0.054	0.051	0.005
	[0.217]	[0.226]	[0.220]	(0.010)
Contract employee	0.069	0.069	0.069	-0.001
	[0.254]	[0.253]	[0.254]	(0.011)
Self employed	0.039	0.037	0.039	-0.002
	[0.194]	[0.189]	[0.193]	(0.008)
SOHO	0.015	0.023	0.017	0.008
	[0.123]	[0.151]	[0.131]	(0.006)
Agriculture, fishery, and forestry	0.003	0.005	0.004	0.002
	[0.057]	[0.070]	[0.061]	(0.003)
Professional	0.033	0.032	0.033	-0.001
	[0.178]	[0.176]	[0.178]	(0.009)
Part-time worker	0.139	0.165	0.146	0.026
	[0.346]	[0.371]	[0.353]	(0.019)
Homemaker	0.077	0.070	0.075	-0.007
	[0.267]	[0.255]	[0.264]	(0.013)
Student	0.028	0.023	0.027	-0.004
	[0.164]	[0.151]	[0.161]	(0.007)
Unemployed	0.120	0.141	0.126	0.022
	[0.325]	[0.349]	[0.332]	(0.016)
Other	0.025	0.042	0.030	0.017*
	[0.156]	[0.200]	[0.170]	(0.009)
Observations	2160	813	2973	2973

Notes: This table reports the summary statistics of survey respondents' characteristics as follows: Column (1): mean and standard deviation for the sexual minorities in the municipalities that have not introduced the same-sex partnership policy by March 1, 2022 included in the first and second surveys (control group); Column (2): mean and standard deviation for the sexual minorities in the municipalities that have introduced the same-sex partnership policy between March 2, 2021, and March 1, 2022 included in the first and second surveys (treatment group); Column (3): mean and standard deviation for the all; Column (4): the difference between the treatment and the control groups and the standard error. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A14: Summary statistics: Cisgender heterosexuals

	Control	Treatment	All	Difference
Age	48.906	49.701	49.175	0.795*
	[10.783]	[10.168]	[10.585]	(0.395)
# of household	2.569	2.667	2.602	0.098
	[1.221]	[1.217]	[1.221]	(0.062)
Educational background				
Compulsory education	0.027	0.019	0.024	-0.008
	[0.162]	[0.137]	[0.154]	(0.005)
High school	0.264	0.308	0.279	0.043*
	[0.441]	[0.462]	[0.449]	(0.024)
Junior coullege/Higher vocational school	0.138	0.138	0.138	0.000
	[0.345]	[0.345]	[0.345]	(0.013)
University	0.478	0.461	0.472	-0.018
	[0.500]	[0.499]	[0.499]	(0.028)
Master course	0.055	0.047	0.053	-0.008
	[0.229]	[0.212]	[0.223]	(0.010)
Doctoral course	0.017	0.010	0.014	-0.007
	[0.128]	[0.099]	[0.120]	(0.004)
Other degree	0.020	0.018	0.020	-0.002
	[0.141]	[0.133]	[0.138]	(0.005)
None	0.043	0.062	0.050	0.019*
	[0.203]	[0.241]	[0.217]	(0.009)
Household income				
(Unit: 1,000 Yen)				
-100	0.041	0.044	0.042	0.003
	[0.199]	[0.205]	[0.201]	(0.008)
100-199	0.069	0.065	0.068	-0.004
	[0.254]	[0.247]	[0.251]	(0.009)
200-399	0.187	0.186	0.187	-0.001
	[0.390]	[0.389]	[0.390]	(0.016)
400-599	0.208	0.213	0.210	0.004
	[0.406]	[0.409]	[0.407]	(0.016)
600-799	0.161	0.161	0.161	0.000
	[0.367]	[0.368]	[0.367]	(0.013)
800-999	0.125	0.126	0.125	0.001
	[0.330]	[0.332]	[0.331]	(0.012)
1,000-1,199	0.061	0.071	0.064	0.010
	[0.239]	[0.257]	[0.245]	(0.010)
1,200-1,399	0.040	0.027	0.036	-0.013*
	[0.196]	[0.162]	[0.186]	(0.007)
1,400-	0.065	0.046	0.058	-0.019*
	[0.246]	[0.209]	[0.234]	(0.009)
Occupation				
Employee	0.287	0.303	0.292	0.016
	[0.452]	[0.460]	[0.455]	(0.016)
Managerial occupation	0.119	0.096	0.112	-0.024
	[0.324]	[0.295]	[0.315]	(0.014)
Top management	0.033	0.023	0.030	-0.010*
	[0.179]	[0.150]	[0.170]	(0.006)
Government, education	0.054	0.047	0.052	-0.007
	[0.227]	[0.212]	[0.222]	(0.008)
Contract employee	0.056	0.060	0.058	0.004
	[0.231]	[0.237]	[0.233]	(0.008)
Self employed	0.046	0.046	0.046	0.000
	[0.209]	[0.209]	[0.209]	(0.009)
SOHO	0.015	0.011	0.013	-0.004
	[0.121]	[0.104]	[0.115]	(0.004)
Agriculture, fishery, and forestry	0.002	0.002	0.002	0.000
	[0.039]	[0.045]	[0.041]	(0.002)
Professional	0.029	0.021	0.026	-0.008
	[0.168]	[0.143]	[0.160]	(0.006)
Part-time worker	0.098	0.149	0.115	0.051***
	[0.298]	[0.356]	[0.319]	(0.014)
Homemaker	0.130	0.111	0.123	-0.019
	[0.336]	[0.314]	[0.329]	(0.014)
Student	0.011	0.007	0.010	-0.004
	[0.105]	[0.083]	[0.098]	(0.004)
Unemployed	0.105	0.110	0.107	0.005
	[0.307]	[0.313]	[0.309]	(0.013)
Other	0.015	0.015	0.015	0.000
	[0.121]	[0.122]	[0.121]	(0.004)
Observations	1967	1001	2968	2968

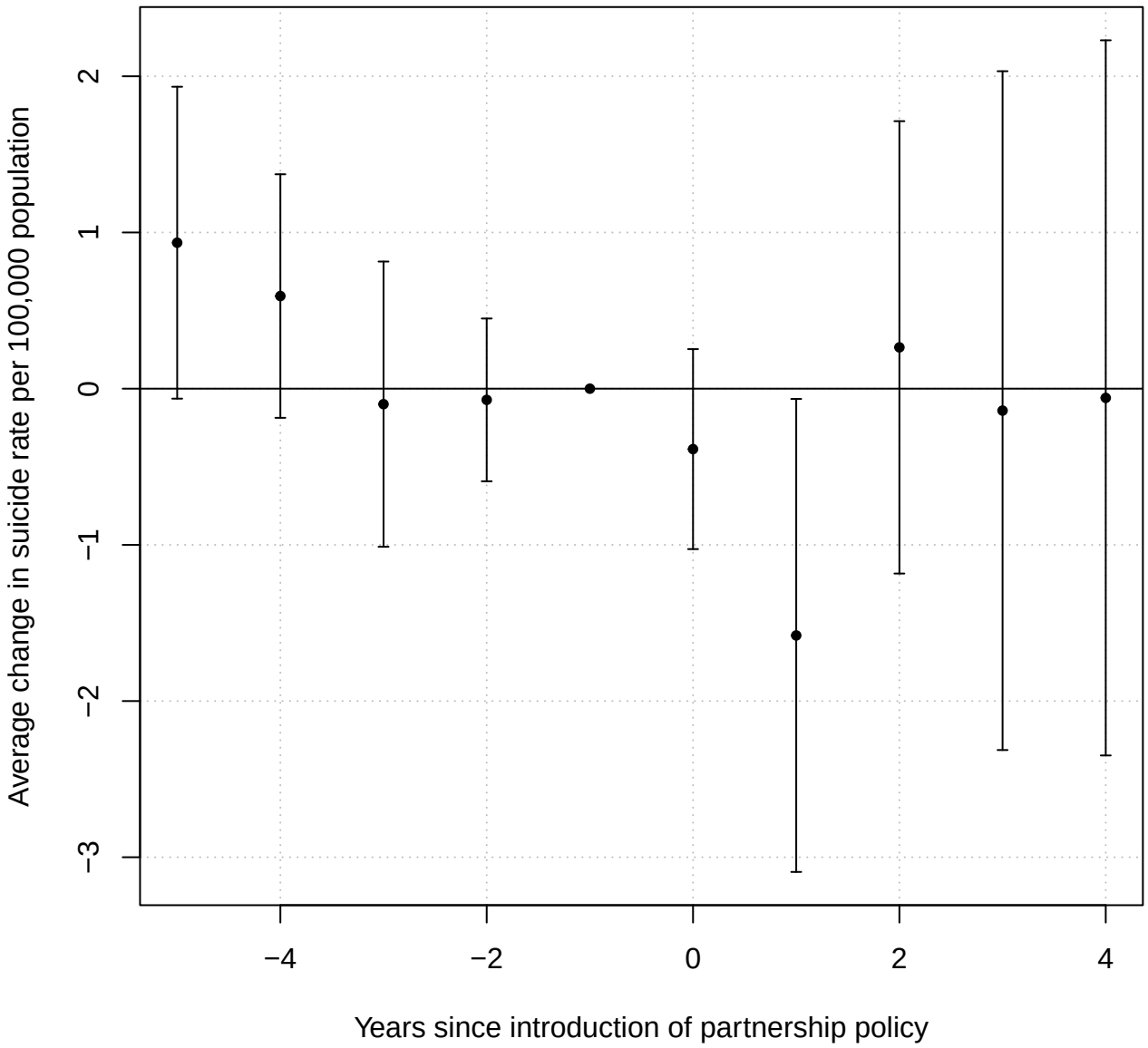
Notes: This table reports the summary statistics of survey respondents' characteristics as follows: Column (1): mean and standard deviation for the cisgender heterosexuals in the municipalities that have not introduced the same-sex partnership policy by March 1, 2022 included in the first and second surveys (control group); Column (2): mean and standard deviation for the cisgender heterosexuals in the municipalities that have introduced the same-sex partnership policy between March 2, 2021, and March 1, 2022 included in the first and second surveys (treatment group); Column (3): mean and standard deviation for the all; Column (4): the difference between the treatment and the control groups and the standard error. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A15: The effects on awareness of the same-sex partnership policy

Dependent variable is:	Dummy that respondents assume their munic. has introduced the policy			
	LGBT		Non-LGBT	
	(1)	(2)	(3)	(4)
Partnership$_m$	0.054***	0.035***	0.038**	0.012
	(0.018)	(0.012)	(0.017)	(0.013)
R^2	0.010	0.029	0.013	0.035
Observations	2,514	2,514	2,976	2,976
Mean dep. var.	0.046	0.046	0.028	0.028
Bordered municipalities F.E.s		✓		✓
Controls	✓	✓	✓	✓

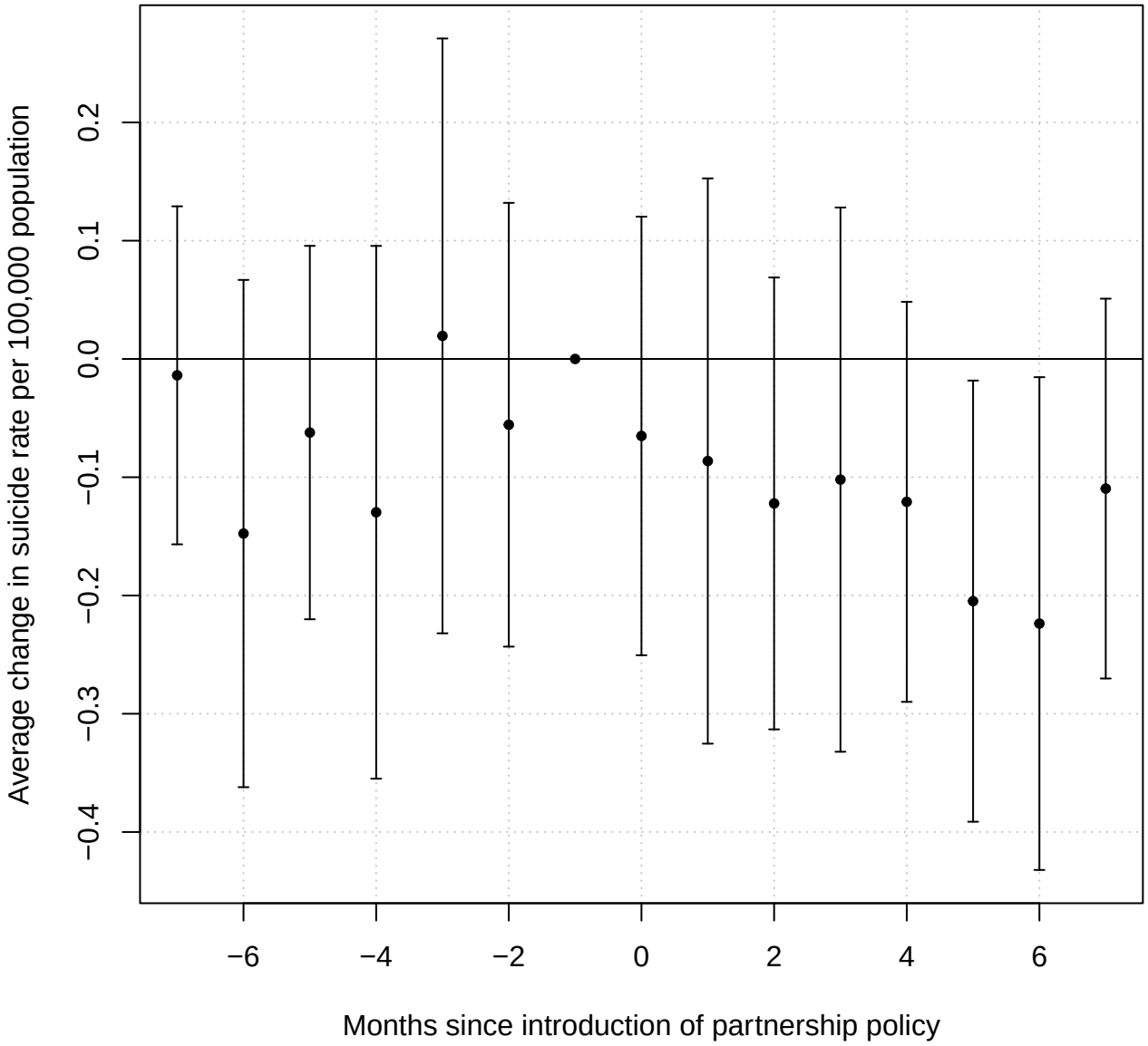
Notes: The table reports coefficients on the dummy that takes 1 if municipality m has introduced the same-sex partnership policy between March 2, 2021, and March 1, 2022, included in the second survey. Control variables are municipality-level socioeconomic factors in 2015 that contain the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. All regressions are weighted by the population in each municipality in 2015. Standard errors clustered at the municipality level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Figure A1: Annual-level event study



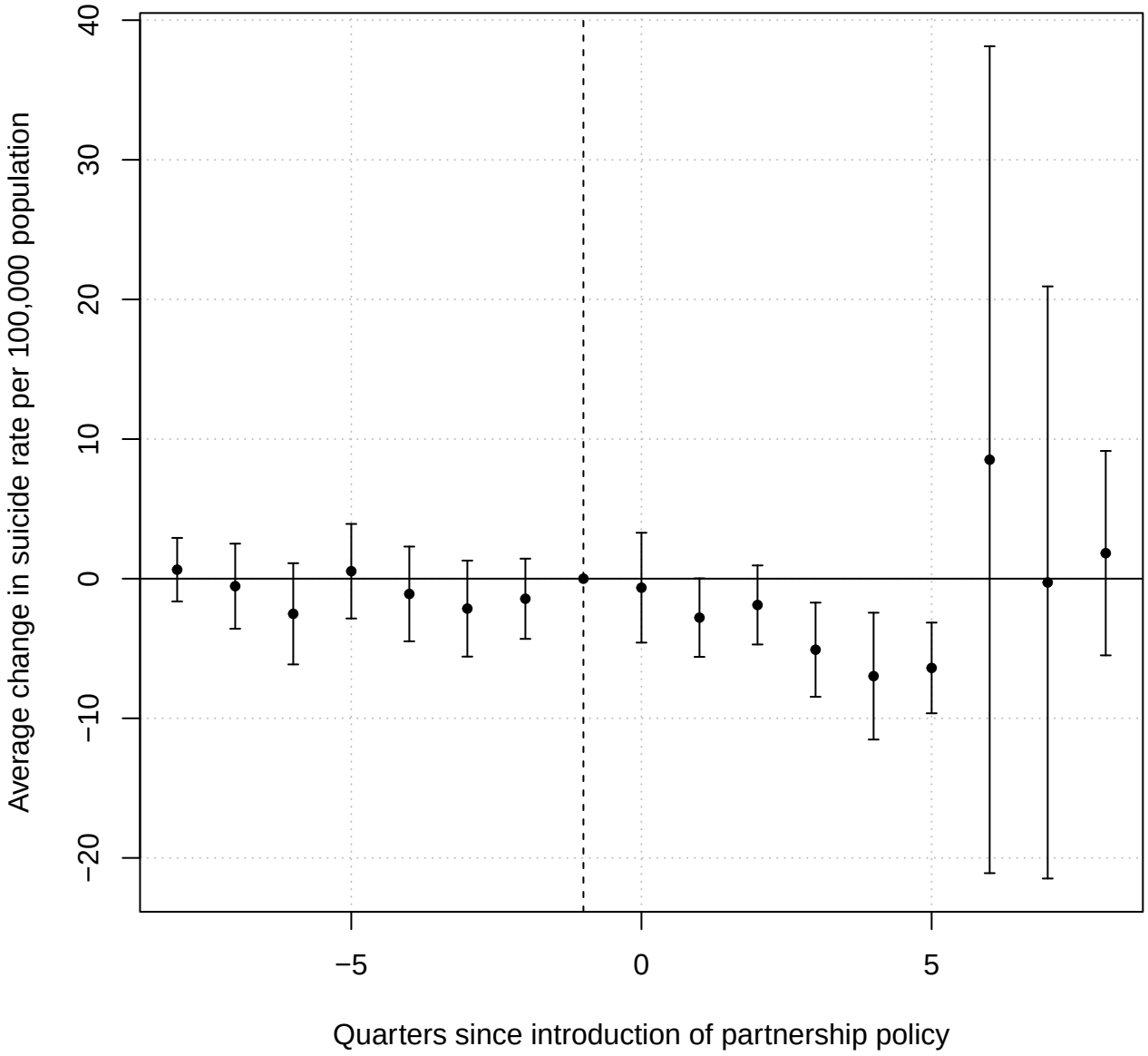
Notes: This figure shows annual-level event study estimates and the corresponding 95 percent confidence intervals using the cluster-robust standard error at the municipality level. $y = -1$ is a reference year. The dependent variable is the annual suicide rates per 100,000 population. The independent variables are dummies that take 1 if year y equals the year of the introduction plus or minus l. The estimation includes municipality fixed effects, year fixed effects, and controls × year fixed effects. Controls are the municipality-level socioeconomic factors in 2010 that contain the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. The regression is weighted by the population in each municipality in 2009.

Figure A2: Monthly-level event study



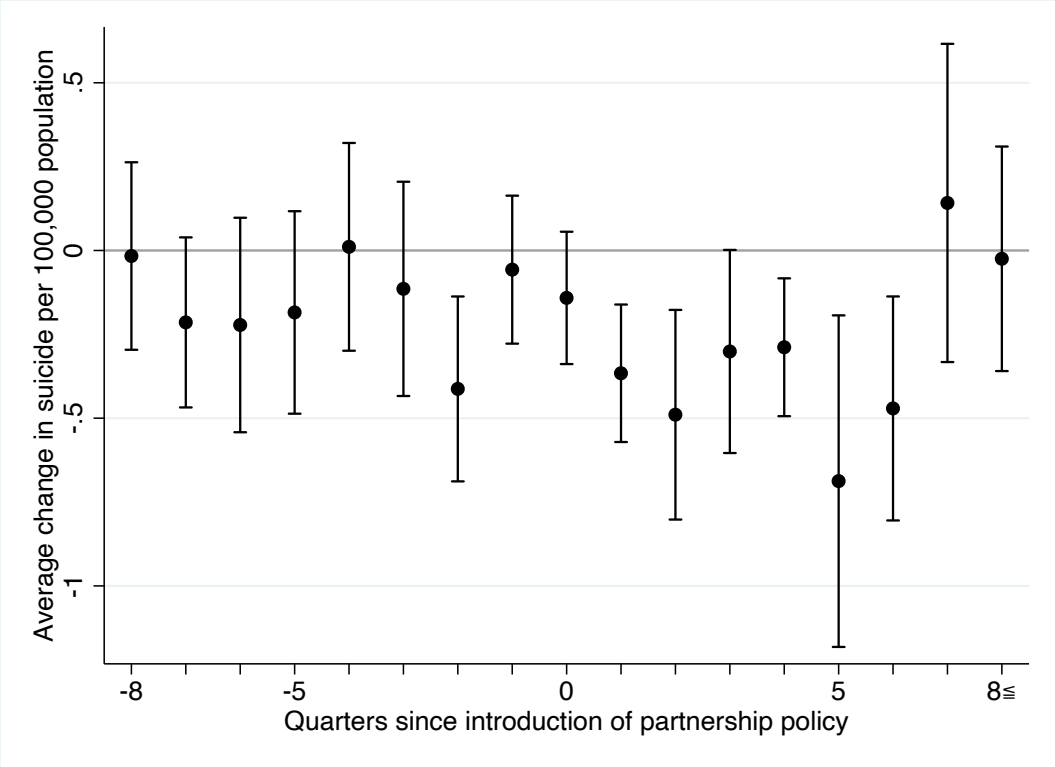
Notes: This figure shows monthly-level event study estimates and the corresponding 95 percent confidence intervals using the cluster-robust standard error at the municipality level. $t = -1$ is a reference month. The dependent variable is the monthly suicide rates per 100,000 population. The independent variables are dummies that take 1 if month t equals the month of the introduction plus or minus l. The estimation includes municipality fixed effects, month fixed effects, and controls $\times$ month fixed effects. Controls are the municipality-level socioeconomic factors in 2010 that contain the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. The regression is weighted by the population in each municipality in 2009.

Figure A3: Quarterly-level event study by vote share of non-LDP parites



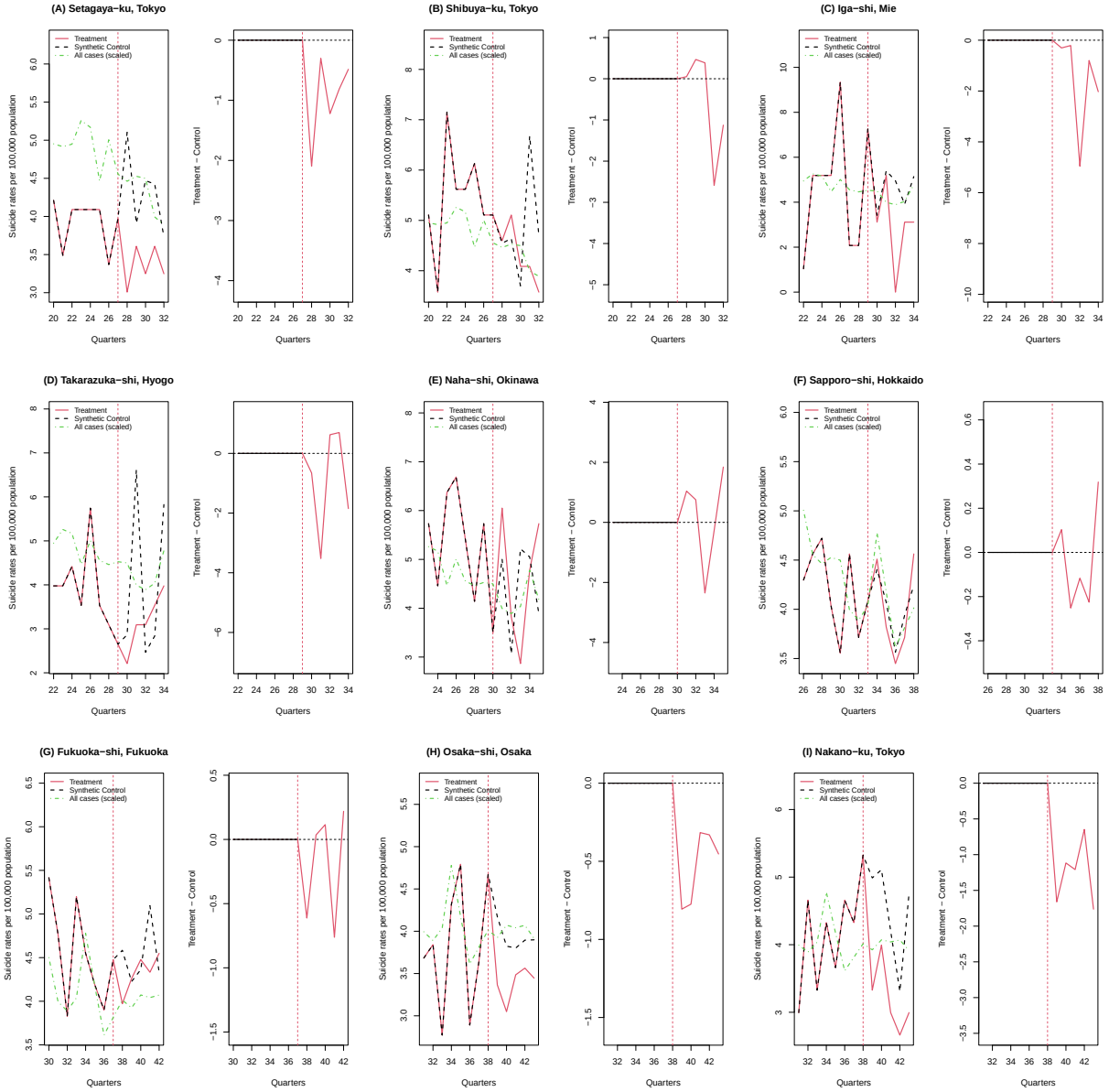
Notes: This figure shows event study estimates and the corresponding 95 percent confidence intervals using the cluster-robust standard error at the municipality level. $q = -1$ is a reference quarter. The dependent variables are the quarterly suicide rates per 100,000 population. The independent variables are dummies that take 1 if quarter q equals the quarter of the introduction plus or minus l, and the interaction terms with average vote shares of non-LDP parties at municipality m in lower house elections (2009, 2012, 2014, 2017). This figure plots only the estimated coefficients on the interaction terms. The estimation includes municipality fixed effects, quarter fixed effects, and controls × quarter fixed effects. Controls are the municipality-level socioeconomic factors in 2010 that contain the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. The regression is weighted by the population in each municipality in 2009.

Figure A4: Quarterly-level event study: Alternative method by Borusyak et al. (2021)



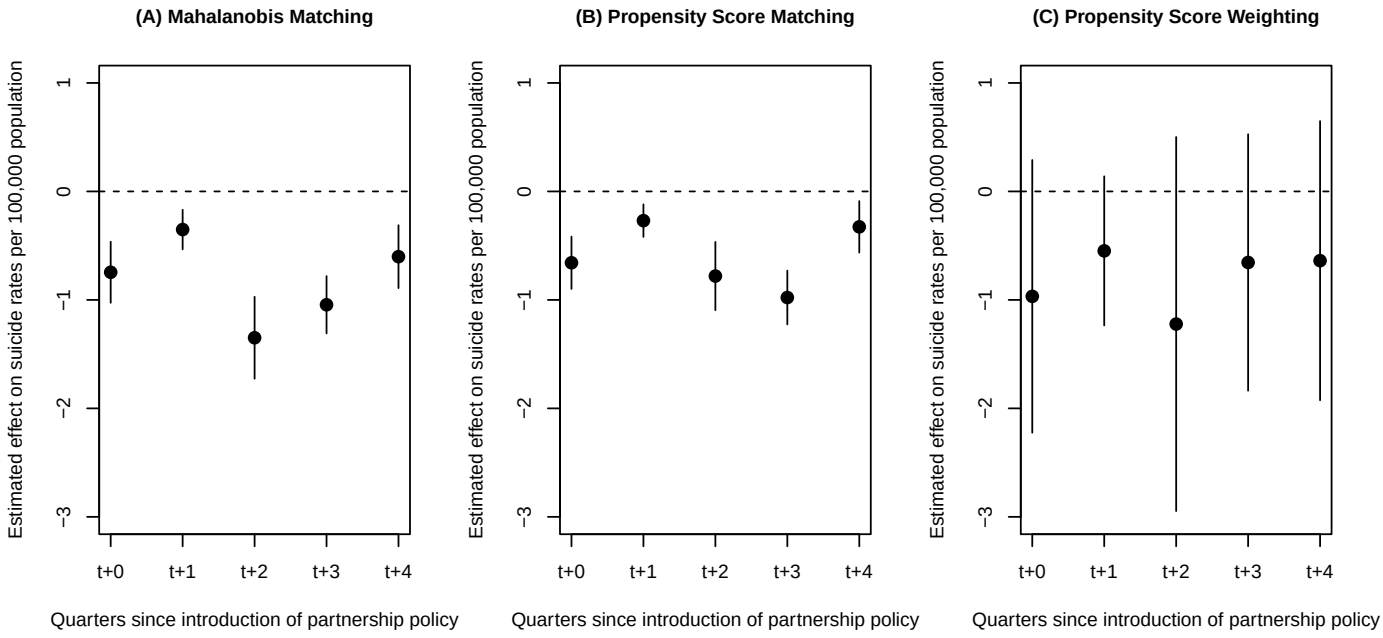
Notes: This figure shows event study estimates from a method proposed by Borusyak et al. (2021) and the corresponding 95 percent confidence intervals using the cluster-robust standard error at the municipality level. The dependent variable is the quarterly suicide rates per 100,000 population. Appendix A.1 explains the estimation procedure. The estimation includes municipality fixed effects, quarter fixed effects, and controls $\times$ quarter fixed effects. Controls are the municipality-level socioeconomic factors in 2010 that contain the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. The regression is weighted by the population in each municipality in 2009.

Figure A5: Results of synthetic control method



Notes: This figure reports results from the synthetic control method. The outcome is the quarterly suicide rates per 100,000 population. The predictor time-varying variables are the quarterly suicide rates per 100,000 population of all age groups, those under 20, in their 30's, 40's, 50's, and over 60. The synthetic controls are constructed using the predictors in the prior 8 quarters before the introduction of the same-sex partnership. The results are compiled up to 4 quarters after the policy introduction. The dashed verticle lines correspond to the quarter of the introduction of the policy.

Figure A6: Result of matching methods



Notes: This figure reports the average treatment effects on treated of introducing the same-sex partnership policy using matching methods proposed by Imai et al. (2021). The outcome is the quarterly suicide rates per 100,000 population. The covariates are municipality-level socioeconomic factors in 2010 that contain the population density, the share of the population aged 65 years or over, the sex ratio, the ratio of foreign residents, the share of employment in the primary industry, and the share of employment in the tertiary industry. Panel A applies Mahalanobis distance matching, and Panel B applies propensity score matching, each with up-to-ten matches. Panel C applies propensity score weighting. All specifications construct the matches or the propensity score based on the one-quarter past treatment status.