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Essays in Empirical Economics of the Family

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Abstract

This thesis studies important family decisions empirically. Families are confronted with various challenges and shocks including long commutes, insolvency and unwanted pregnancies. I study how different actors including firms, state officials and NGOs influence family outcomes when dealing with these challenges. This thesis consist of three quasi-experimental empirical studies that approximate causal impacts of shocks, including firm relocations, aid center locations and mailing campaigns, on important outcomes of family stability, personal bankruptcy rates and reproductive rates. This thesis contributes to existing empirical research by proposing novel identification strategies and using new sources of variation.

In the first chapter we study the impact of firm relocations on commuting distance and the probability of married and cohabiting couples with children separating. We use Swedish register data for 2010-2016 and select employees of relocating firms with one workplace and more than 10 employees. Focusing on this sample allows us to use plausibly-exogenous variation in the commuting distance arising from the relocation. We extend the literature on the effect of commuting on relationship stability by reducing the possibility for unobserved time-variant factors to bias our estimates. While previous literature has focused on the difference between short- and long-distance commuting, we focus on changes in the commuting distance that are externally induced by firm management. We find a small but statistically significant negative effect of increased firm relocation distance on family stability. A 10 km change in commuting distance leads to a 0.09 percentage point higher probability of separation if the commuter remains with the firm for the next 5 years.

In the second chapter, we study how access to public services can influence their take-up. Personal bankruptcy aims to provide a fresh start to debtors. While bankruptcy is often the only solution to financial distress, large spatial distances to affordable legal services may result in their underuse by eligible debtors. Using a large administrative dataset of personal bankruptcies, we study the impact of spatial distance from public Centers for Legal Aid (CLAs) on the regional incidence of personal bankruptcy in Slovakia. We avoid endogeneity by focusing on the increased availability of legal aid while controlling for the expected distance from the nearest CLA, which serves as the first contact point in the process of filing for personal bankruptcy in the Slovak Republic. Distance from these legal aid centers has a significant impact on personal bankruptcy rates: the closer the nearest CLA, the larger the prevalence of personal bankruptcy in

a given municipality. We quantify the impact of service access on personal-bankruptcy rates, showing that improved access to free legal aid has both a statistically and substantively significant impact on the use of personal bankruptcy by the public. At the end of the almost 3-year-long period analyzed, municipalities with good access to CLAs had 3.3 more bankruptcies per 1,000 inhabitants than those with weak access to CLAs. This effect is significant, as the average national bankruptcy rate until December 2019 reached 6.3 bankruptcies per 1,000 persons.

In the third chapter, I study the impact of mailing campaigns on families. Both pro-choice and pro-life NGOs believe their campaigns to be effective, but there is little evidence on this question due to the rare nature of abortion and the selective nature of activist location and intensity. In this paper, I provide the first empirical assessment of the conception and abortion-rate effects of a pro-life leaflet mailing campaign. I study an extensive mailing campaign that operated in Slovakia during 2016-2017 and employed widely used pro-life frames. To identify its effects, I use a municipality-level inverse probability weighting strategy. The results suggest that the campaign did not have a statistically significant effect on conception rates or abortion rates conditional on conception.

Abstrakt

Tato práce empiricky zkoumá důležitá rodinná rozhodnutí. Rodiny jsou konfrontovány s různými problémy a šoky, včetně dlouhého dojíždění do práce, platební neschopnosti a nechtěného těhotenství. Studuji, jak různí aktéři včetně firem, státních úředníků a nevládních organizací ovlivňují výsledky rodin při řešení těchto výzev. Tato práce se skládá ze tří kvaziexperimentálních empirických studií, které aproximují kauzální dopady šoků, jako jsou přemístění firem, umístění center pomoci a poštovní kampaně, na důležité výsledky stability rodiny, míru osobního bankrotu a míru reprodukce. Tato práce přispívá ke stávajícímu empirickému výzkumu tím, že navrhuje nové identifikační strategie a využívá nové zdroje variability.

V první kapitole studujeme dopady přemístění firem na dojezdovou vzdálenost a pravděpodobnost rozchodu manželských párů a párů s dětmi žijící ve společné domácnosti. Používáme údaje ze švédského registru za období 2010-2016 a vybíráme zaměstnance stěhujících se firem s jedním pracovištěm a více než 10 zaměstnanci. Zaměření na tento vzorek nám umožňuje využít věrohodně exogenní variabilitu dojezdové vzdálenosti vyplývající z přemístění. Rozšiřujeme tak literaturu o vliv dojíždění do zaměstnání na stabilitu vztahů tím, že omezujeme možnost, aby nepozorované časově proměnné faktory zkreslovaly naše odhady. Zatímco předchozí literatura se zaměřovala na rozdíl mezi dojížděním na krátkou a dlouhou vzdálenost, my se zaměřujeme na změny dojezdové vzdálenosti, které jsou externě vyvolány vedením firmy. Zjistili jsme malý, ale statisticky významný negativní vliv zvýšené dojezdové vzdálenosti firmy na stabilitu rodiny. Změna dojezdové vzdálenosti o 10 km vede ke zvýšení pravděpodobnosti rozchodu o 0,09 procentního bodu, pokud dojíždějící zůstane ve firmě po dobu následujících 5 let.

Ve druhé kapitole zkoumáme, jak může přístup k veřejným službám ovlivnit jejich využití. Cílem osobního bankrotu je poskytnout dlužníkům nový začátek. Ačkoli je bankrot často jediným řešením finanční tísně, velká prostorová vzdálenost k dostupným právním službám může vést k tomu, že jej oprávnění dlužníci nevyužívají dostatečně. Na základě rozsáhlého souboru administrativních údajů o osobních bankrotech studujeme vliv prostorové vzdálenosti od veřejných center právní pomoci (CPP) na regionální výskyt osobních bankrotů na Slovensku. Endogenitě se vyhýbáme tím, že se zaměřujeme na zvýšenou dostupnost právní pomoci kontrolující očekávanou vzdálenost od nejbližšího CPP, které funguje jako první kontaktní místo v procesu podávání žádostí o osobní bankrot ve Slovenské republice. Vzdálenost od těchto center právní pomoci má významný vliv na míru osobního bankrotu: čím blíže je nejbližší CPP, tím větší je výskyt

osobního bankrotu v dané obci. Kvantifikujeme vliv dostupnosti služeb na míru osobního bankrotu a ukazujeme, že lepší dostupnost bezplatné právní pomoci má statisticky i věcně významný vliv na využívání osobního bankrotu veřejností. Na konci téměř tříletého analyzovaného období připadalo v obcích s dobrým přístupem k CPP o 3,3 osobního bankrotu na 1 000 obyvatel více než v obcích se slabým přístupem k CPP. Tento efekt je významný, neboť průměrná celostátní míra bankrotů do prosince 2019 dosáhla 6,3 bankrotu na 1 000 osob.

Ve třetí kapitole se zabývám dopadem mailingových kampaní na rodiny. Jak pro-choice, tak pro-life nevládní organizace se domnívají, že jejich kampaně jsou účinné, ale důkazů o této otázce je málo vzhledem k nízké frekvenci potratů a selektivní povaze umístění a intenzity aktivistů. V tomto článku předkládám první empirické hodnocení účinků pro-life letákové mailingové kampaně na početí a potraty. Studuji rozsáhlou mailingovou kampaň, která probíhala na Slovensku v letech 2016-2017 a využívala široce používané pro-life rámce. K identifikaci jejích účinků využívám strategii inverzního vážení pravděpodobnosti na úrovni obcí. Výsledky naznačují, že kampaň neměla statisticky významný vliv na míru početí ani na míru potratů podmíněných početím.

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Chapter 1

Firm Relocations, Commuting and Relationship Stability

Coauthored with Erika Sandow (Department of Geography at Umeå University) and Urban Lindgren (Department of Geography at Umeå University).

1.1 Introduction

Commuting times of workers in Europe have been increasing slowly but steadily in the last decade.[1] Policymakers and firms sometimes weigh the costs and benefits of expanding labor markets and increasing average commuting distances. The cost-benefit analyses of commuting often consider the economic and environmental effects of commuting. Fewer studies consider its social impacts. In particular, commuting time may influence the quality of partnerships. Time spent traveling could decrease time spent with family members and contributing to household chores, which may increase stress. Ultimately, the commuting situation could become unbearable and lead to separation. However, wage compensation and increased employability may also result from a longer commute and thus the total impact of commuting on relationship stability is an empirical question. Currently, evidence about the impact of commuting time on relationship stability is

1. According to Numbeo, which provides traffic rankings since 2012 at https://www.numbeo.com/traffic/rankings_by_country.jsp, and calculations by the authors, among 27 countries included in the 2012 and 2018 index, the population weighted average of commuting time increased from 35.5 minutes one way in 2012 to 36.6 minutes in 2018. Further information about the methodology is available at numbeo.com.

scarce.

Commuting distance is a job characteristic that is usually known to the job seeker when he chooses to accept the job. We can understand commuting distance as a joint equilibrium outcome on a job market and a housing market. Therefore, we are not usually able to model commuting distance as randomly allocated. [2] Jarvis (1999a) finds that the employment structure of the household influences mobility strategies. In particular, dual-earner households are less likely to migrate.Green, Hogarth, and Shackleton (1999) find that most commuting benefits are enjoyed by the commuter, and most commuting costs are borne by the commuter's partner.

If a firm relocates, it might be a surprise to an employee, though the particular circumstances of any employee should not influence the relocation decision in large firms. A change in commuting distance could be considered to be a random shock to both the commuting distance and job quality.

In this paper, we focus on employees of single-site firms with more than 10 employees in 2010 or 2011. First, we examine the properties of relocating firms and check that they are similar to non-relocating firms in baseline characteristics. We then estimate the impact of relocation-induced commuting distance changes on family stability outcomes. Our main results point to a small but significant effect of an increase in the commuting distance on the probability of couples separating.[3]

Subsequently, we consider other sources of variation in the commuting distance: job change and residential move. We find that change in commuting distance related to job change is weakly and statistically non-significantly associated with separation, whether the original firm relocated or remained stationary. Moving residence during firm relocation does not have a significant association with separation. Residence moves away from a stationary firm were associated with significantly lower separation probability. Moving while working in a stationary firm seems to be the most endogenous choice, as a decision to invest in a common good of a new house can be reasonably expected to depend on partnership expectations.

2. There are also qualitative papers studying factors that influence couple mobility strategies (Jarvis 1999b, 1999a, 1999c; Green 1997; Green, Hogarth, and Shackleton 1999).

3. Sample size does not allow us to consider the potentially assymetrical impact of commuting distance.

1.2 Literature review

This study is related to several strands of literature. We follow the family stability literature in uncovering how social and economic factors influence separation and divorce. Theoretical works provide frameworks for empirical studies, including observational studies about the association of long-distance commuting and family stability. Next, we build on the firm relocation literature methodologically, by examining the impact of relocations on socioeconomic outcomes. Further, it is useful to compare our results with firm-closure studies that have also considered family stability an outcome.

1.2.1 Theories of marital stability

Kley (2015) identifies several theories of marital stability, which she uses to support her empirical model. In particular, she uses social exchange theory (Kelley and Thibaut 1978) and microeconomic theory of divorce (Becker, Landes, and Michael 1977). These theories are not mutually exclusive and often give rise to similar predictions.

According to social exchange theory, marital stability is influenced by unfolding social-exchange processes within and outside of the couple. This theory was applied early by Levinger (1965), who recognizes attractions, barriers to leaving, and the presence of attractive alternatives to the relationship.

In the context of our study, long-distance commuting potentially changes the relationship value for one or both partners by reducing the time the couple spends together and by changing the bargaining-power distribution within the couple. Long-distance commuting could influence attractive alternatives to the partnership both ways. On the one hand, less time to socialize outside of family and work may decrease the perceived alternatives to the relationship. On the other hand, as Kley (2015) mentions, the commuter may likely have friends at work that are not shared with the less-mobile partner and can increase alternatives to the relationship.

According to the microeconomic theory of divorce, the value of staying in a partnership is central to family stability. The partnership value depends on partner match, division of labor within the household, and investment in couple-specific capital. The partner match is related to the labor division within the couple. While Becker argued that the negative correlation of wage-earning powers should increase couple stability, newer empirical studies contest this theory. For example, Kley (2015) finds that while labor participation of women in Western Germany is associated with the probability of

separation, it is not significantly associated in Eastern Germany. The partner match is improved by homogeneity in complementary factors including age and education level. The couple-specific capital includes shared homeownership and children. These assets may stabilize relationships because they lose value at separation. However, the value of couple-specific capital may be reduced during long-distance commuting as the more mobile partner has less time to use it, and the less-mobile partner may need to do more chores. Kley (2015) finds that women, in particular, may be affected by long commutes as they are responsible for the bulk of household chores.

1.2.2 Association of long-distance commuting with family stability

This paper is also related to the literature about the effects of long-distance commuting. In particular, several recent studies analyze the association of long-distance commuting with family stability (Sandow 2014; Kley 2015; Kley and Feldhaus 2017). Research has also been conducted on perceptions of partnership quality (Viry, Widmer, and Kaufmann (2010)).

Sandow (2014) uses the Swedish National Register and event-history analysis with discrete-time logistic regression to examine whether long-distance commuting predicts couple separation. The study finds that the association differs according to gender and the duration of the long-distance commuting. Couples who commute for longer than 5 years seem to either selectively survive or to have become accustomed to the commuting lifestyle. The register data did not include either travel time or travel mode, and it was not possible to control for specific motivations for long-distance commuting. In this study, we estimate commuting time to see if the results from commuting distance generalize for commuting time. Sandow (2014) did not provide a causal interpretation of the data as the commuting behavior may be endogenous to individual time-changing factors associated with relationship stability. In this paper, we address this limitation by using firm relocations as a source of variation in commuting distance.

Kley and Feldhaus (2017) and Kley (2015) use German survey data. Kley and Feldhaus (2017) study long-distance commuting using the nationally representative sample Panel Analysis of Intimate Relationships and Family Dynamics (pairfam) with 2,500 couples and monthly data over 3 years. Pairfam data allows one to control for an annual measure of marital quality and commuting for up to 2 residences. Furthermore, pairfam

includes the timing of breakups and changes of residence and employment in months.[4] The authors find that long-distance commuting of women for longer than 1 hour predicts lower family stability, except for women living in the suburbs. These associations persist even after controlling for breakup predictors frequently found in the literature, including social position of the partners, barriers to leaving the partnership, partnership quality and labor division within the household. Kley (2015) studies the effect of commuting time on relationships. She uses monthly data from a three-year-long study on 890 inhabitants of two comparable German cities and tests hypotheses based on social exchange and economic theories of marital stability. The author controls for theoretically important predictors of marital stability, including indicators of the partner match (age at start of marriage, homogamy in education levels, division of labor within the household, couple-specific capital, and barriers to leaving the relationship. While these predictors are theoretically important, they are likely interrelated, which could make the interpretation of the findings difficult. The study finds higher and significant associations of long-distance commutes of women for Eastern Germany and lower association for women of Western Germany. The author explains this difference by different social acceptance of paid work of mothers.[5] In Western Germany, the women's paid employment predicts the separation of couples, while long-distance commuting has a smaller and less significant association with family stability.

Using the British Household Panel Study dataset, Nisic and Kley (2019) study long-distance commuting and define long-distance as longer than 1 hour. They also consider moves longer than 50 km. The authors first describe the social structure of mobile couples and then focus on their satisfaction with their social life. They find that women are more affected by mobility than men. They find a negative association of long commuting and satisfaction with social life for women who move because of their partner's career prospects.

Viry, Widmer, and Kaufmann (2010) view daily long-distance commuting as one of the examples of mobility strategies. Their survey data include a reason for commuting and information about other kinds of mobility (weekend commuting, long-distance relationships). They find that the effect of commuting can be mediated by the life course, the process by which one becomes mobile, social status, and national context (including

4. For more information about pairfam, see Huinink et al. (2011).

5. The social acceptance of female paid work is consistent with labor participation data: In 2005, 72% women in Eastern Germany participated in the labor market in contrast to 65% of women in Western Germany. (Source: Federal Statistical Office Germany.)

family policy regime, residential amenities such as daycare and after school programs, and quality of transportation systems).

For a comparison of the main studies, see Table 1.1.

Table 1.1: Commuting longer than 30 km and family stability

Odds ratios of separation	Women	Men	Country
Annual:			
Sandow (2014) commuter	0.98–1.02	1.02–1.05 *	Sweden
Sandow (2014) 1-4 years	1.00–1.04	1.02–1.06 *	Sweden
Sandow (2014) 5+ years	0.90–0.96	1.00–1.05 *	Sweden
Monthly:			
Kley (2015)	3.33**	0.33	Germany
Kley and Feldhaus (2017)	1.97**	1.04	Germany

We have included a range of estimates depending on reported specifications.
Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Sources: Sandow (2014), Kley (2015), and Kley and Feldhaus (2017)
Time frames: 3 years Kley (2015) and Kley and Feldhaus (2017), 5-10 years Sandow (2014).
Reference groups: non-commuting workers Sandow (2014) and Kley and Feldhaus (2017), non-working Kley (2015).

1.2.3 Firm relocations

This study is methodically inspired by the literature about firm relocations. The closest paper to our study is by Mulalic, Van Ommeren, and Pilegaard (2014), who investigate wage compensation of workers at relocating firms. They estimate the causal effect of firm relocation using data from the Danish register of companies. The study finds that workers are compensated for the increase in commuting distance in the long term, while the short-term wage compensation is small. The authors deal with endogeneity in commuting distance by including worker and firm fixed effects in the empirical model. They then estimate their models in terms of first differences and consider both the short- and long-term

1.2.4 Firm closures

When we consider the effect of single-site firm relocation on the social life of the firm's employees, one obvious comparison is with firm closures. In both cases, a given job bundle ceases to exist. The difference is that in the case of firm relocations, a similar job bundle appears at a different spatial location. The employee of a relocating firm thus has, ceteris

paribus, a better position in terms of employment opportunities. Therefore, firm closures should impact family stability more strongly than firm relocations.

Rege, Telle, and Votruba (2007) use data from Statistics Norway for 1992 to 2005. They estimate a linear probability model of marital dissolution of workers in firms with more than 10 employees. They control for characteristics of couples, including socioeconomic variables for both the couple and each spouse individually, indicators of spousal dependency (children, age and educational homogamy), and the downsizing experience of the wife. The marriages of husbands originally employed in firms that closed between 1995 and 2000 were 11 percent more likely to end in divorce by 2003 than comparable marriages of husbands in stable firms. The authors also test whether working in a firm before closure increases the divorce rate and find that it does not.

Keldenich and Lücke (2018) use the German Socioeconomic Panel (SOEP) from 1984 to 2015.[6] They use involuntary job loss as a source of variation, including firm closures. For robustness checks, they examine firm closures: They estimate a discrete-time proportional-hazard model, specifically a complementary log-log model for censored data. They use 1,526 involuntary job losses and find a 70 percent larger risk of divorce in the following year if the husband has experienced an involuntary job loss. They compare this finding to results using only firm closures and find no substantive difference in results.

Our study links the family stability literature to the methodology of firm-relocation studies. This allows us to provide the first causal estimates of the effect of commuting distance on the probability of couple separation. We also extend the firm-relocation literature methodologically by estimating the lower bound of relocation-induced changes in commuting time. Our commuting-time algorithm, based on geographical coordinates, appears to be the first such algorithm in the literature; it allows for new avenues of research, including that on firm relocation.

Based on the literature review, we formulate the following hypotheses:

- Hypothesis 1: Increase in commuting distance due to firm relocation increases long- and medium-term risk of separation.

- Hypothesis 2: Increase in commuting time due to firm relocation increases long- and medium-term risk of separation.

6. For more information on the dataset, see Wagner, Frick, and Schupp (2007).

1.3 Data

In this study, we use linked Swedish individual and firm register data from Statistics Sweden, obtained via secured institutional access. The data includes comprehensive administrative information about firms, employees and their partners. Our firm data was limited to single-site firms, allowing us to unambiguously define firm relocation as the change in easting and northing coordinates of the workplace. These coordinates are available in the administrative data with a 100-meter precision. The firm and workplace definitions of Statistics Sweden allow us to follow stable and relocating firms that keep at least half of their workforce every year.

We restrict the sample further to firms with 10 or more employees in the baseline year, as relocation decisions of large firms are plausibly unrelated to the situation of individual workers (similarly to Mulalic, Van Ommeren, and Pilegaard (2014)). We restrict the baseline years to 2010 and 2011 and study the workers in firms that belong to both baseline samples 3 and 5 years after each baseline year. 2010-2011 was chosen as a relatively stable period following the 2008/9 economic crises. We also chose a 5-year period of followup so our data spans up to 2016. [7] From these firms, 702 firms in the baseline year relocated 5–200 km the following year. We restrict the relocating firms to those that relocated by less than 200 km, because given larger relocations, workers may be more selective in deciding whether or not to stay with the firm. Another 5,760 firms relocated by 5 km or less. These shorter relocations should not have a noticeable impact on commuting distance or commuting time, so they are not included in the main analysis.[8] From the firms that relocated by 5–200 km, we selected employees who were either married or cohabiting with common children in the baseline year. This leaves 3,424 employees who met our criteria and remained with the relocating firms 3 years after relocation, and 2,197 who remained 5 years after relocation, with attrition of 36% of workers. For a comparison, 287,266 employees worked in the firms which relocated 0-5 km 3 years after the relocation, and 222,759 remained 5 years after relocation, with attrition of 22% of workers.

We use the following **firm level variables** from statistics Sweden: workplace coordinates, **industry SNI code**, municipality code, number of employees and sales.

Individual variables: Gender, residential coordinates and municipality, education

7. We consider 3 years for a medium- and 5 years for a long-term effect of induced commuting on relationship separation. We pool workers from both baseline years together to increase the sample size.

8. We include them as a placebo test in the Robustness section, Table 1.15.

level, single digit occupation, family status, duration of partnership, number of children under the age of 6 in the household, and education level of the partner.

Variables are motivated mostly by inclusion in the literature, shown in Table 1.2.

Table 1.2: Overview of variables used

Variable name	**Sources using similar variables or reason for inclusion**
Commuting distance	Mulalic, Van Ommeren, and Pilegaard 2014; Sandow 2014; Kley 2015; Kley and Feldhaus 2017; Nisic and Kley 2019
Industry code (SNI)	Sandow 2014: employment sector
Residence municipality code	Mulalic, Van Ommeren, and Pilegaard 2014; Sandow 2014
Municipality type	Sandow 2014; Viry, Widmer, and Kaufmann 2010
Number of employees, profit	Mulalic, Van Ommeren, and Pilegaard 2014 (supplementary model, p. 1102)
Sales	This variable is a marker of firm quality.
Gender	Mulalic, Van Ommeren, and Pilegaard 2014; Sandow 2014; Kley and Feldhaus 2017; Viry, Widmer, and Kaufmann 2010; Nisic and Kley 2019
Education level	Mulalic, Van Ommeren, and Pilegaard 2014; Sandow 2014; Kley 2015; Viry, Widmer, and Kaufmann 2010; Nisic and Kley 2019
Single digit occupation	Relocation may be associated with occupation change, which may influence family stability.
Family status	Sandow 2014; Kley and Feldhaus 2017; Viry, Widmer, and Kaufmann 2010
Duration of partnership	Sandow 2014; Kley 2015
Number of children under the age of 6 in the household	Sandow 2014; Kley and Feldhaus 2017; Viry, Widmer, and Kaufmann 2010; Nisic and Kley 2019
Partner's education level	Kley 2015; Viry, Widmer, and Kaufmann 2010
Partner's job status	Kley 2015; Nisic and Kley 2019

We construct the relocation distance of the firm, commuting distances and distances of any residential moves in meters. The precision of distances is 100 m^2. We construct categorical values for marriage or cohabiting with children, for separation, for occupation and for education.

We also estimate minimal commuting time using ArcGIS and considering available

roads, one-way restrictions, tunnels and bridges, including the speed limits.[9] Our model uses the following inputs: first, a table with firm coordinates in each year. We include only firms that relocate more than 5–200 km in this table, and second, a table with employee-residence coordinates and firm ID for each year. In the second table, we include only the employees working in the relocating company in the baseline. The model matches firms and employees by year and firm ID, then calculates an optimization problem that minimizes the estimated commuting time. The output includes the estimated time of the commute and road distance of the calculated route. The correlation of estimated commuting time and road distance is 0.96, while that of road distance with euclidean distance is 0.97.

Our estimation of commuting time uses an assumption that everyone commutes by car. The Swedish Transport Administration Annual Report for 2017 gives the following statistics: "*On a regular day, nearly 1 million people take a bus, almost as many people choose to cycle, 4.5 million take a car and 370,000 take a train. ... More than 21,600 people take a domestic flight per day.*" (p. 6). The vast majority of travel is by car or bus, which use public roads. The number of flights is negligible and we assume they are used mostly for commutes longer than 200 km, which we do not consider in this analysis. Rail transport may introduce some measurement error, but it is limited by the relatively low use of rail in Sweden. Another possibly more serious cause of measurement error in our commuting time estimates is due to imprecise estimation of travel time. The estimate could be biased upwards due to low compliance with road speed limits, which is only about 40% [10]. It could also be biased downwards due to traffic congestion, which is documented and monitored in the Stockholm, Gothenburg, and Malmö metropolitan areas.[11].

We constructed educational homogamy based on education levels of both partners in the couple (we used the 7 levels used by Statistics Sweden). Couples' educational homogamy was rated as 1 if the education levels were the same.

9. To move from estimating minimal commuting time to actual commuting time, it would be ideal to include information about traffic congestion. Unfortunately, we did not have this data and could not take the coordinate pairs out of the computer lab to Google Maps API.

10. Swedish Transport Administration Annual Report for 2017, p. 18.

11. Swedish Transport Administration Annual Report for 2017, p. 11.

1.3.1 Local context

In comparison to many other countries in Europe Sweden is sparsely populated, having a population density of 26 inhabitants per square kilometre. The population density of the ten largest urban areas spans between 3,580 and 7,724 inhabitants per square kilometre. This reflects a settlement pattern dominated by a few large cities (Statistics Sweden 2022c).

Based on commuting patterns there are currently 70 local labour-market regions, but the long-term trend indicates a continued decrease since improvements in the transport infrastructure have gradually made it possible to commute over longer distances (e.g. Sveriges Kommuner och Landsting 2008). However, commuting long distances is still rather rare. In 2020, the average work-trip distance was circa 21 kilometres, and nearly 60 per cent of trips to work were shorter than 15 kilometres. Most workers with a commuting distance between 30 and 150 kilometres live close to larger cities, and these flows are usually directed towards larger-city regions and specifically to the three metropolitan regions (Trafikanalys 2020). Despite the proportion of workers having a long commute being low, a register-based study by Andersson, Lavesson, and Niedomysl (2018) showed that commutes longer than 100 km increased by 42 per cent between 1990 and 2009. Rural-to-urban commuting comprises the largest share of long-distance commutes. It should be noticed that the flow of long-distance commuters represents less than three per cent of the workers living in rural areas. In comparison to other countries, interregional commuting amounts to circa seven per cent of the workforce, which is substantially less than in many other European countries, including Germany, the UK, and France. However, interregional commuting in the US is less frequent than in Sweden (SOU 2022).

From a Covid-19 pandemic point of view it has been argued that working from home patterns may have seen a structural shift that potentially triggered fundamental changes in commuting patterns. Travel time is considered to be important for the willingness to accept a daily commute. Prior to the pandemic, 45 minutes was found to be a generally acceptable threshold (e.g., Huang et al. 2018; Van Ham 2001). Similar results have been reported for Sweden (Sandow and Westin 2010). To what extent the post-pandemic development has changed people's preferences for where to work, and thus their commuting behaviour, is mainly an uncharted area of research. However, in a recent report by Statistics Sweden (2022b) the results do not show much indication of large structural changes taking place in terms of working from home patterns. In the beginning of 2021, during

the pandemic, 30 per cent of employees aged 15 to 74 worked from home for at least half of their working days. In May 2022, after the removal of the Covid-19 Act on temporary infection control measures, this proportion had gone down to 17 per cent. These results show that the proportion of people working from home is decreasing, suggesting a slow process of going back to a pre-pandemic situation.

In Sweden, it is common that partners living together are not married. About one third of all couples living together are not married (Statistics Sweden 2022a). Only focusing on divorce rates is therefore misleading when studying relationship stability. In this study, we therefore include all couples living together who are married, registered partners or cohabiting with children-in-common. While Sweden has one of the highest divorce rates in Europe (EUROSTAT 2022), the majority of separating couples are not married (Statistics Sweden 2022a). Separations are more common among younger couples. At the age 25, nearly one sixth of the couples living together separates within one year, compared to 8 percent of couples in their 30s and around 1 percent of 65-year olds (Statistics Sweden 2020). While it is more common that couples with no children separate, 3.7 percent of all couples living together with children-in-common separated during 2020. This separation rate has remained stable since 2015 (Statistics Sweden 2021).

1.4 Descriptive statistics

There were 32,870 single-site firms with more than 10 employees in total. 26,099 of these did not relocate at all, 6,005 firms relocated more than 0 and less than 5km, 766 firms relocated 5–200 km. Firms that did not relocate employed 416,415 workers. Firms that relocated more than 0 and less than 5km employed 142,783 workers and firms that relocated 5–200 km employed 10,019 workers.

Only firms with no missing control variables were included in the regressions. There were 702 such firms that relocated 5–200 km in 2011 or 2012, employing 3,910 employees who meet our criteria,[12] of which 3,424 remained with the firm 3 years after the relocation and 2,197 remained 5 years after the relocation. Firms do not seem to be selective in relocating more than 5 km. Table 1.3 shows that, prior to relocation, the firms relocating further have higher mean sales but the difference is not statistically significant.

Firms relocating 5–200 km are more likely to be in a large (metropolitan) location at

12. An employee living with a partner in the same residence. Having a partner is defined as being married, having a registered partner or cohabiting with common children.

Table 1.3: Baseline firm sample comparison by relocating

	(1) Relocated 5-200 km			(2) Relocated 0-5 km			(3) Baseline difference
	Median	Mean	SE	Median	Mean	SE	Δ
Relocating (km)	11.05	22.43	30.00	0.10	0.52	0.94	-22.91***
Profit (SEK 10,000)	9.59	-57.29	2776.21	12.89	110.80	1132.20	159.81
Sales (SEK 10,000)	264.6	2566.7	30307.0	313.1	1932.9	15351.0	-420.11
Firm size	17.00	65.46	591.73	20.00	65.25	368.83	4.79
Δ Firm size	1.00	2.82	47.10	1.00	1.71	29.98	-0.79
Observations	702			5760			7823

the baseline, as Table 1.4 shows.

Table 1.4: Initial firm location by relocation distance

Initial firm location	Did not relocate	Relocated 0-5 km	Relocated 5-200 km
Large municipality	165,431	49,931	5,512
Medium/ small municipality	250,984	92,852	4,507

Table 1.5 shows the distributions of firms and employees among industries. We can see that Construction, Transportation and Storage and Administrative and Support Service industries are more predominant within firms that relocate 5–200 km.

Table 1.5: Industry distribution of firms and employees.

	All		Relocating		All		Relocating	
Industry code (SNI07)	Employees	Employees (%)	Employees	Employees (%)	Firms	%	Firms	
Agriculture, forestry and fishing	3,396	0.59	43	0.43	487	1.31	9	1.7
Mining and quarrying	837	0.14	14	0.14	68	0.18	4	0.48
Manufacturing	172,957	29.80	2,22	21.97	6,499	17.42	111	13.25
Electricity, gas, steam and air conditioning	7,5	1.29	36	0.36	221	0.59	2	0.24
Water supply; sewerage, waste management	2,747	0.47	112	1.11	215	0.58	9	1.7
Construction	47,538	8.19	1,416	14.1	4,698	12.59	161	19.21
Wholesale and retail trade; repair	83,828	14.45	1,718	17.00	7,682	20.59	139	16.59
Transportation and storage	32,418	5.59	969	9.59	2,469	6.62	91	10.86
Accommodation and food service	13,789	2.38	122	1.21	1,794	4.81	20	2.39
Information and communication	31,672	5.46	613	6.7	1,808	4.85	47	5.61
Financial and insurance activities	13,305	2.29	133	1.32	522	1.40	11	1.31
Real estate	11,521	1.99	138	1.37	811	2.17	16	1.91
Professional, scient. and techn. activities	40,764	7.2	725	7.17	2,947	7.90	66	7.88
Administrative and support services	21,177	3.65	871	8.62	1,572	4.21	63	7.52
Public administration and defence	12,158	2.10	37	0.37	241	0.65	2	0.24
Education	31,936	5.50	222	2.20	1,645	4.41	19	2.27
Human health and social work activities	31,36	5.40	564	5.58	1,46	3.91	37	4.42
Arts, entertainment and recreation	8,524	1.47	58	0.57	774	2.7	6	0.72
Other service activities	12,862	2.22	95	0.94	1,39	3.73	25	2.98
Activities of extraterritorial organizations	8	0.00	0	0	1	0.00	0	0
Total	580,297	100.00	10,106	100.00	37,304	100.00	838	100.00

There may be a selection of employees into the kind of firms that relocate. The t-test reveals a significant difference in several potentially-important variables before relocation between employees in firms that relocate shorter and longer distances (see Table 1.6).[13]

13. Employees in firms relocating longer distances are older. This suggest that more fragile younger families may have already selected out of the sample. Thus, our estimate of the effect of commuting distance could be a lower bound of the effect in the population.

Table 1.6: Baseline sample comparison by relocating

	(1) Relocated 5-200 km			(2) Relocated 0-5 km			(3) Baseline
	Median	Mean	SE	Median	Mean	SE	difference
Female	0.00	0.30	0.46	0.00	0.37	0.48	0.06***
Age	43.00	44.02	10.19	44.00	44.68	10.18	1.06***
Separated 2010-2016	0.00	0.12	0.33	0.00	0.11	0.32	-0.02***
No. children 0-5	0.00	0.50	0.73	0.00	0.47	0.72	-0.05***
Duration of partnership	12.00	12.39	7.40	13.00	13.10	7.44	0.81***
Work income (SEK 1,000)	342.30	399.76	344.73	342.30	386.67	305.33	-3.66
Disposable HH income (SEK 1,000)	552.09	639.13	470.71	551.92	633.43	669.54	8.50
P: Parental leave b. (SEK 1,000)	0.00	14.94	32.51	0.00	13.33	30.48	-1.80***
Commuting distance (CD) (km)	12.46	20.37	26.53	8.41	14.90	21.33	-13.80***
CD $\geq$ 30 km	0.00	0.19	0.39	0.00	0.12	0.32	-0.07***
CD $\geq$ 50 km	0.00	0.09	0.28	0.00	0.05	0.21	-0.04***
CD $\geq$ 100 km	0.00	0.02	0.15	0.00	0.01	0.11	-0.01***
Educational homogamy	0.00	0.49	0.50	0.00	0.50	0.50	0.01
Observations	9753			139087			300622

In comparison with Table 1.7, we see that the commuting distance increased more for employees in substantially relocating firms.[14] Still, the majority of workers have a relatively short commuting distance after the relocation (median 12.8, mean 21 km) and 81% of workers have commutes shorter than 30 km.

1.5 Identification strategy

to identify the causal effect of commuting distance, we use firm relocations. To study precisely-defined relocations, we focus on firms larger than 10 employees with one workplace. We observe workplace relocation when a workplace changes geographical coordinates and at least half the workplace's employees remain with the firm. This is in line with the definition of a workplace by Statistics Sweden. While for some employees the commuting distance has decreased as a result of workplace relocation, for others the distance has increased. However, the average commuting distance grows with average relocation distance.

Our identifying assumptions are that the relocation needs to be uncorrelated to the error term of the separation regression, and the relocation should not affect separation

14. We use substantial relocation as a shorthand for relocating 5–200 km.

Table 1.7: Year after relocation sample comparison by relocating distance

	(1) Relocated 5-200 km			(2) Relocated 0-5 km			(3) Difference
	Median	Mean	SE	Median	Mean	SE	in T_1
Female	0.00	0.31	0.46	0.00	0.35	0.48	0.08***
Age	44.00	44.58	9.85	45.00	45.34	9.87	0.13
No. children 0-5	0.00	0.51	0.75	0.00	0.45	0.72	-0.02**
Duration of partnership	13.00	13.07	7.16	15.00	13.90	7.18	0.40***
Work income (SEK 1,000)	357.20	442.03	389.33	351.80	396.53	265.91	-29.38***
Disposable HH income (SEK 1,000)	595.68	699.05	461.58	564.10	654.60	717.35	-22.28***
P: Parental leave b. (SEK 1,000)	0.00	14.06	31.59	0.00	12.56	29.49	-0.92**
Commuting distance (CD) (km)	12.78	21.04	26.16	8.70	14.99	21.26	-11.49***
CD $\geq$ 30 km	0.00	0.19	0.39	0.00	0.12	0.32	-0.07***
CD $\geq$ 50 km	0.00	0.08	0.27	0.00	0.05	0.21	-0.03***
CD $\geq$ 100 km	0.00	0.02	0.15	0.00	0.01	0.11	-0.01***
Educational homogamy	0.00	0.50	0.50	1.00	0.51	0.50	0.02***
Observations	3910			40239			298887

behavior if the commuting distance is held constant.

1.5.1 Empirical model

In common situations, separation and divorce may be endogenous to commuting distance. Therefore, in our main specification, we use only the quasi-experimental variance in the commuting distance coming from firm relocations that are more likely to be exogenous. Following Mulalic, Van Ommeren, and Pilegaard (2014) we use the first difference of a fixed-effect model:

$$sep_i = \beta_1 \Delta CD_{i,f} + \beta_2 \Delta X_{i,f} + \beta_3 Z_{i,f} + \delta_f + v_{i,f} \tag{1.1}$$

where sep_i denotes the separation of individual i, considered as a change in partnership status, Δ denotes the time-differencing operator, CD is commuting distance, X includes time-variant control variables and Z includes time-invariant control variables with a time-varying effect. δ_f denotes a firm-fixed effect. In the analytical samples of this model, we include only those who remain with the firm and do not move residence in the corresponding 3- and 5-year period.

In this setting, there is a possibility for unobserved variables at the individual level. Therefore, fixed effects are needed. We use a linear probability model with fixed effects.

Another possibility would be multinomial logit with fixed effects. We choose the linear probability model because the interpretation of coefficients is more straightforward. Odds ratios are incomparable without knowing the baseline probabilities, which the fixed-effect multinomial logit unfortunately cannot compute.[15]

We estimate commuting time as a function of work and residence coordinates using network analysis in ArcGIS, taking into account the road network, including speed limits, one-way roads, and tunnels. This allows us to run an analogous regression with estimated commuting time as the independent variable of interest:

$$sep_i = \beta_1 \Delta \hat{CT}_{i,f} + \beta_2 \Delta X_{i,f} + \beta_3 Z_{i,f} + \delta_f + v_{i,f} \tag{1.2}$$

In order to compare the coefficients of relocation-induced changes in commuting distance with other common sources of variation in commuting distance, we also estimate the following model:

$$sep_i = \sum_S \beta_1 \Delta CD_{i,f} \cdot S_{i,s} + \beta_2 \Delta X_{i,f} + \beta_3 Z_{i,f} + \delta_f + v_{i,f} \tag{1.3}$$

where S is a set of sources of variation, including relocation, job change, residential move and other. $S_{i,s}$ is a dummy variable representing the source of variation in commuting distance of individual i.

1.5.2 Checking for ex ante family stability differences

The external validity of our estimates would be low if the kind of worker who leaves a company after relocation is also the kind of person more likely to separate from a partner. To check for this possibility we calculate a family-stability ranking based on employees in non-relocating firms. First, we regress separation on control variables in a sample of workers from non-relocating firms. We then compute an out-of-sample prediction of separation for the workers in relocating companies in the baseline year. This prediction is our baseline family-stability ranking. We then look at whether this ranking predicts if workers remain in relocating firms. We find that remaining in the relocating firms is not predicted by the stability ranking, with or without using the control variables (see Tables 1.8 and 1.9).

We provide more details about the other sources of variation in Appendix 1.A. For

15. See Pforr (2014).

Table 1.8: Remaining with the firm 3 and 5 years after relocation

	(1)	(2)
	3 years after relocation	5 years after relocation
Stability ranking	0.0496	0.0682
	(0.0401)	(0.0584)
Controls	NO	NO
Firm FE	NO	NO
Municipality FE	YES	YES
Industry dummies	YES	YES
Observations	3759	2457

Standard errors in parentheses

Source: Astrid database, own calculations. Raw correlation without control variables.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.9: Remaining with the firm 3 and 5 years after relocation

	(1)	(2)
	3 years after relocation	5 years after relocation
Stability ranking	0.00152	-0.00892
	(0.00581)	(0.00929)
Controls	YES	YES
Firm FE	NO	NO
Municipality FE	YES	YES
Industry dummies	YES	YES
Observations	263722	190626

Standard errors in parentheses

Source: Astrid database, own calculations. Time-varying and fixed controls included.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

example, we find non-significant results in the probability of separation for both workers who leave a stationary firm and those who leave a relocating firm.

1.6 Main results

Table 1.10 shows the results of the main regressions. While 3 years after relocation there is a weak positive significant effect of commuting distance, after 5 years there is a stronger positive significant effect of relocation-induced change in the commuting distance on the separation of workers who remain in substantially relocating firms for at least 5 years. This effect on separation probability is significant but relatively small. Using firm fixed effects (FE) reduces the significance of the estimate. For a 10 km change in commuting distance, this translates to a 0.086 percentage point change in the probability of separation. This risk of separation is small in comparison with the average of 15 percent of couples in the sample separating over five years. This average rate of separations among the studied workers is similar to the share of separations found in Sandow (2014) study analysing all Swedish couples (11 percent). For a 50 km change in the commuting distance, the effect would be 0.43 percentage points, which is a moderate effect on family stability. Although the effect is small, these results show that when commuting distances increase due to firm-relocations, the probability of separation increases over time and with distance. An increased risk of separation with longer commuting distances is in line with results from a previous study of Swedish couples Sandow (2014). [16]

The specification with estimated commuting time had unexpected results. Table 1.11 shows effects that are 10 times smaller than the corresponding estimate for the commuting distance, and they are not statistically significant at even the 10 per cent level. These results point to limitations of using ArcGIS for calculating commuting times without recourse to traffic data.

1.6.1 Urban vs Rural areas

We also control for relocations within and between Large and Medium/Small municipalities (see Table 1.12). The first dummy refers to the direction of change of the firm location: Within Small/Medium municipalities, From Large (to Small/Medium) or Into

16. Interestingly, when we interact the commuting distance with a firm relocation-induced commute longer than 30 km, the threshold used in the literature, we find no effect of crossing a 30 km threshold on the effect of commuting distance.

Table 1.10: Changes in probability of separation 3 and 5 years after relocation

	(1) 3 years after relocation	(2) 5 years after relocation	(3) 5 years after, Firm FE
Δ commuting distance (km)	0.000377*	0.000860**	0.000980*
	(0.000211)	(0.000388)	(0.000521)
Sales (10.000SEK)	-0.000000131	0.000000529	
	(0.000000327)	(0.00000110)	
Log number of employees	0.00316	0.0108	
	(0.00783)	(0.0119)	
Δ log number of employees	0.0174	-0.0375	
	(0.0315)	(0.0392)	
Δ number of children 0-5	-0.0132	0.0159	0.0304
	(0.0126)	(0.0212)	(0.0224)
Occupation change	0.000797	0.000138	-0.00902
	(0.00841)	(0.0230)	(0.0370)
Female	0.00835	0.00686	0.00262
	(0.00804)	(0.0131)	(0.0144)
Baseline duration of partnership	-0.00158***	-0.00243***	-0.00279***
	(0.000542)	(0.000863)	(0.000949)
Educational homogamy	-0.0587***	-0.0873***	-0.0894***
	(0.00674)	(0.0109)	(0.0122)
Firm FE	NO	NO	YES
Municipality FE	YES	YES	YES
Industry dummies	YES	YES	NO
Observations	3424	2197	2197

Standard errors in parentheses

Source: Astrid database, own calculations. Time-varying and fixed controls included.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.11: Main analysis with commuting time

	3 years after relocation	5 years after relocation
Change in commuting time	-0.000323	-0.0000397
	(0.000276)	(0.000275)
Sales (10.000SEK)	-0.000000152	0.000000656
	(0.000000275)	(0.000000506)
Log number of employees	-0.0000986	-0.00362
	(0.00597)	(0.00513)
Change in log number of employees	0.0429**	0.0268
	(0.0214)	(0.0251)
Occupation change	0.0143*	0.000582
	(0.00835)	(0.00869)
Female	0.0119**	0.0169**
	(0.00569)	(0.00762)
Baseline duration of partnership	-0.000780**	-0.000892**
	(0.000384)	(0.000360)
Educational homogamy	-0.0496***	-0.0373***
	(0.00480)	(0.00500)
Firm FE	NO	NO
Municipality FE	YES	YES
Industry dummies	YES	YES
Observations	4545	3136

Standard errors in parentheses

Source: Astrid database, own calculations. Time-varying and fixed controls included.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Large municipalities. The second interacted dummy refers to change in residence (if any). Including these controls does not change the results substantively. Commuting changes within firms that stay in Medium or Small municipalities or move into large municipalities lead to even higher separation rates than firms that move out of large municipalities into smaller ones or firms that stay in large municipalities for those who stay 5 years with the relocating firm.

Table 1.12: Controlling for relocations within and between Large and Medium/Small municipalities

	Commuting distance		Commuting time	
	3 years after relocation	5 years after relocation	3 years after relocation	5 years after relocation
Change in commuting	0.000441*	0.00121***	0.0000204	-0.000119
	(0.000231)	(0.000421)	(0.000154)	(0.000202)
Out of Large	-0.0997	0	0.00291	0
× Stable Medium / Small	(0.0716)	(.)	(0.0121)	(.)
Out of Large	-0.00753	0.393**	0.00463	-0.000801
× Stable Large	(0.0177)	(0.188)	(0.0112)	(0.0212)
Stable Medium / Small	-0.132**		-0.0350	
× Out of Large	(0.0556)		(0.0644)	
Stable Medium / Small	0.0108	0.749***	0.00842	0.0915***
× Stable Medium/Small	(0.0148)	(0.0822)	(0.0112)	(0.0336)
Stable Medium / Small	-0.0213		0.0213	
× Into Large	(0.0440)		(0.0247)	
Stable Medium / Small	0.0126	0.772***	0.0175	0.112***
× Stable Large	(0.0239)	(0.0850)	(0.0184)	(0.0382)
Into Large	0.0304	0.721***	0.0147	0.110**
× Stable Medium / Small	(0.0195)	(0.0930)	(0.0153)	(0.0446)
Into Large	0.0209	0.644***	0.00483	0.0945**
× Stable Large	(0.0247)	(0.0871)	(0.0153)	(0.0408)
Stable Large	0.0227		0.00992	
× Out of Large	(0.0213)		(0.0160)	
Stable Large	0.0886	0.386**	0.00582	0.0119
× Stable Medium / Small	(0.0626)	(0.159)	(0.0190)	(0.0266)
Stable Large	0.00642	0.328**	0.00580	0.00959
× Stable Large	(0.0243)	(0.147)	(0.0187)	(0.0234)
Controls	YES	YES	YES	YES
Firm FE	NO	NO	NO	NO
Municipality FE	YES	YES	YES	YES
Industry dummies	YES	YES	YES	YES
Observations	3424	2197	3371	2132

1.6.2 Heterogeneity across groups

Table 1.13 shows heterogeneous effects across groups. Families with young children may be particularly strongly influenced by commuting distance as children require care that is typically unequally distributed if one of the partners has to commute long distances. We interact the regression with having any children and find a significant positive effect for workers with children 5 years after relocation.

Educational homogamy is a much stronger predictor of separation than relocation distance, as couples with the same education level are less likely to separate. The probability of separation for couples that have the same education level increases with relocation distance. However, this increase is smaller than the effect of educational homogamy itself.

Controlling for a partner's change of commuting distance makes the result of commuting distance non-significant, even 5 years after relocation, and smaller than in the main specification.

Controlling for a partner's working status does not change the main results. Having a working partner is associated with a greater likelihood of separation and working full-time has an even stronger association.

Working in the same workplace as a partner may change the impact of commuting distance. In our sample, 201 workers have partners in the same baseline workplace. Removing these workers does not change the main results substantively.

1.7 Robustness

1.7.1 Different treatment specifications

To test the robustness of our results to the 5 km threshold for firm relocations, we replicated our main analysis using 1 km and 10 km as alternative thresholds. Both show similar and significant results. 1–200 km relocations are more common, which brings a larger sample size, but also concerns that these data include more small changes that should not have any impact on probability of separation. 10–200 km relocations are less common but more likely to result in a substantive change in commuting distance or time. Table 1.14 shows that the main result 5 years from relocation is robust to including firms relocating by at least 1 km and at least 10 km. The effect grows with minimal relocation distance. For firms relocating by at least 10 km, there is a large protective effect of an

Table 1.13: Heterogenous effects across groups

	Commuting distance		Commuting time	
	3 years after relocation	5 years after relocation	3 years after relocation	5 years after relocation
Controlling for	0.000397*	0.000706*		
no. children 0-5	(0.000237)	(0.000387)		
Controlling for	0.000669**	0.00114**		
Educational homogamy	(0.000320)	(0.000514)		
Controlling for	0.000475	0.000585	-0.000233	-0.000876
Partner changing CD	(0.000475)	(0.00168)	(0.000371)	(0.000956)
Controlling for partner	0.000394*	0.000863**	0.0000102	-0.000138
working	(0.000214)	(0.000390)	(0.000144)	(0.000172)
Controlling for partner	0.000408*	0.000886**	0.0000135	-0.000131
working full-time	(0.000215)	(0.000386)	(0.000145)	(0.000171)
Different	0.000356	0.000883**	0.0000293	-0.000196
baseline workplace	(0.000224)	(0.000418)	(0.000147)	(0.000194)
Controls	YES	YES	YES	YES
Firm FE	NO	NO	NO	NO
Municipality FE	YES	YES	YES	YES
Industry dummies	YES	YES	YES	YES
Observations	1013	627	956	562

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

induced 30 km commute 3 years from relocation, but the commuting distance for these induced commuters has a negative effect on family stability.

Table 1.14: Is separation influenced by commuting induced by relocations longer than 1 km and longer than 10 km?

	Relocation > 1 km			Relocation > 10 km		
	3 years after relocation	5 years after relocation	5 years after Firm FE	3 years	5 years	5 years after,
Change in commuting distance	0.000203	0.000799**	0.00103**	0.000263	0.00177***	0.00143**
	(0.000192)	(0.000327)	(0.000520)	(0.000305)	(0.000646)	(0.000710)
Induced commute ≥ 30 km	-0.0192	0.0240	-0.0102	-0.0522**	-0.0189	-0.0649
	(0.0207)	(0.0271)	(0.0331)	(0.0236)	(0.0400)	(0.0442)
Induced commute ≥ 30 km ×	0.00156*	-0.00195*	-0.000817	0.00211**	-0.00149	0.00141
Change in commuting distance	(0.000907)	(0.00113)	(0.00151)	(0.00103)	(0.00234)	(0.00262)
Controls	YES	YES	YES	YES	YES	YES
Firm FE	NO	NO	YES	NO	NO	YES
Municipality FE	YES	YES	YES	YES	YES	YES
Industry dummies	YES	YES	NO	YES	YES	NO
Observations	8239	5557	5557	1748	1033	1033

Standard errors in parentheses
Source: Astrid database, own calculations. Time-varying and fixed controls included.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

1.7.2 Placebo group of firms moving 0-5 km

We also tried to run the regression for the control group of firms, which moved only 0-5 km. Workers in these firms do not face large changes in transport costs so we expect no impact of commuting distance changes on separation rates. Table 1.15 shows the results. As expected, the estimated effect is not statistically significant and 10-100 times smaller than the effect for the firms relocating by 5–200 km.

1.7.3 Alternative specifications

We also replicated the applicable robustness checks from Mulalic, Van Ommeren, and Pilegaard (2014). We ran the main regression without the commuting distance. The coefficients of the control variables did not change in economically or statistically significant ways. We also considered changes in the set of control variables. Including profit as a control variable makes the main result non-significant. Profit is a rather noisy variable, which can be influenced by an accounting strategy, and is therefore not used in our preferred specification. We ran a cubic specification and the only significant effect was a linear one 5 years after relocation, of similar size to the main regression. Rescaling the Sales variable does not change the main results substantively. A regression using the log

Table 1.15: Placebo group: 0-5km relocations

	3 years after relocation	5 years after relocation
Change in commuting distance	0.000232	0.0000129
	(0.000220)	(0.000263)
Sales in million SEK	-0.000000841	-0.000000534
	(0.000000919)	(0.00000126)
Log number of employees	0.000403	0.000910*
	(0.000365)	(0.000531)
Change in log number of employees	-0.00308	-0.00235
	(0.00265)	(0.00347)
Change in number of children 0-5	-0.00747***	0.00588**
	(0.00129)	(0.00244)
Occupation change	0.00234**	0.00202
	(0.00100)	(0.00192)
Female	-0.00633***	-0.0109***
	(0.000836)	(0.00120)
Baseline duration of partnership	-0.00182***	-0.00288***
	(0.0000584)	(0.0000906)
Educational homogamy	-0.0596***	-0.0854***
	(0.000752)	(0.00110)
Firm FE	NO	NO
Municipality FE	YES	YES
Industry dummies	YES	YES
Observations	238472	170806

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

of commuting time is weakly significant, with a magnitude and sign corresponding to the regression with distance.

Possibly the main limitation of the study is the use of a linear probability model. We also estimated logit specifications and there were positive and statistically significant results of similar magnitude to those in the main linear model. We chose the linear probability model for this study because the interpretation of coefficients is more straightforward.

1.7.4 Other sources of variation

As we can see in Table 1.16, when we contrast changes in commuting distance related to firm relocation versus other changes, the possibly more endogenous changes show the opposite correlation: people who change commuting distance for other reasons have, on average, more stable marital or cohabiting relationships. These "other" reasons include moving residence, changing jobs, and combinations of firm relocation with moving or job change. We explore these sources in more detail in Appendix 1.A.

Table 1.16: Analysis of relocation vs other sources of variation

	3 years after relocation	5 years after relocation
Other variation × Change in commuting distance	-0.000739***	-0.000618***
	(0.000171)	(0.000196)
Relocating 5 - 200 km=1 × Change in commuting distance	0.000307	0.000673*
	(0.000267)	(0.000376)
Controls	YES	YES
Firm FE	NO	NO
Municipality FE	YES	YES
Industry dummies	YES	YES
Observations	290690	224992

Standard errors in parentheses

Source: Astrid database, own calculations. Time-varying and fixed controls included.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

1.7.5 Removing top earners and part-time workers

Two groups of workers could potentially interfere with our results. First, part-time workers may be less affected by time constraints if they do not commute daily. In our data,

we cannot identify part-time workers precisely. Workers in the bottom income decile are more likely to be part-time workers. Second, workers in the top income decile are probably among the most productive employees, and could be more likely to influence the firm's relocation decision and break our identifying assumption. Removing both these groups should provide a sense of how much our estimates are sensitive to these concerns. In Table 1.17 we see the results of the analysis with a restricted sample, removing the top and bottom income deciles. The effect for the restricted sample is quantitatively similar and more significant.

Table 1.17: Removing top and bottom income deciles.

	(1)	(2)
	3 years after relocation	5 years after relocation
Change in commuting distance	0.000452*	0.00121**
	(0.000257)	(0.000515)
Controls	YES	YES
Firm FE	NO	NO
Municipality FE	YES	YES
Industry dummies	YES	YES
Observations	2815	1811

Standard errors in parentheses

Source: Astrid database, own calculations.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

1.8 Conclusion

This study analyses the effect of commuting distance and estimated commuting time on family stability using matched register data for firms and workers in Sweden. We mitigate the endogeneity of commuting and family stability by focusing on changes in commuting distance that are due to firm relocations. We show that an increase in commuting distance due to firm relocations leads to a small and significant increase in the probability of separation 5 years later. The result is robust to including relocation-related changes in commuting distance of workers who leave the company a year after the relocation and to a battery of checks in the spirit of Mulalic, Van Ommeren, and Pilegaard (2014).

Focusing on commuting time as the channel through which commuting distance may influence family stability produced mixed results. Depending on the specification, the effect of commuting time may be consistent with the result of commuting distance or may

be non-significant. Future research could potentially mitigate missing data problems by adding traffic data to the time estimation model.

When comparing our results with observational studies on commuting distance, we find an effect in the same direction as the associations found in Sandow (2014), Kley (2015), and Kley and Feldhaus (2017), but the effect is much smaller than the associations. This study implies that firm-relocation-induced changes in commuting distance have a measurable but small negative impact on family stability.

The estimated outputs can shed light on best practices at the firm and household levels. Our results suggest that at the firm level it may be beneficial to give extra support to workers whose commuting distance changes to longer commutes. This support may include psychological counseling, flexible working schedule, remote work days and similar measures. These may be particularly important for employees with small children. This support might also help with employee retention and improve their work-life balance and productivity.

At the worker household level our results suggest that a worker who values work-life balance and relationship satisfaction could consider looking for a new job if commuting distance increases in a problematic way. Short periods of long-distance commuting are not necessarily harmful to family stability, so workers could still keep their relocated job while looking for a better job in terms of location nearer to the household residence.

1.A Appendix A: More details on other sources of variation

Tables 1.18 and 1.19 show the association of commuting distance with separation for other sources of variation in commuting distance. In Table 1.18 we look at job changes in 2010 and 2011 in stationary and relocating firms. For workers changing jobs in stationary firms, family stability is correlated with several control variables but not with the change in commuting distance. For workers changing jobs while firms are relocating, family stability is not significantly associated with commuting distance.

The picture for workers who remain at their company but move their residence looks different (Table 1.19) Moving residence while working in a stationary firm is probably a choice endogenous to relationship-stability expectations.

Table 1.18: Job moves and changes in separation probability

	Job changes from stationary firms		Job changes from relocating firms	
	3 years after relocation	5 years after relocation	3 years after relocation	5 years after relocation
Job change related Δ CD	0.0000495	-0.0000615	-0.0000824	-0.000372
	(0.0000681)	(0.0000875)	(0.000212)	(0.000300)
Controls	YES	YES	YES	YES
Firm FE	NO	NO	NO	NO
Municipality FE	YES	YES	YES	YES
Industry dummies	YES	YES	YES	YES
Observations	188417	155409	395	262

Standard errors in parentheses

Source: Astrid database, own calculations.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.19: Separations for residence movers

	Residence movers in stationary firms		Residence movers in relocating firms	
	3 years after relocation	5 years after relocation	3 years after relocation	5 years after relocation
Move-induced Δ CD	-0.00120***	-0.00121***	0.00167	-0.000505
	(0.000241)	(0.000307)	(0.00205)	(0.00415)
Controls	YES	YES	YES	YES
Firm FE	NO	NO	NO	NO
Municipality FE	YES	YES	YES	YES
Industry dummies	YES	YES	YES	YES
Observations	19321	14442	395	262

Standard errors in parentheses

Source: Astrid database, own calculations.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

1.B Appendix B: Removing groups of controls

Our results are robust to removing fixed controls (gender dummy, educational homogamy and baseline length of the partnership). Using only municipality and industry dummies as controls leads to nonsignificant coefficients of similar magnitude to the main regression.

Chapter 2

Proximity to Help Matters: The Effect of Access to Centers for Legal Aid on Bankruptcy Rates

Coauthored with Štefan Domonkos (Social Policy Institute and Slovak Academy of Sciences).

2.1 Introduction

In 2017, the Slovak Republic launched a major reform of its personal bankruptcy mechanism. The new legal framework, which entered into force in March 2017, replaced a difficult-to-access and rigid personal bankruptcy program with a system that combines fast fresh-start bankruptcy with low barriers to entry. The reform increased the attractiveness of personal bankruptcy and made it significantly more available to the broad public. The new legislation also established the public Center for Legal Aid (CLA) as the institution accepting all personal bankruptcy applications and submitting them further to the relevant court. Thus, communication with the CLA became a necessary first step in personal bankruptcy proceedings.

We study the impact of spatial access to CLAs on the ratio of personal bankruptcies to the population at the municipal level. For several reasons, good access to CLAs may play an important role in the takeup of personal bankruptcy in a given region. Apart from the importance of CLAs as the key intermediary between bankruptcy applicants and courts,

financially distressed households are also spatially less mobile. We find that spatial access to CLAs has an important role in ensuring effective access to personal bankruptcy. Better spatial access to CLAs increases average bankruptcy rates in a municipality. The net impact of spatial access to CLAs shows that in municipalities with good spatial access to the CLA network (road distance from the nearest CLA below 20 kilometers [12.43 miles]), the bankruptcy rate is larger by 3.3 personal bankruptcies per 1,000 individuals than in similar municipalities that are spatially distant from CLAs (road distance from the nearest CLA more than 40 kilometers [24.85 miles]). This effect represents 52% of the nationwide average personal bankruptcy rate in Slovakia reached by December 2019. Thus, a large distance from the nearest CLA may prevent eligible debtors from filing for personal bankruptcy. Our results point to the importance of spatial access to comprehensive public services for citizens. Without spatial access, such services may remain unused by target groups. In the context of helping highly indebted households, it is critical to offer publicly-financed legal and financial counseling services in locations that are spatially close to potential clients. These services are crucial in countries such as Slovakia, whose judicial system, by 2017, had to grapple with as many as 3.6 million enforcement procedures in a nation of about 5.4 million. In the international context, the importance of accessible legal services to households in financial stress will only grow as a result of the COVID-19 pandemic. Reasons for this include the heavy toll the pandemic has exacted on low-wage sectors, such as the hospitality industry, as well as the limitations on mobility imposed on the population during periods of more serious coronavirus outbreaks.

2.1.1 Importance of distance to services

Studies show that distance to public services can be a crucial factor determining their use. Nguyen (2000) and Alessandrini, Presbitero, and Zazzaro (2009) link credit access to the distance to bank branches where the credit allowance decisions are made. Nguyen (2000) finds that bank branch closings lead to a persistent decline in local small business lending, which persists for up to six years. Alessandrini, Presbitero, and Zazzaro (2009) find that the distance to decision-making centers matters in the supply of credit for small firms. Enflo and Karlsson (2018) consider the distance from a mediator as an instrument for mediation in work conflicts. They find that the involvement of a mediator in a conflict resulted in a higher probability of compromise. They also find that mediation was more

likely to occur when strikes took place in municipalities with a mediator among the residents.

2.1.2 Bankruptcy

While lawmakers in the US treat bankruptcy as a means to resolve business failure, legislators in Europe view it more through the lens of social policy. In line with this, most of the recent European personal-bankruptcy reforms have been introduced in reaction to recessions (Niemi-Kiesiläinen 1999). Alleweldt et al. (2013) and Eurofound (2020) monitor over-indebtedness in the European Union and state policies intended to alleviate this problem. Slovakia, together with several of its neighbors from the ranks of Post-Socialist EU member states, has a relatively low proportion of individuals at risk of over-indebtedness in its population. Nevertheless, this figure has been growing, and during times of economic turmoil (e.g. the 2008-2009 financial crisis) reached levels exceeding the EU average (see Alleweldt et al. 2013, p. 44). Increasing the accessibility of personal bankruptcy options has therefore become a popular policy option implemented in a varied group of European countries.[1]

Personal bankruptcy may be thought of as a social insurance program (Dobbie and Song 2015), whose costs are manifested in the form of a higher equilibrium interest rate. This interest rate reflects the increased risk of debtor default in a lenient personal-bankruptcy legal framework.[2] Keeping such *ex-ante* effects of personal-bankruptcy policies fixed, many studies, e.g. recent ones by Dobbie and Song (2015) and Dobbie, Goldsmith-Pinkham, and Yang (2017), consider the impact of bankruptcy on microeconomic outcomes. Dobbie and Song (2015) and Dobbie, Goldsmith-Pinkham, and Yang (2017) use a compelling identification strategy with random judge assignment to Chapter 13 (repayment plan) bankruptcies in the US. Dobbie and Song (2015) find a positive impact of bankruptcy on debtors' welfare: the enforcement of money judgments through

1. Kilborn (2018) lists Slovakia's case as a prime example of lawmakers introducing a personal-bankruptcy system more accessible to debtors and less concerned with possible abuses by bankruptcy filers. Instances of similar policy changes include the 2015 Polish reform and the 2017 Austrian law amendments that tackled high administrative costs and addressed the restrictive requirement to produce a minimum dividend within the bankruptcy proceedings.

2. Taking into account that policies allowing debtors to default more easily will translate into higher borrowing costs, such policies cannot be qualified as debtor-friendly in general. The Slovak case is different in that it extended the possibility to default also to debtors who accrued their debts *before* the introduction of the new personal-bankruptcy legislation. However, the loan terms agreed upon before late 2016, when the legislative reform was concluded, could not have reflected the changed risk of borrower default under a more accessible personal bankruptcy framework.

deducting payments from the debtor's wage (wage garnishment) instead of seizing other property and debt forgiveness increase annual earnings, reduce five-year mortality and reduce five-year foreclosure rates. Dobbie, Goldsmith-Pinkham, and Yang (2017) consider the impact of bankruptcy on the financial situation of the debtors and find that Chapter 13 bankruptcy protection reduces financial strain, improves credit scores, increases the probability of being a homeowner, and reduces debt in collection. While this methodology is well suited to study repayment plan bankruptcies, it cannot be used to study the more frequent bankruptcy by asset liquidation, as virtually all valid filings of these bankruptcies are accepted by U.S. courts.

2.2 The Slovak institutional setting

For the purposes of this analysis, we define personal bankruptcy as debt liquidation for a natural person (consumer). Since March 2017, personal bankruptcy in Slovakia can be implemented either by asset liquidation and protection from further wage garnishment or by setting up a repayment plan. These two forms of bankruptcies are akin to the US Chapter 7 and Chapter 13 proceedings, respectively. Although the legal framework for personal bankruptcy has formally existed in Slovakia since 2005, this legal instrument only became significantly more accessible to the majority of indebted natural persons only after its reform came into effect in March 2017 (Kilborn 2018; Oršula 2019; Spectator 2017).[3] The new legislation allowed people with no-income-no-asset (NINA) to undergo bankruptcy (Oršula 2019). In addition to the repayment plan, bankruptcy through liquidation was introduced, where debts are satisfied only with money obtained from a quick sale of the debtor's current assets. Bankruptcy through asset liquidation, which quickly became the preferred form of insolvency, is thus a typical example of the so-called fresh-start bankruptcy known from Anglo-Saxon countries. Access to bankruptcy has also been improved through curbing entry costs and providing a loan to cover these expenditures to those debtors who have filed for bankruptcy through asset liquidation.

Thanks to the relatively low costs and the removal of legislative barriers, entering the personal bankruptcy process in Slovakia depends mainly on the debtor's decision. The essential condition that every personal bankruptcy filer must fulfill is at least one

3. See the original law at https://www.slov-lex.sk/pravne-predpisy/SK/ZZ/2005/7/20161115.html and the 2017 amended version at https://www.slov-lex.sk/pravne-predpisy/SK/ZZ/2005/7/20170301.html.

foreclosure pending against them for more than a year. The bankruptcy petition must be filed through the Center for Legal Aid (CLA), a network of public legal aid offices. The CLA is thus the mandatory first stop in the personal bankruptcy process.[4]

As a result of the 2017 reform, the number of personal bankruptcies has skyrocketed in Slovakia. From March 2017 to July 2021, nearly 50 thousand personal bankruptcies were initiated according to the public Register of Bankruptcy Filers.[5] Of this pipeline, the courts have managed to complete 42,800 cases as of July 2021. By comparison, in the five years immediately preceding the reform (2012-2016), the number of bankruptcy petitions handled by the courts amounted to 548 (Analytical Center of the Ministry of Justice of the Slovak Republic [AC MS SR], Analytical Center 2016). Thus, an unprecedentedly large number of individuals underwent bankruptcy between March 2017 and July 2021. This wave of personal bankruptcies took place in a short period characterized by stable macroeconomic conditions until March 2020, when the break caused by the onset of the COVID-19 pandemic occurred. Figure 2.1 below provides an overview of the evolution of the number of personal bankruptcies over the last decade based on the data presented in the statistical yearbooks of the Ministry of Justice (Analytical Center 2016, 2017, 2018, 2019, 2020).

Using microdata on personal bankruptcies from March 2017 to December 2019, our study evaluates the extent to which the use of personal bankruptcy is conditional on physical proximity to the CLA. The experiment in a broader sense is the unexpected change in bankruptcy law in Slovakia in tandem with the CLA suddenly gaining a new function. The CLAs were concerned with legal aid only and did not function as the necessary first instance in bankruptcy cases prior to the law change in 2017. Thus, while the subsequent expansion had to consider distressed individuals considering personal bankruptcy, the initial location was based on other use cases. While debt legal counseling was involved, personal bankruptcy did not dominate the workload of the CLAs. The experiment in a narrower sense is the difference between the expected distance to the nearest CLA based on a host of available socio-economic and geographic variables and the actual distance to the nearest CLA. Controlling for the expected distance, we look at differences in bankruptcy rates in municipalities that are near and far from the CLA.

4. The number of persons who consider personal bankruptcy to deal with their debts in collection is difficult to estimate. However, a good indication of the severity of the problem is the fact that, as of March 2017, there were 3.6 million pending foreclosures in Slovakia, and 474,000 foreclosures were initiated in 2016 alone (Paller 2017).

5. ru.justice.sk

We assume that the remaining variation in access to the CLA is as good as random.

The bankruptcy rate in municipalities located closer to the CLA was higher, even after controlling for relevant socioeconomic and demographic characteristics. After controlling for other relevant variables, the number of personal bankruptcies filed between March 2017 and December 2019 in municipalities close to CLA offices or counseling centers was higher by approximately 3.3 bankruptcies per 1,000 inhabitants than in municipalities farther from the CLA network.

This net effect of CLA proximity on population bankruptcy rates is both substantively and statistically significant and highlights the importance of CLA accessibility to residents' effective access to personal bankruptcy. Our results show that the physical proximity of public financial counseling services to the population is an essential condition for their use by insolvent citizens. This conclusion also aligns with the existing international literature, which has paid little attention to Central and Eastern Europe so far.

A natural way to look at bankruptcy rates would be bankruptcy per number of distressed individuals. Unfortunately, we do not have access to a direct measure of distress cases among natural persons. Our analysis relies on the assumption that in socioeconomically similar municipalities with similar expected distance to CLAs, the per-capita number of distress cases among natural persons will be similar. Importantly, the indicators used in the comparison of municipalities are, among others, the per-capita number of debtors on taxes and social-security contributions, the per-capita number of distressed households estimated from the 2019 Household Finance and Consumption Survey (HFCS, Household Finance and Consumption Network 2020) and the per-capita number of judicial enforcement procedures in 2020. We also include the proportion of marginalized Roma communities where 27.6 percent of inhabitants experienced debt enforcement procedures, and 13 percent of inhabitants experienced debt enforcement multiple times (Grauzelová and Markovič 2018, page 66). While these indicators are useful in describing the financial situation of the population, neither of them gives us the number of distress cases *per se*. It is also important to note that the 3.6 million enforcement procedures in 2017 do not translate directly into 3.6 million cases of distressed natural persons who should (or could) undergo personal bankruptcy. Many enforcement procedures are related to legal persons; one legal or natural person can have numerous enforcements and not all enforcements concern large financial amounts that would warrant personal bankruptcy.

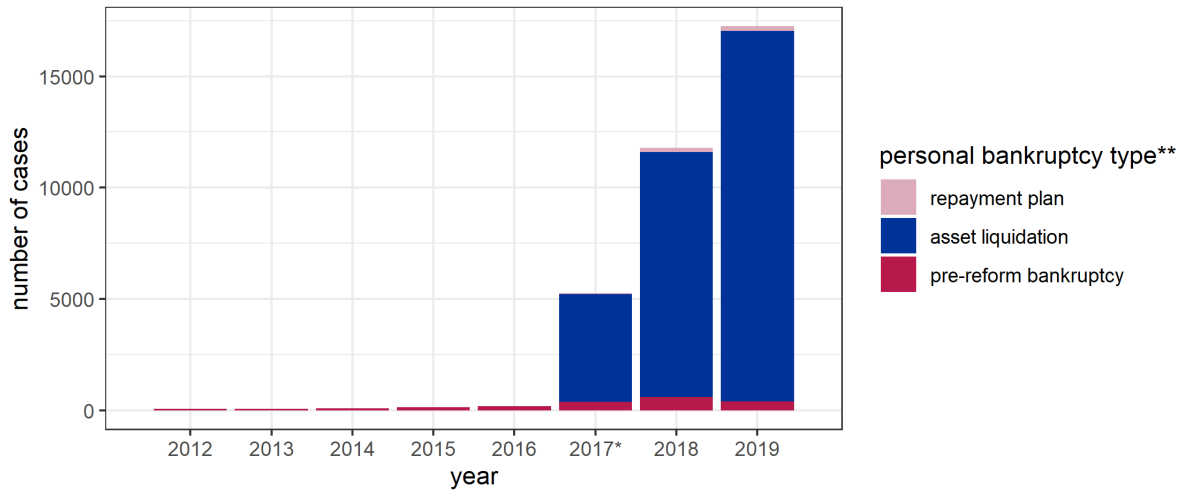

Figure 2.1: Number of personal bankruptcies (cases), 2012-2019

2.2.1 The Slovak personal-bankruptcy landscape

The adoption of the sweeping personal-bankruptcy reform, which came into force in March 2017, was primarily motivated by a desire to address excessive debt as a serious social problem (Ministry of Justice SR 2016). That legislation was adopted at a time when, according to available data, there were 3.6 million debt collection procedures pending in Slovakia, with nearly half a million new procedures being initiated annually (Paller 2017). Parallel to this development, there was also a gradual increase in the number of debt collection procedures against particularly vulnerable groups of the population, such as pensioners (Social Insurance Agency 2016). Making fresh-start personal bankruptcy broadly available could thus have contributed to solving the economic and social problems of the population and reducing the burden on Slovak courts. The map in Figure 2.2 shows the geographical distribution of the bankruptcy rate at the district level from the introduction of the personal bankruptcy reform in March 2017 to the beginning of the COVID-19 pandemic in March 2020. The most important cluster of districts significantly affected by bankruptcies is in the Banská Bystrica region (e.g. districts of Žiar nad Hronom, Banská Štiavnica, Veľký Krtíš, Poltár, Rimavská Sobota, Lučenec). The districts of Svidník, Stropkov, and Medzilaborce in the northeast of the country also showed significantly above-average bankruptcy rates. Conversely, below-average bankruptcy rates are characteristic of several districts in the west of Slovakia (e.g., Senec, Pezinok, Dunajská Streda, Piešťany, Skalica, Malacky and Bratislava districts) and selected districts in the north of the Slovak Republic (Námestovo and Tvrdošín districts).

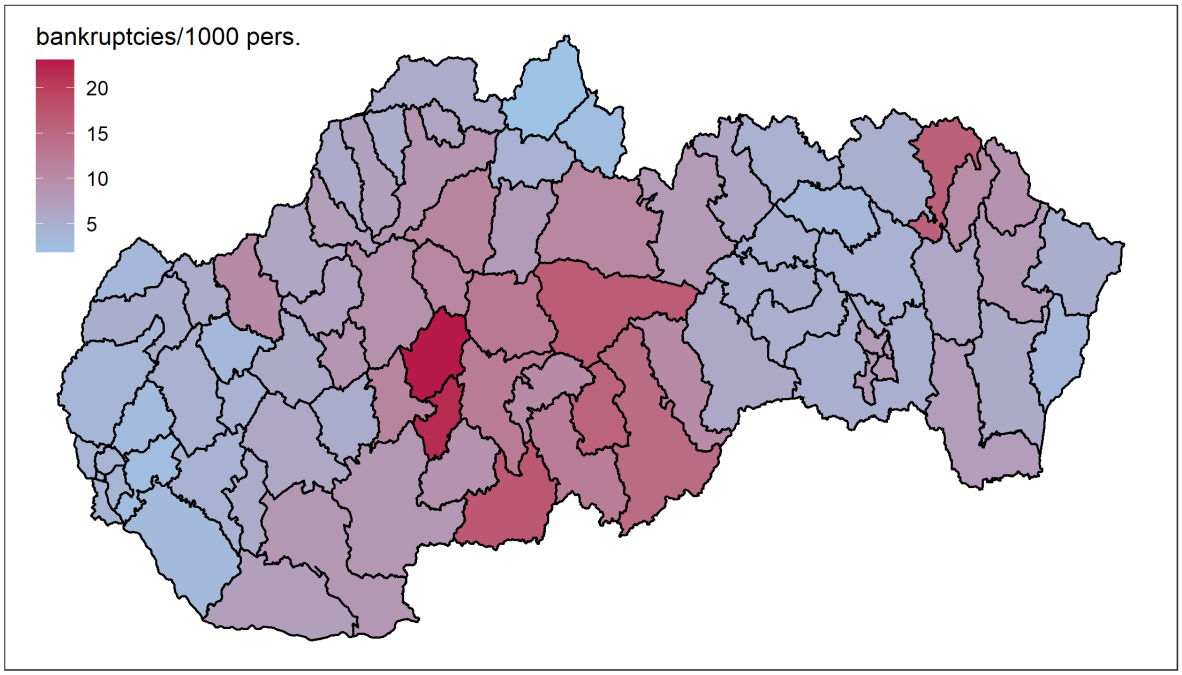

Figure 2.2: Number of personal bankruptcies per 1,000 inhabitants in districts of the Slovak Republic (March 2017-March 2020).
Source: Authors' elaboration based on micro-data on bankruptcies from the Register of Bankrupts (ru.justice.sk).

Note: We evaluate the districts Bratislava I to Bratislava V and Košice I to Košice IV as one geographical unit. This explorative analysis shows that personal bankruptcy is more frequently used in economically less developed regions of Slovakia. In the twelve Least Developed Districts (LDDs), which obtained the status of LDDs at the turn of 2015 and 2016, the number of personal bankruptcies per 1,000 inhabitants at the end of March 2020 was 9.23.[6] The national average achieved by March 2020 was 7.0 cases per 1,000 inhabitants, while this indicator averaged 6.15 in the other 67 districts. Thus, as of March 2020, the bankruptcy rate was approximately 50% higher in the twelve original LDDs than it was in the rest of the country. The difference in the frequency of personal bankruptcies as a proportion of the population is also present when taking into account data from all twenty districts that had LDD status in 2020. However, the difference is more moderate (7.71 personal bankruptcies per 1,000 inhabitants compared to 6.2 in the other 59 districts of the Slovak Republic). These results align with the expectation that

6. The Slovak LDDs constitute a group of economically less developed NUTS4 regions that have consistently suffered from an unemployment rate exceeding the national average by at least 50%. Most of these regions are in Central and Northeastern Slovakia. By contrast, the West of the country, especially the vicinity of the capital city Bratislava, enjoys a more buoyant labor market with higher wages and a significantly lower unemployment rate.

those impacted by debt distress are likely to be residents of regions with long-term high unemployment.

Building on the above analysis, Sections 2.2.2 and 2.3 address two questions: (i) What is the distance of municipalities with different levels of unemployment and bankruptcy from the nearest CLA? and (ii) What is the statistical relationship between the prevalence of personal bankruptcies in Slovak municipalities and the geographic proximity of CLAs?

2.2.2 The geographical spread of bankruptcies and the CLA network

Better accessibility to CLAs due to their geographical proximity may play an important role in the prevalence of personal bankruptcy in specific municipalities and regions in Slovakia for several reasons. As mentioned in the introduction, CLAs are the only organization that can file a personal bankruptcy petition with the court on behalf of the bankruptcy filer. At the same time, the bankruptcy paperwork requires personal contact with CLA staff, as the papers must include an overview of the filer's total assets and all debts. If the CLA also provides a loan to cover the advance on the bankruptcy trustee's fees, the provision requires the filer to sign an agreement; which is included in the paperwork. For these reasons, it is virtually impossible to substitute another form of contact for a physical visit to the CLA during the personal bankruptcy process.

In addition to the aforementioned institutional specificities, the likely lower mobility of bankruptcy filers also plays an important role. Bankruptcy filers have a higher risk of material deprivation and economic inactivity than the general population. Low socioeconomic status and low disposable income are, in turn, significant barriers to accessing transport services (e.g., Hine and Mitchell 2001; Roorda et al. 2010; Titheridge et al. 2014; Lucas et al. 2016). The association between low socio-economic status and low mobility has also been confirmed by empirical research based on data collected in Slovakia (Kováč and Hlavatý 2020).[7] Another critical factor is the frequency and quality of transportation connections between materially deprived communities and cities with CLA offices and consultation sites.

Given their small size, Slovak municipalities are an appropriate geographical unit to use in our analysis. The average Slovak municipality has just below 1,900 inhabitants,

7. Moreover, the literature on the use of financial services and its dependence on geographic proximity to prospective clients gives further underpinning for the main hypothesis of this study (e.g., Witte et al. 2015)

while the median municipality has less than 700 inhabitants. In terms of surface, the average Slovak municipality is smaller than 17 square kilometers [6.56 square miles] and thus is among the smallest in the EU (OECD 2018). Moreover, the centroids of these municipalities have been determined using input from the users of the OpenStreetMap database (OpenStreetMap 2022), which generally allows a more useful placement of the centroids in the built-up areas of the municipalities.

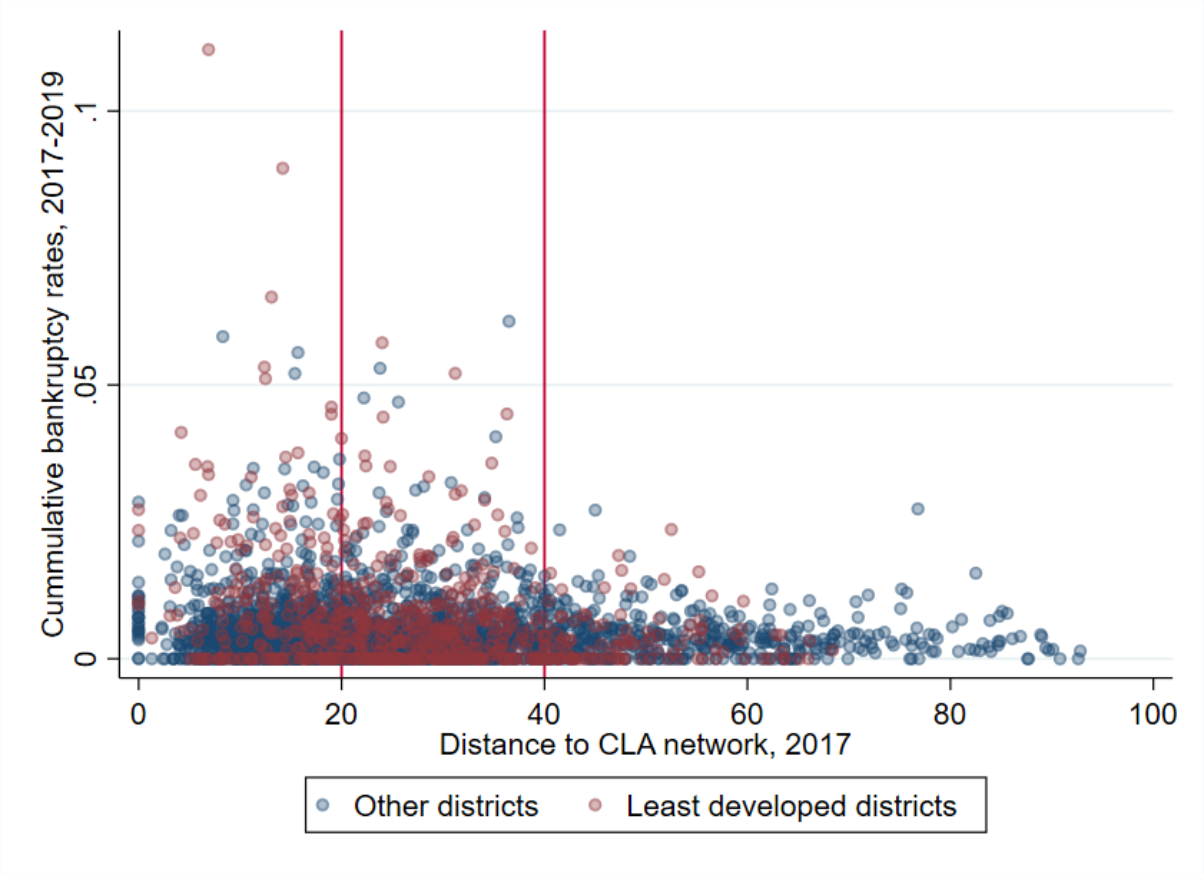

Figure 2.3: Access to CLA in municipalities, 2019 (km)
Source: Authors' elaboration based on data on the location of CLA offices and sites from the CLA Directorate and data from the Ministry of Transport's Institute of Transport Policy (IDP) on the inter-municipality road distance.

A visual analysis of the relationship between the proximity of the CLA institutional network, the level of economic development of the (micro)region, and the prevalence of personal bankruptcies among the population suggests that some municipalities that are in the least developed regions of the Slovak Republic and at the same time were geographically close to the CLA network at the beginning of the period of observation (first quarter of 2017) form small clusters of high bankruptcy rates (see Figure 2.3). However, the statistical significance of the association between the availability of CLA

services and the number of bankruptcies is not identifiable by visual inspection of the data alone. The possible presence of this relationship requires deeper analytical investigation.

As a first insight, we checked the results of a linear regression of bankruptcy rates in a municipality using distance to CLA in kilometers and the control variables reported in Table 2.1. The distance coefficient corresponds to 0.7 additional bankruptcies per 10 km [6.21 mi] of distance to the nearest relevant CLA workplace per 1,000 inhabitants of the municipality ($p<0.001$). Some of the circumstances specific to Slovakia also allow for a more detailed analysis, as individual municipalities in this country vary considerably in their distance to the nearest CLA office or consultation site. This holds true even among municipalities located in the same broader region, whose economic profile (e.g., unemployment rate) is comparable.[8]

2.2.3 Distance to the CLA

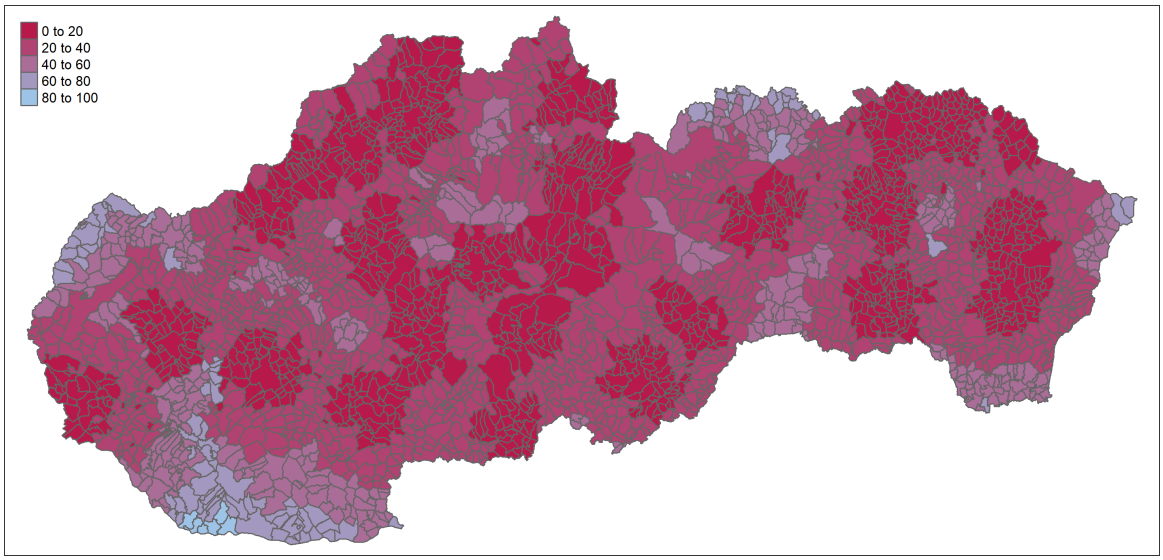

Figure 2.4: Regional distribution of access to CLA in 2017 [km of road distance]

The extent of the CLA network was characterized by relative stability over time (see Figures 2.4 and 2.5), allowing the relationship between the geographic availability of the CLA and population bankruptcy to be sufficiently reflected in the data. There was no expansion of the CLA network until October 2018. Subsequently, in mid-October and

8. As shown in Figure 2.4, a fitting example of the heterogeneity of municipalities in the distance to the CLA at the beginning of our observation period (March 2017) are the Dunajská Streda, Komárno, and Galanta districts located in Southwestern Slovakia. There are also significant differences between municipalities in the Kežmarok, Levoča, and Stara Ľubovňa districts in the Northeast, as well as in the districts of Košice-Okolie, Rožňava, and Revúca in the Southeast.

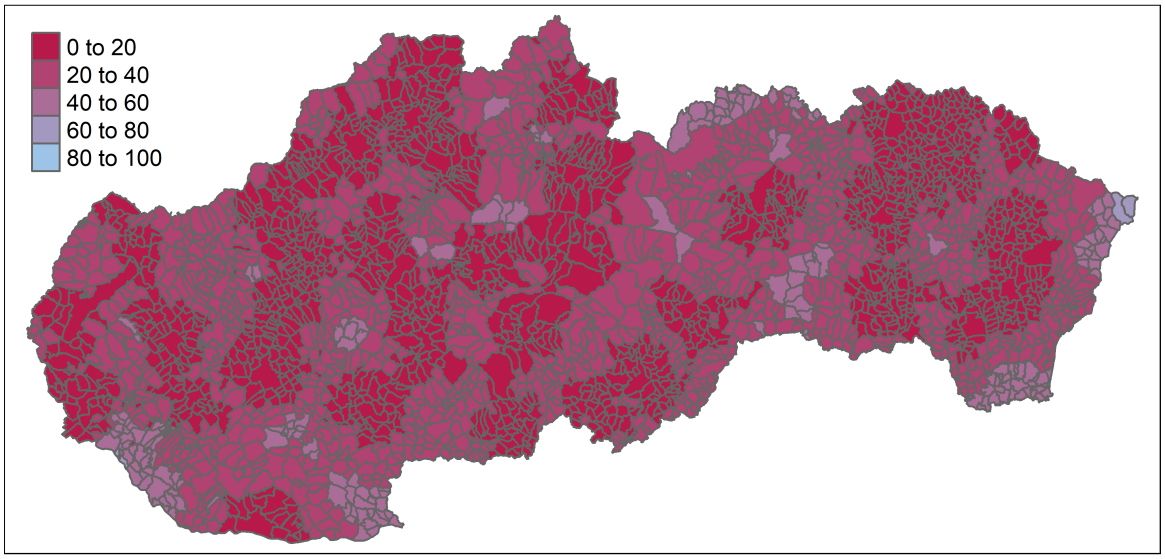

Figure 2.5: Regional distribution of access to CLA in 2019 [km of road distance]

November 2018, the CLA network was expanded with consultation offices in Fiľakovo and Giraltovce and CLA offices in Trnava and Tvrdošín. Later in 2019, the CLA network was further expanded by 13 additional units (see Appendix 2.B for more details). The expansion of the CLA network resulted in a gradual decrease in the distance of Slovakia's municipalities from the nearest CLA, which was most significant in Western Slovakia. As shown in Figure 2.5, at the end of the CLA network expansion process, there remains less diversity in the distance of villages from the nearest CLA unit or office.

Suppose geographic proximity to a CLA is an essential prerequisite for adequate access to personal bankruptcy for residents. In that case, municipalities close to the CLA network will have higher rates of population bankruptcy, even after controlling for the socioeconomic characteristics of towns and villages. At the same time, we can expect most of this difference in bankruptcy rates among the population of municipalities to arise between March 2017 and the end of 2018, when differences in geographic proximity to CLA offices and sites were most pronounced.

For our assessment of the relationship between CLA distance and the prevalence of personal bankruptcy in Slovak municipalities, we created two groups of municipalities for each month: We consider municipalities close to the CLA network to be those municipalities whose road distance to the nearest CLA consultation center or CLA office under

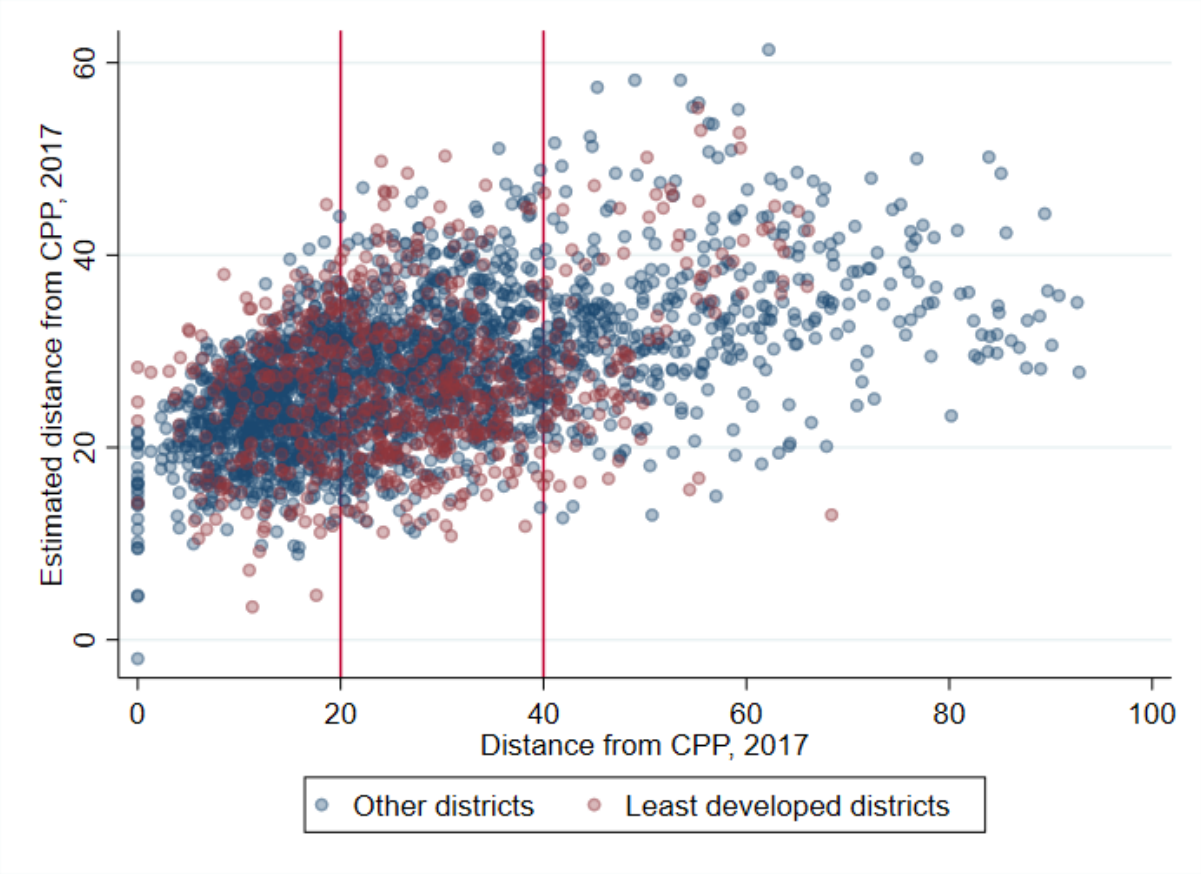

Figure 2.6: Distance to CLA and predicted distance to CLA
Source: Authors' own creation based on data on the location of CLA offices and consultation sites from the CLA Directorate, data from the Institute of Transport Policy of the Slovak Ministry of Transportation (IDP) on inter-municipal road distance, data from the Bankruptcy Registry (ru.justice.sk) and the list of LDDs.

which the municipality falls was less than 20 km in a given month.[9] Municipalities remote from the CLA network are those whose road distance from the nearest CLA consultation center or CLA office under which the municipality falls was more than 40 km in a given month.

Table 2.1 shows that more than one-third of municipalities (1,122) were closer than 20 km to a CLA in January 2017. Less than one-fifth of Slovak municipalities (525) were further than 40 km from the CLA. Later, due to the expansion of the CLA network, by December 2019, more than half of the villages (1,593) were closer than 20 km from the

9. This 20-kilometer-threshold is based on data suggesting that average commuters using bicycles as a means of transportation can overcome such a distance in one hour. In effect, according to STRAVA's 2021 'Year in Sport' statistical report, their average bicycle user has overcome 26.3 kilometers in just below 78 minutes. This translates to an average speed of 20.2 kilometers per hour (STRAVA 2021). Nevertheless, as part of our robustness analysis, we have tested our results using a variety of thresholds both smaller and larger than 20 kilometers.

	January 2017			December 2019		
	Averages		Difference	Averages		Difference
Variable	Far	Near	Near-Far	Far	Near	Near-Far
Distance from CLA	54,517	12,810	-41,707***	46,507	12,574	-33,933***
	(12.359)	(4.773)	(0.426)	(5,705)	(4,854)	(0,424)
Job seeker ratio	0.057	0.066	0.009***	0.076	0.062	-0.013***
	(0.043)	(0.048)	(0.009)	(0.049)	(0.047)	(0.004)
Education: high school and lower	1 696,557	2 229,604	533,047*	1 162,615	2 140,881	978,266*
	(4 024,99)	(6 737,03)	(318,230)	(1 741,34)	(6 292,64)	(519,23)
Tax debtors	0,005	0,005	-0,001***	0,005	0,005	0,000
	(0,004)	(0,004)	(0,000)	(0,004)	(0,004)	(0,000)
Public health insurance debtors	0,023	0,019	-0,004***	0,021	0,020	-0,001
	(0,013)	(0,011)	(0,001)	(0,011)	(0,011)	(0,001)
Public social insurance debtors	0,040	0,035	-0,005**	0,043	0,035	-0,008**
	(0,054)	(0,043)	(0,002)	(0,076)	(0,038)	(0,004)
Financial risk (HFCS)	0,044	0,043	-0,000	0,047	0,043	-0,004***
	(0,006)	(0,007)	(0,000)	(0,007)	(0,007)	(0,001)
Population rate of enforcement procedures, 2020	0,074	0,064	-0,010***	0,082	0,065	-0,017***
	(0,068)	(0,063)	(0,003)	(0,079)	(0,061)	(0,005)
Rate of marginalized Roma communities	4,967	8,772	3,806***	5,581	7,945	2,364
	(13,32)	(18,88)	(0,92)	(12,73)	(17,75)	(1,49)
Material need, promile 2017	25,539	25,847	0,308	41,506	24,364	-17,142***
	(26,620)	(31,487)	(1,591)	(31,436)	(29,604)	(2,559)
Logarithm of population, 2017	6,682	6,676	-0,006	6,394	6,677	0,283***
	(1,111)	(1,240)	(0,064)	(1,105)	(1,209)	(0,103)
Municipal neighbors	5,613	5,916	0,303***	5,365	5,924	0,560***
	(2,027)	(2,186)	(0,113)	(2,051)	(2,172)	(0,186)
Elevation (m)	261,256	325,923	64,667***	274,223	301,586	27,363**
	(183,661)	(157,872)	(8,833)	(229,895)	(153,485)	(13,878)
Travel time to the capital (min)	145,594	186,498	40,904***	197,047	171,924	-25,123***
	(103,181)	(96,552)	(5,235)	(117,499)	(97,648)	(8,554)
Travel time to regional capital (min)	58,649	48,051	-10,597***	77,628	46,760	-30,868***
	(25,773)	(29,444)	(1,502)	(25,959)	(26,916)	(2,307)
Travel time to district capital (min)	22,038	14,295	-7,743***	33,074	14,541	-18,533***
	(12,526)	(6,220)	(0,464)	(14,153)	(6,450)	(0,638)
Median wage (EUR)	613,452	592,558	-20,895***	556,436	608,124	51,688***
	(140,212)	(131,623)	(7,129)	(115,625)	(131,323)	(11,182)
Number of municipalities	524	1 107	2 887	148	1 576	2 887

Table 2.1: Columns 2 and 5 (*Far*) consist of averages of variables (standard errors in parentheses) for municipalities that were at a given time at least 40 km from the nearest CLA consultation center, or CLA office with jurisdiction over the municipality (further in the text as distance from the CLA network). Columns 3 and 6 (*Near*) include averages of variables (standard errors in parentheses) for municipalities that were at a given time less than 20 km from the CLA network. Column 4 and 7 (*Difference Far-Near*) include the difference between Columns *Far* and *Near* for samples in a given year. Data on public health insurance debtors includes records from the publicly owned health insurance provider *VŠZP* covering approximately 60 % of the entire health insurance market. Sources: Elaboration of the authors based on the analytic dataset (see Table 2 and Data section for sources).

CLA, and only five percent of Slovakia's villages (148) were further than 40 km from the CLA. Municipalities with better spatial access in January 2017 have, on average, fewer tax and social-security debtors and lower foreclosure rates. However, they are farther from the capital city and have almost twice the proportion of marginalized Roma communities, are located at a higher elevation, and are closer to regional and district towns. Compared to municipalities with worse access, municipalities with better access at the

end of 2019 have more inhabitants with less education than high school, more social insurance debtors, and a higher risk of illiquidity. However, they have fewer residents in material need and are closer to the national capital city, as well as the county and district capitals. The change in the sign of the difference in distance from the capital confirms that the new CLA workplaces that have been built since the last quarter of 2018 have been built mainly in Western Slovakia, which initially had a shortage of CLAs in 2017. It is possible that the CLA network developed until 2017 was optimally spaced given the demand for free legal advisory services, while the 2019 distribution is more responsive to the demand for personal bankruptcy, which also exists in the West of the country. Given these considerable differences, which change over time as CLA sites are added, it would be risky to draw conclusions from a simple comparison of the number of bankruptcies among residents of communities that are distant or close to CLAs. Instead, we evaluate the impact of initial CLA distance on the average number of personal bankruptcies per 1,000 municipal residents. At the same time, we use a statistical approach that allows other socio-economic and geographic characteristics of a given municipality to be considered in the comparison process. We use the characteristics above to estimate the expected distance to the CLAs. Figure 2.6 shows the correlation of actual and predicted distance to CLAs. Subsequently, this expected distance is used as a control variable in evaluating the impact of the actual distance of municipalities from the CLA on bankruptcy. This proposed method mitigates the risk that the difference in bankruptcy rates of the population of different municipalities stems from circumstances other than the distance to the CLA itself.

Our analysis will concentrate on the cumulative difference in bankruptcy rates between the municipalities that were (i) close to the CLA network and (ii) far from the CLA network as of early 2017. Given that changes in the distance of municipalities to the CLA only occurred in the last third of the 2017-2019 observation period, the initial distance of municipalities to the CLA is a suitable proxy variable describing proximity to the CLA network.

2.3 Methods

The experiment that we consider in a broader sense is the unexpected reform of bankruptcy law in Slovakia together with the CLA suddenly gaining a new function. The CLAs were concerned with legal aid and did not function as the necessary first instance in

bankruptcy cases prior to the law change in 2017. Thus, while the subsequent expansion had to consider financially distressed individuals considering personal bankruptcy, the initial location was based on other use cases. While debt legal counseling was involved, personal bankruptcy did not dominate the workload of the CLAs. The experiment that we consider in a narrower sense is the difference between the expected distance to the CLA based on a host of available socio-economic and geographic variables and the actual distance to the CLA. Controlling for the expected distance, we look at differences in bankruptcy rates in municipalities that are near to and far from the CLA. We assume that the remaining variation in access to the CLA is as good as random.

Our empirical strategy is based on the importance of the CLA as a gateway to personal bankruptcy. Given that the Slovak legislation has introduced mandatory debtor representation by the CLA, the proximity of the CLA is also an important prerequisite for accessing personal bankruptcy itself.

Methodologically, we draw inspiration from the econometric work of Powell (1987), which describes the properties of the methodology used, such as its $\sqrt{n}$-consistency and possibility to use a semiparametric regression for the estimation. The method allows us to aggregate the control variables into an index that expresses the expected distance from the CLA, thus avoiding the "curse of dimensionality" in accounting for a multitude of variables.[10]

We use semiparametric regression in Stata as described in Verardi and Debarsy (2012). We first estimate the expected distance from the CLA location with the following equation:

$$distance_i = \beta X_i + \epsilon_i \tag{2.1}$$

In the above equation, X includes the following variables:

- **municipality population ratios**: low-skilled workforce, share of job seekers, debtors of the Slovak fiscal authority (*Finančné riaditeľstvo Slovenskej Republiky* - FR SR), public social insurance agency (*Sociálna poisťovňa* - SP), and the publicly owned health insurance company (*Všeobecná zdravotná poisťovňa* - VŠZP), low liquidity risk estimated from the HFCS survey, individuals in financial distress in 2020, marginalized Roma settlements, and recipients of Benefit in Material Need[11]

10. We also considered OLS regression with polynomial distance, but the Runge phenomenon (see, e.g., Cheney and Light 2000) would compromise the accuracy of the results. OLS with linear distance from CLA has qualitatively and order-of-magnitude-wise comparable results to our methodology.

11. This benefit provides minimum financial aid to households in poverty. Approximately 2.6% of the

- **other variables**: log(Population); number of neighbour municipalities; geographical elevation of the municipality; travel time to the nearest regional, district and capital city; median wage.

These variables were chosen to represent the socio-economic and geographic situation in the municipalities that could plausibly influence the decision about the location of CLAs.

We then estimate the impact of the initial distance to the closest CLA location on the bankruptcy rate, estimating the following equation:[12]

$$bankruptcy\ rate_{it} = f(\widehat{distance}(X_i)) + \gamma_t access_i + \nu \tag{2.2}$$

The predicted distance to the CLA is represented by $\widehat{distance}(X_i)$. We allow the functional form of the influence of the predicted distance to be flexible, using the semi-parametric regression. Index i stands for the municipality and index t refers to the months from Jan 2017- Dec 2019. $Bankruptcy\ rate_{it}$ refers to cumulative bankruptcy rates from Jan 2017 to a given month. To show the results, we plot coefficients γ_t and their respective confidence intervals in graphs to show the evolution of the cumulative effect of initial CLA access on bankruptcies. In our main specification, we define $access_i$ as 1 if the CLA is closer than 20 km and 0 if the CLA is farther than 40 km.[13] Other definitions of access that we substitute in robustness sections include minutes of minimal travel time by public transport and distance to the CLA in km. In the robustness section, we report tests with several different distance threshold combinations.

In order to compute the confidence intervals of the effect, we use bootstrapping. We bootstrap the whole 2-step process 100 times. Using this procedure, we calculate three kinds of confidence intervals: bias corrected, percentile and normal distribution based confidence intervals. These intervals do not change the qualitative results and therefore in this paper we show only the bias corrected confidence intervals.

2.4 Data

To study the incidence of bankruptcies, we use various administrative, commercial and publicly available datasets. We aggregate bankruptcy counts from the publicly avail-

Slovak population received financial assistance from this program in 2019.

12. We use semipar function in Stata 14 to estimate the equation.

13. For the remaining municipalities, this indicator is undefined.

able bankruptcy registry at ru.justice.sk. The data consist of 38,101 individual personal bankruptcies from March 2017 until March 2020. The total number of cases in the register for this period is 38,360 cases. In 259 cases (about 0.86% of all observations), we could not identify one of the key variables. 65 records were of duplicated persons. Including them only once with the earliest date of bankruptcy recorded does not influence our results.

The main explanatory variable, distance between a given municipality's central point (see subsection 3.2) and the nearest CLA, is calculated using data from the Institute of Transportation Policy of the Ministry of Transport and Construction of the Slovak Republic.

We combine data from the Center for Legal Aid with administrative unemployment and social benefit data and data from the Slovak Statistical Office from the 2011 census.[14] We construct social benefits data as follows: The proportion of Benefit in Material Need (*Dávka v hmotnej núdzi*, BMN) recipients equals the number of BMN recipients divided by municipal population. We construct the job seeker ratio as the number of registered unemployed divided by the working age population of the municipality.

The share of low-skilled on the population is calculated as the proportion of job seekers with schooling lower or equal to high school. Tax debtors, Public health insurance debtors and Public social insurance debtors have been obtained from the database of the Slovak Credit Bureau CRIF. Municipality-level data on the municipal number of enforcement procedures in 2020 has been obtained from the Chamber of judicial officers of the Slovak Republic, marginalized Roma settlements from the Atlas of Roma communities,[15] and material need recipients are taken from the administrative data of the Central Office for Labor, Social Affairs and Family (*Ústredie práce, sociálnych vecí a rodiny*). Population, the number of neighbouring municipalities, elevation of the municipality, travel time to the nearest regional, district and capital city and municipality type are provided by Vladimir Bačík. Detailed information on these data can be found in Bačík (2010, 2012) and Bačík and Klobučník (2015). The median wage is aggregated from the administrative data of the Social Insurance Agency. Low liquidity risk is estimated from the HFCS survey[16] in the following way: We regress the indicator of illiquidity of a household

14. Hlavac, Marek. 2016. "Census 2011 in the Slovak Republic - Data Set." Slovak Data Project, 9 August 2016.

15. For more information on the Atlas of Roma communities, see the webpage of the Ministry of Interior (https://minv.sk/?atlas-romskych-komunit-2019).

16. For more information about the HFCS survey, see Household Finance and Consumption Network 2020.

on household size (population is used as municipal equivalent), number of members in age groups 0-34, 35-44, 45-54, 55-64, more than 65, number of pensioners, number of unemployed and number of females. The coefficients from this regression were used with the respective census variables to calculate the estimated number of risky households for each municipality. This number was divided by population to calculate the estimated ratio of risky households.[17]

2.5 Results

Let us first look at the association between CLA availability and bankruptcy rates. This association does not consider the ways in which CLA location may correlate with regional poverty. The red line in Figure 2.7 shows a crude relationship between the trajectory of cumulative bankruptcy rates and CLA availability during 36 months after the reform of bankruptcy law. The graph compares "good access" municipalities closer than 20 km from the nearest relevant CLA location with "worse access" municipalities further than 40 km from the nearest CLA. The comparison shows a substantial difference in bankruptcy rates associated with access to CLA. The difference emerges after a short period of adjustment. Towards the end of the studied period, about two years after the reform, the municipalities with better access to CLA faced 2.8 bankruptcies more per 1,000 individuals. This is a large difference in comparison to the country average of 6.3 bankruptcies per 1,000 persons reached by December 2019. The major CLA expansion in February 2019 seems to correlate with reductions in the slope of cumulative bankruptcy rate differences.[18]

Next, we show the results adjusted for the expected distance to CLA in kilometers. These results show the impact of CLA availability on top of the socio-economic and geographic factors that we used to predict CLA distance. The blue line in Figure 2.7 shows the estimate of the net effect of CLA access on bankruptcy prevalence in municipalities. When we account for a non-parametric function of the expected distance from CLA based on a range of socio-economic variables, CLA access shows a positive effect on the bankruptcy rate. Before the reform started to be implemented, there is no difference in bankruptcy rates based on CLA access, because the total number of bankruptcies was

17. Using logit or linear regression to calculate this control variable does not change the results of our analysis.

18. Timeline of the expansions is summarized in Appendix 2.B.

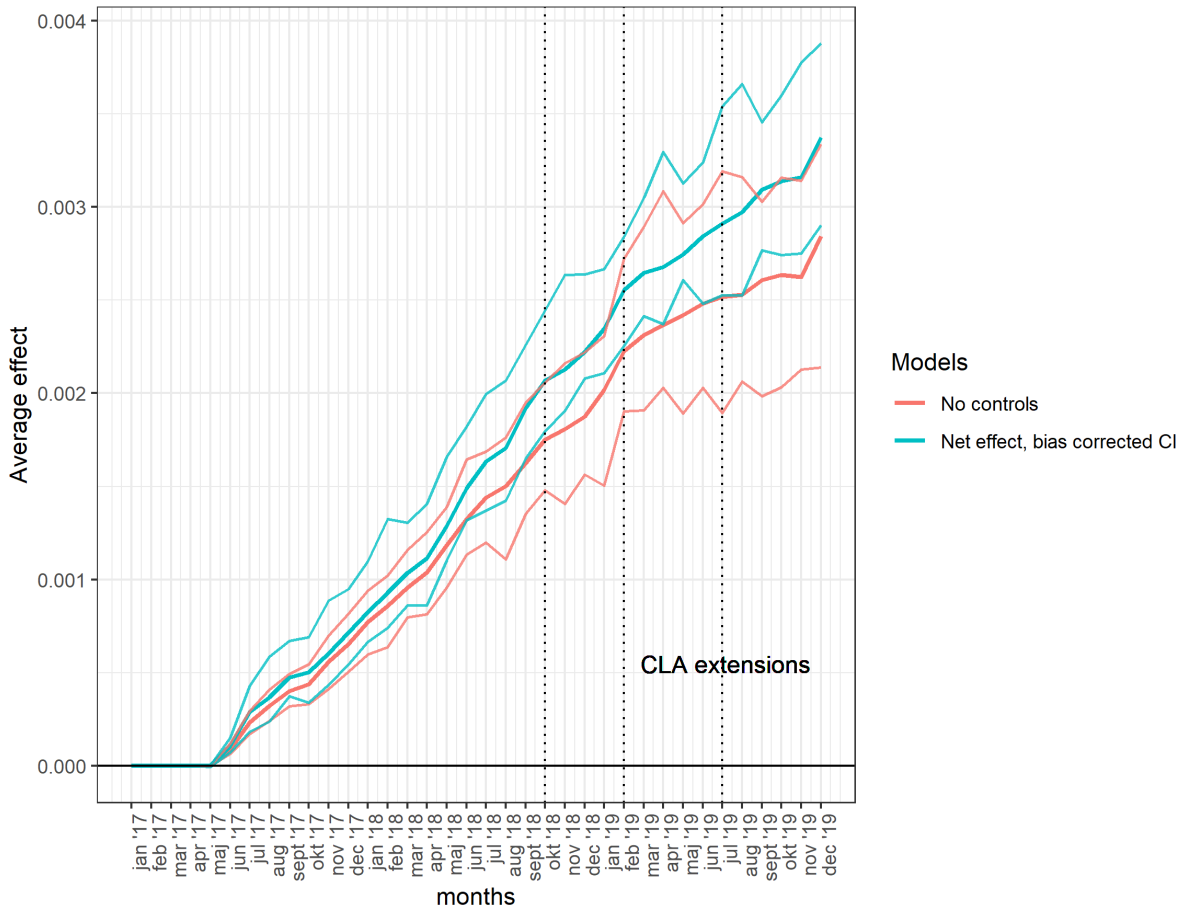

Figure 2.7: Correlation and net effect of better access to CLA (20 km vs 40 km) with cumulative bankruptcy rates

negligible. Toward the end of the studied period, at the end of 2019, we estimate the cumulative effect of CLA access on bankruptcies at 3.3 bankruptcies per 1,000 individuals. This effect is both economically and statistically significant at the level of 0.05. The effect is 52 percent of the state mean of 6.3 bankruptcies per 1,000 individuals.[19] The thick line in Figure 2.8 shows the mean effect estimate while the thinner lines correspond to the different 95% confidence intervals generated by bootstrapping.

The economic significance of the effect can also be illustrated by the following exercise: If we imagine that in 2017 all the municipalities with "worse access" would have better access instead, due to some consultation centers being opened, the estimate of total additional bankruptcies would be around 3,000 bankruptcies in the period of 2017-2019.

These results suggest a possible unrealized demand for bankruptcies from June 2017

19. The association between CLA availability and bankruptcy rates is biased downwards 15% from the improved estimate.

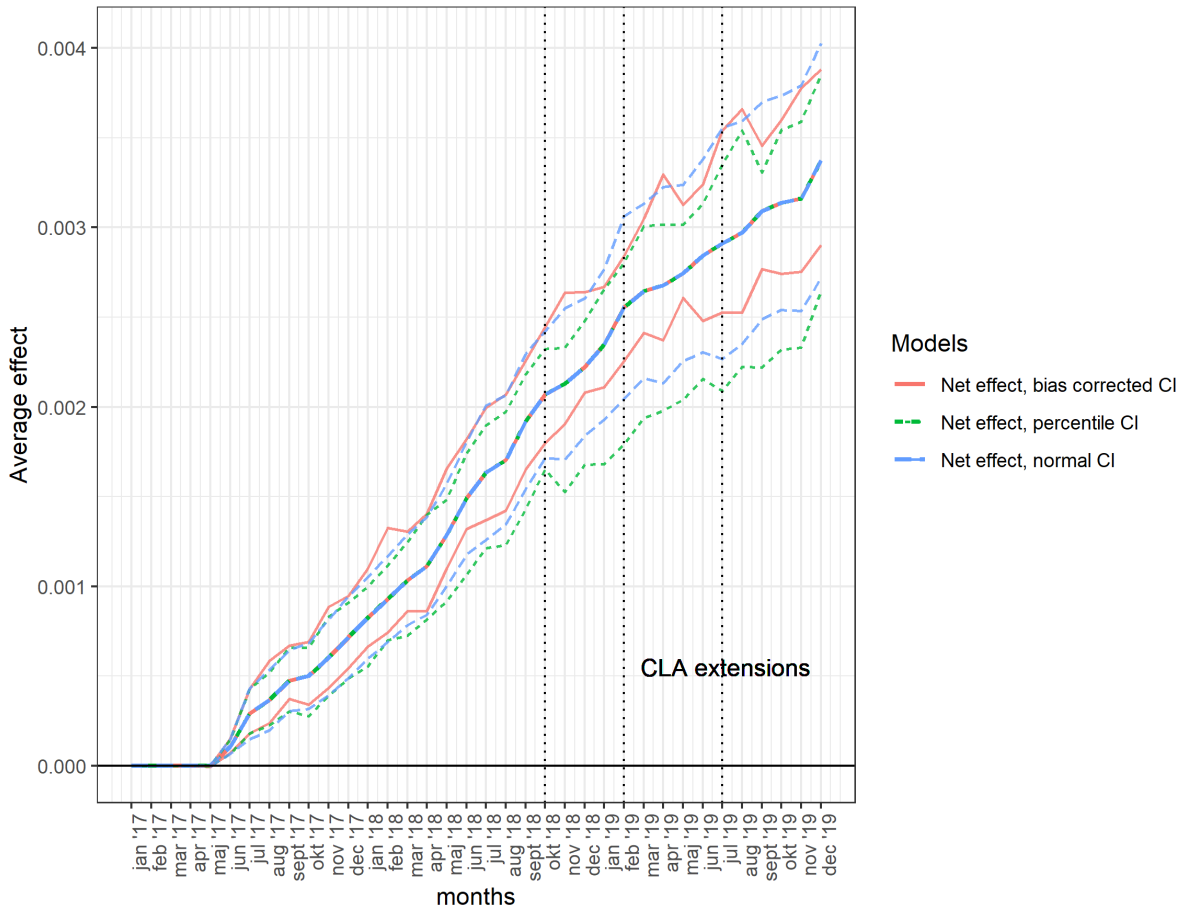

Figure 2.8: Correlation and net effect of better access to CLA (20 km vs 40 km) with cumulative bankruptcy rates

until January 2019 in municipalities with worse CLA access when the cumulative differences between good and worse access municipalities were growing.

2.5.1 Distance from the CLA expressed as a continuous variable in kilometers

We also looked at the impact of accessibility using distance in kilometers expressed as a continuous variable, i.e. without creating a near and far group of villages. We control for the expected distance in kilometers. The advantage of this approach is that we use all municipalities for the purpose of the analysis. We find an increasing effect of initial distance from the CLA on total bankruptcy rates. At the end of the period (December 2019), the difference amounts to 0.7 bankruptcies per 1,000 inhabitants for every 10 km of distance of the municipality from the CLA. As can be seen in Figure 2.9, this

is a statistically significant effect. When converted to the interval used in the main part of the analysis, this result would indicate a difference of 1.4 bankruptcies per 1,000 inhabitants. This difference is smaller, which is understandable since such an analysis includes municipalities with very small differences in CLA availability, which reduces the estimate.

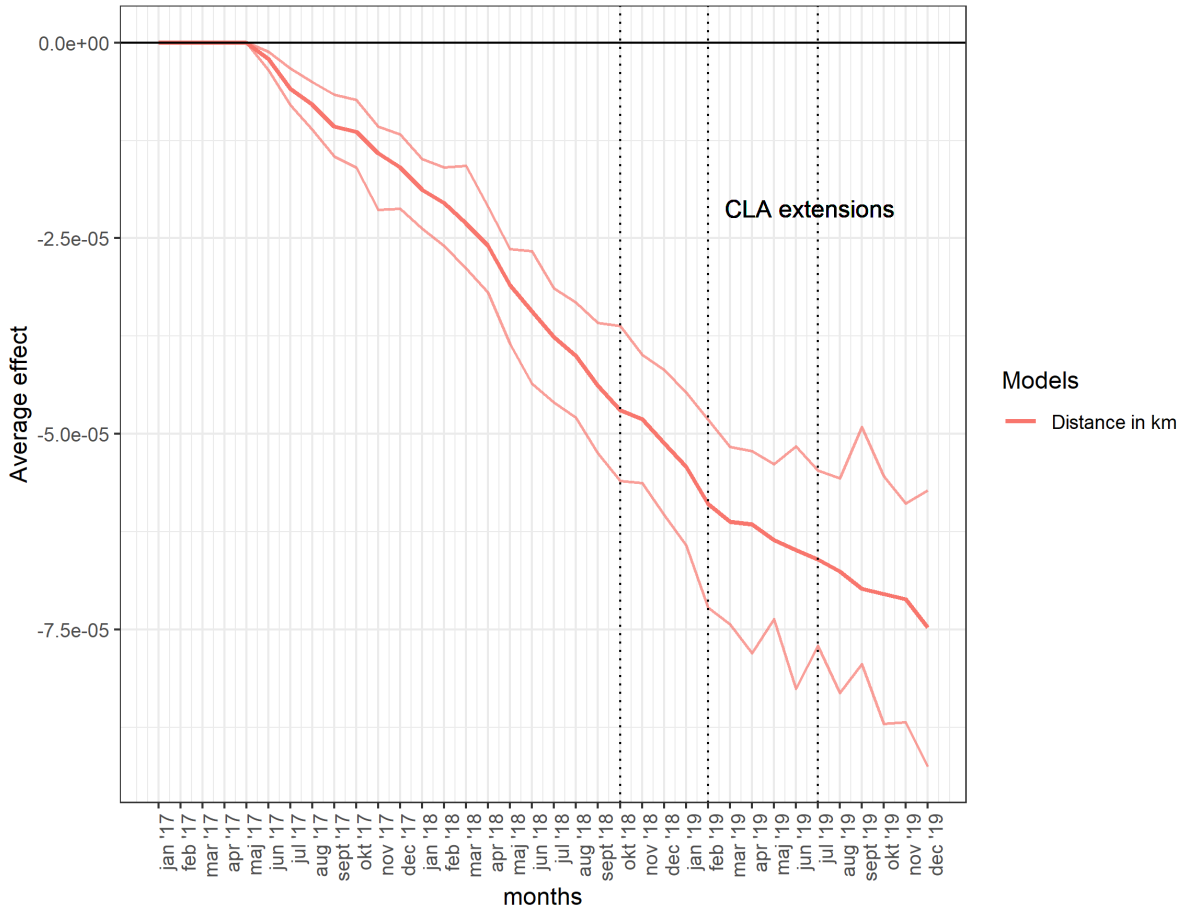

Figure 2.9: Net effect of distance to CLA (in km) on cumulative bankruptcy rates in municipalities

Our results are robust to specification changes including various definitions of CLA access (other road distance thresholds, access by public transport and distance in kilometers), different samples selected for analysis, and are also confirmed by a placebo check. The next section shows these robustness results.

2.6 Robustness

2.6.1 Alternative access specifications

Our definition of access is based on a choice of access thresholds. In the following section, we show that the results from July 2017 until December 2019 are robust to changes in this definition. Our main results are robust to comparisons of 10 vs 40 km, 30 vs 40 km, 20 vs 30 km and 20 vs 50 km, as shown in Figure 2.10. When comparing the effect sizes, their confidence intervals largely overlap, with the exception of the closer than 20 km vs farther than 30 km.

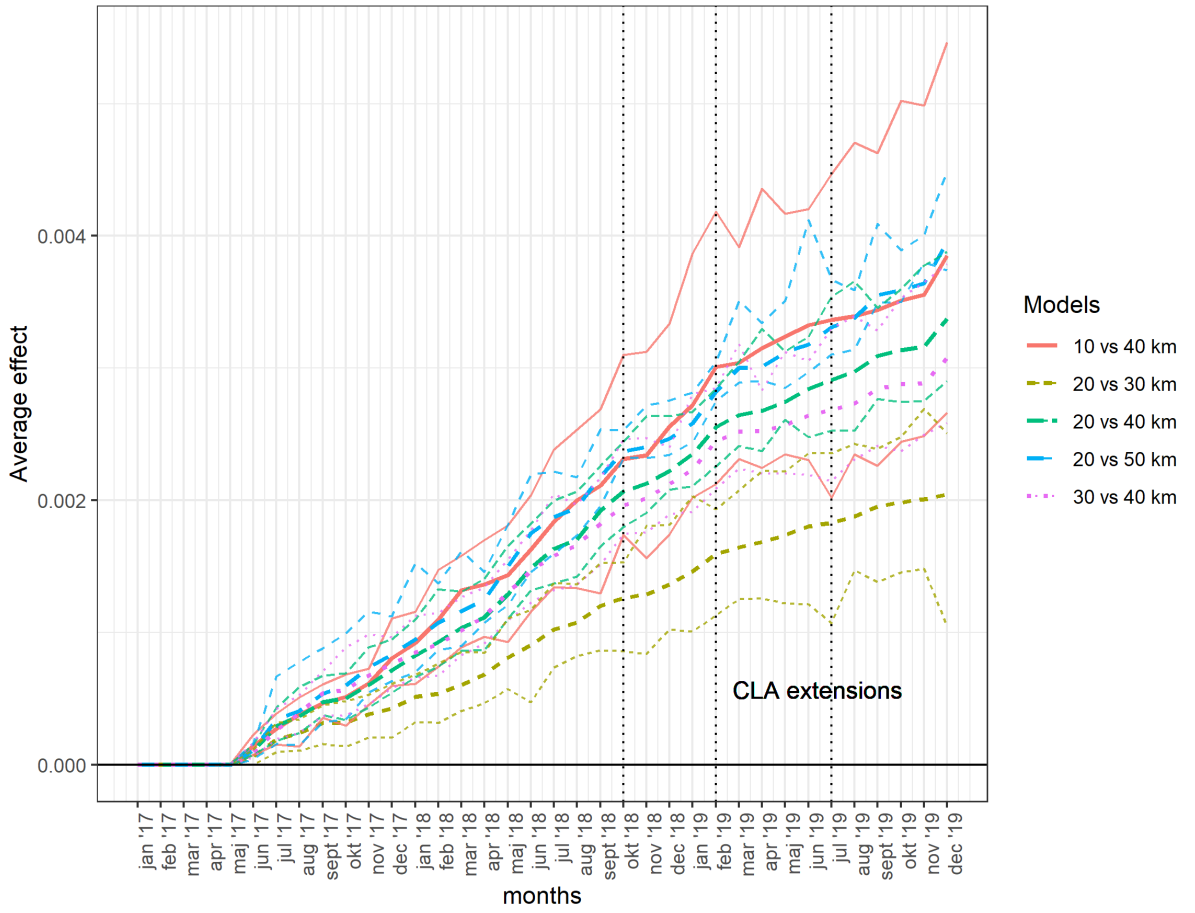

Figure 2.10: Net effect of better access to CLA (different thresholds) on cumulative bankruptcy rates

Furthermore, we obtained matrices of minimal and mean travel times with the use of public transport. Our main result of a substantial and statistically significant effect holds using good access defined as less than 20 minutes of minimal travel time and worse access as more than 40 minutes of minimal travel time. Figure 2.11 shows the evolution of the

differences in cummulative bankruptcy rates. Minimal travel time shows a large average effect of access to CLA with public transport. When traveling to CLA, which is only necessary once to sign the documents, it is likely tthat the fastest available connection will be used, represented by minimal travel time. Mean travel time may be less relevant, which explains the lower effect estimate and a lower bound of the confidence interval touching the zero effect line.

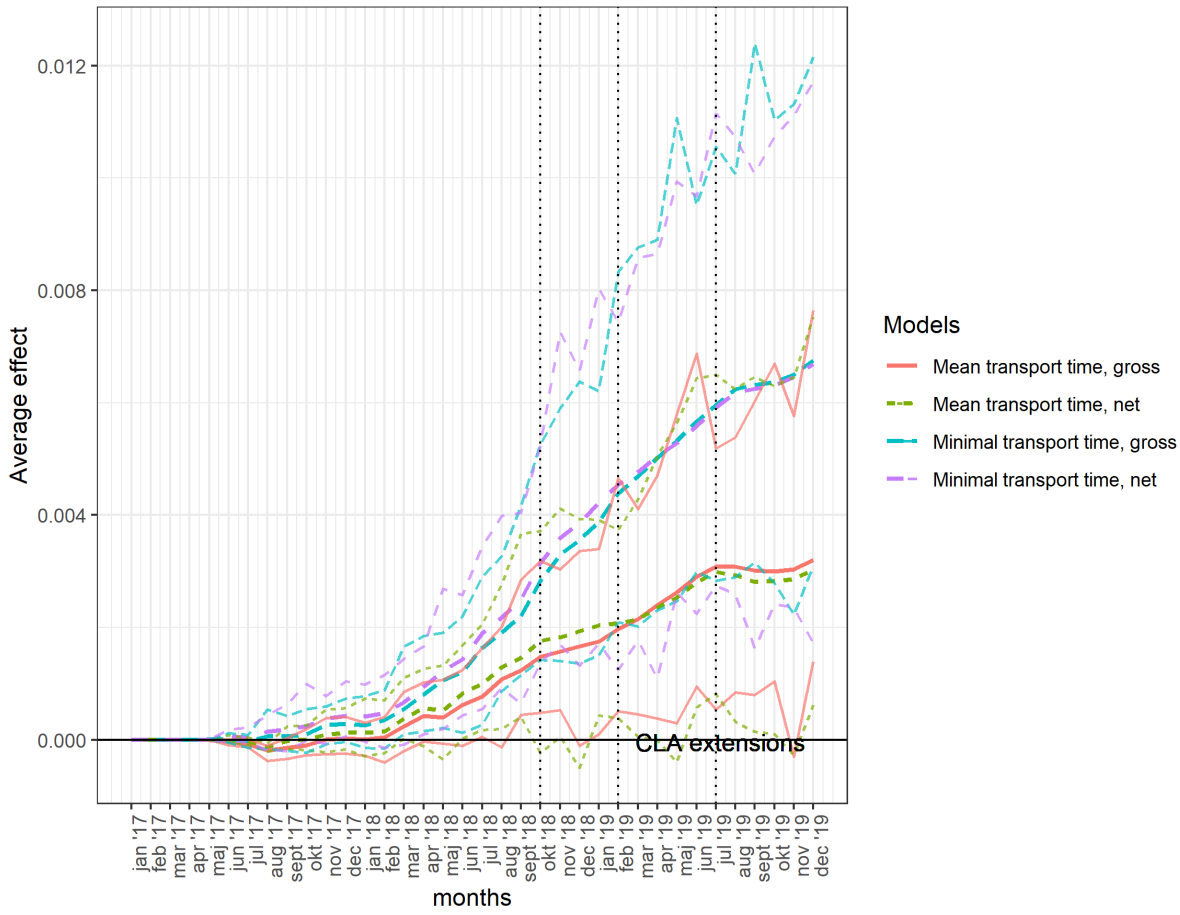

Figure 2.11: Net effect of better access to CLA (20 min vs 40 min travel time using public transport) on cumulative bankruptcy rates

2.6.2 Robustness to sample selection

Figure 2.12 compares the main result with different sample selections and a placebo estimate.

Removing municipalities farther than 60 km from the CLA

Our findings could potentially be driven by outlier municipalities. To mitigate this concern, we restrict the sample to municipalities that are closer than 60 km to the nearest relevant CLA. The resulting mean and confidence interval shown in Figure 2.12 is very similar to the main specification.

Removing municipalities with changes in access

One may be concerned about the changes in sample composition as more and more municipalities improve their access to the CLA. We run the same regressions with a smaller sample of municipalities with no change in access. Results are shown in Figure 2.12. Our main result of persistent impact of access on bankruptcies holds in this sample. However, the effect size in this sample is smaller and the confidence intervals are wider. While we assume the initial CLA placement to be exogenous conditional on the expected distance, the subsequent CLA extensions probably concentrated in municipalities that were farther from the CLA at the beginning. Thus this sample has a lower variability in CLA distances.

Placebo check: random assignment of CLA locations

Fewer municipalities remain without access to CLAs every year[20] and one may be concerned as to whether small sample bias could be driving some of our results. We decided to test whether our results could be replicated with virtual placebo CLAs, built with a random probability each month. Each month we randomly improve CLA access in each municipality with poor access with a probability 0.01. Figure 2.12 shows the results of our exercise. The placebo results are a magnitude smaller than the main results and not statistically significant.

2.7 Conclusions

The geographical proximity of the nearest CLA is an important prerequisite for the effective availability of filing for personal bankruptcy. In communities that were already close to the CLA network in early 2017, the number of personal bankruptcies per capita was

20. In January 2017, there were 472 municipalities farther than 40 km from the CLA. In December 2019, only 187 municipalities remained farther than 40 km.

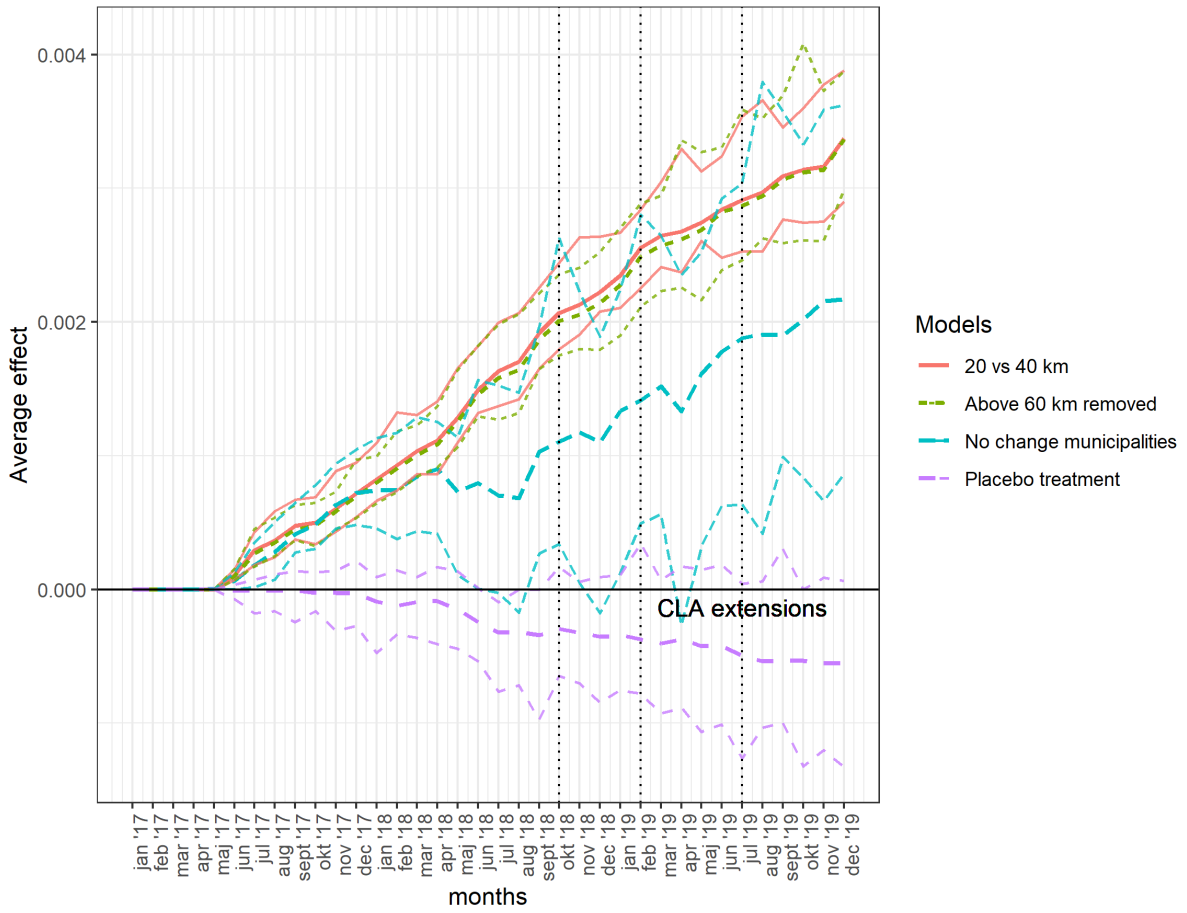

Figure 2.12: Robustness checks

higher during most of the period from March 2017 to December 2019 than in communities located far from the CLA network. The difference between municipalities that had good access to the CLA network and municipalities far from the CLA network peaked at 2.8 bankruptcies per 1,000 residents in December 2019.

Even after the gradual expansion of the CLA network, differences in the cumulative bankruptcy rate of the population persist, which is consistent with the argument about the importance of geographic proximity to the CLA. At the end of December 2019, the net effect of initial CLA availability on the cumulative bankruptcy rate was 3.3 bankruptcies per 1,000 residents. This estimate of the impact of CLA proximity on bankruptcy represents the so-called net effect that we obtained after accounting for social and economic differences between the municipalities compared.

However, it is possible that our results represent a phase-in effect of the reform because the sample of the filers included many who accumulated debts a long time before the personal bankruptcy reform. For these debtors, the possibility of formal bankruptcy

probably did not enter the interest rate calculation. As the legal institution of personal bankruptcy and new locations of CLA become more widely spread, we expect the impact of the initial distance to the CLA on cumulative bankruptcy rates to gradually weaken.

A simple estimation based on these results shows that there would have been approximately three thousand more personal bankruptcies over the 2017-2019 period in municipalities that were far from the CLA network (distance from the CLA above 40 km) if the accessibility of CLA offices and sites had been significantly better (distance from the CLA below 20 km). This estimate does not include the potential scope to meet the unrealized demand of the population for personal bankruptcy even in those villages that were more than 20 km but less than 40 km away from the nearest CLA.

The positive effect of geographic proximity to CLAs on the availability of personal bankruptcy is present even though CLAs provide information both by phone and e-mail. Thus, our findings suggest that for comprehensive counseling services for citizens, the physical accessibility of counseling centers is a prerequisite for the effective availability of such a service to the target group. In the Slovak context, the network of Labor, Social Affairs and Family Offices, which, thanks to their mission, naturally come into contact with people with financial difficulties, can make a significant contribution to increasing the availability of debt counselling to indebted households. The foundations for the inclusion of employment offices in the provision of similar types of services to the public were laid with the launch of the National Programme 'Free Debt Counselling' (Ministry of Labour, Social Affairs and Family SR 2021), which aims to intervene at a stage before the debtor is threatened with bankruptcy. We recommend that debt counselling, in an appropriate scope and content, should become part of the services offered by the Labor offices.

While our research was limited to a period preceding the COVID-19 pandemic, the lessons drawn are highly relevant for the international community of policy-makers seeking to avert its negative social consequences. This crisis not only increased the financial vulnerability of large sections of the global population, but one of the key strategies used to mitigate its effect includes limiting the mobility of the public. Given these circumstances, the importance of providing accessible legal services to vulnerable population groups will only grow in the future.

Future research on bankruptcy could be improved by making data on CLA outreach activities among the local population more accessible. Data on CLA activity may play an important role in determining the extent to which their services are used, but at the

moment, to our knowledge, data on the intensity and quality of outreach activities of individual CLAs are not available (Analytical Center 2021). This would also be useful to disentangle the limiting factors of information access and cash-flow costs of travel.

In addition to efforts to identify and break down institutional barriers related to addressing the situation of heavily indebted households, it is also important to evaluate the success rate of such programs, measured e.g. by the (re)integration of borrowers into the labor market. To estimate the net effects of undergoing personal bankruptcy or debt counselling on various outcome indicators, it is essential to establish a good data base on the frequency of individuals affected by foreclosures and structured micro-data on bankruptcy filers. In addition to basic demographic characteristics of clients, these should also include information on their social situation as well as on significant changes in it. Data of this magnitude would make it possible to assess the degree of the effectiveness of these policies and, where appropriate, their positive budgetary impact.

Spatial proximity is an important aspect of accessibility to personal bankruptcy for the overindebted population. After considering the socio-economic factors, by December 2019, spatial proximity increased cumulative bankruptcy rates in comparison to spatially distant municipalities by 3.3 bankruptcies per 1,000 inhabitants.

We estimate that if all municipalities had been close to CLA since the beginning of 2017, the number of personal bankruptcies could have been increased by about 3,000 during the 3 year period of 2017-2019.

We find this effect of geographical proximity even though CLA offers email and telephone communication. This highlights the importance of personal contact with legal aid centers in the process of filing for bankruptcy.

A host of robustness checks support the effect of spatial distance, whether we consider road distance with various cutoff points, or travel times using public transport.

2.8 Credit author statement

Štefan Domonkos conceived the idea for the study, decided on the variables to be included in the statistical analysis and prepared the microdata for the statistical analysis, collected information on the legislation governing personal bankruptcy and its implementation in practice, wrote the initial draft and revised the manuscript. Kristína Hrehová conceived the idea for the study, decided on the variables to be included in the statistical analysis and prepared the microdata for the statistical analysis, developed the methodology and

performed the statistical analysis, validated the analytical methods, wrote the initial draft and revised the manuscript.

2.A Appendix A: Data sources

2.B Appendix B: List of new CLA locations with opening dates, 2017-2019

- Fiľakovo, Giraltovce - 18.10.2018,
- Trnava (office), Tvrdošín (office) - 01.11.2018,
- Trebišov - 23.01.2019,
- Galanta, Senica, Martin - 01.02.2019,
- Topoľčany, Malacky, Bánovce nad Bebravou, Komárno (office) - 18.02.2019,
- Skalica - 01.04.2019,
- Rožňava - 01.06.2019,
- Stará Ľubovňa - 17.07.2019.

Chapter 3

Persuasive Campaigns, Abortions and Fertility

3.1 Introduction

Abortion rates have declined dramatically since 1990 in developed countries and this has in some part been due to state abortion policies.[1] Studies have focused mostly on policies restricting access to abortion by limiting legal abortion to certain stages of fetal development (Bloom et al. 2009), increasing the cost of an abortion by removing state funding (Beauchamp 2016), imposing a waiting period (Roberts et al. 2016), requiring parental consent in cases of pregnant adolescents (New 2011; Beauchamp 2016), and increasing safety requirements for abortion clinics (Fischer, Royer, and White 2018).

The state, however, is not the only actor attempting to shape abortion behavior. Civic society attempts to influence abortion policies and also the behavior of gynecologists (Engeli 2009) and pregnant women directly. In general, there are two competing sides in this process. Both organize rallies, online petitions, post billboards, stand in front of abortion clinics, and give out leaflets (Medoff 2015). Both pro-choice and pro-life NGOs believe their campaigns are effective. However, there is almost no empirical research on the content and effects of such campaigns. Specifically, it is not known whether pro-life NGOs have contributed to the secular decline in abortion since the 1990s.

The lack of research is perhaps not surprising given that any estimation of the effect of such campaigns must deal with the selective nature of activist location and intensity, and also with the relatively rare nature of abortion. Activist location and intensity are

1. Sedgh et al. 2016. For an overview of global abortion policies, see Johnson Jr et al. 2017.

selective; therefore, pro-life billboards and fliers can be more prevalent in communities with more pro-life views. In other cases, pro-life effort is directed to places that are not pro-life. An ideal experimental design would be to study a randomly-assigned intervention. This ideal is not possible. The NGOs know that abortion is a rare outcome and when they try to influence abortion rates, they try to target their campaigns to women facing unexpected pregnancies. Further, data on campaign locations is also often lacking or imprecise.

In this paper, I study a leaflet mailing campaign that comes close to the scientific ideal because the criterion the NGO running the campaign used to decide campaign locations was clearly defined. The NGO faced financial constraints, which forced it to distribute leaflets in some districts and municipalities and not to others. The NGO picked districts based on an abortion rate cutoff and within these districts, sent leaflets to all municipalities reached by a mailing advertisement company. This creates quasi-random variation in leaflet distribution since one can compare municipalities with similar abortion rates located in treated and non-treated districts. More specifically, I assume that conditional on district pre-campaign abortion rates and on being covered by regular mailing-ad campaigns, municipalities were randomly selected into treatment. I use the inverse propensity score weighting method and compare "control" municipalities, which received only regular advertising, with "treated" municipalities, which received the pro-life leaflets on top of the regular advertising mail.

Another advantage of the campaign studied here is that it is possible to define the location of treatment precisely and thus use administrative data on fertility outcomes. To detect any persuasion effects on a rare outcome, one would have to collect very large random samples or rely on population data on abortions by a geographic group, as we do here. In developed countries, there were about 27 abortions per 1000 women annually between 15 and 44 years old in the 1990-2014 period.[2] This means that if an anti-abortion campaign reaches 1000 random women on the street, and we suppose that 10 percent of these were persuaded by it, the number of abortions would reduce by just 2 or 3 in a given year, which would be a 10 percent change in the abortion rate for these women. This makes the effect of any small, untargeted campaign on abortions difficult to measure. To answer this challenge, I study a relatively large campaign, reaching hundreds of thousands of households. This scale allows me to use municipal-level quarterly abortion and fertility

2. This corresponds to 28% of pregnancies ending in abortion (Sedgh et al. 2016).

administrative (health-system) statistics.[3]

In this paper, I provide the first empirical evaluation of the causal effects of a large pro-life leaflet mailing campaign, based on a campaign that operated in Slovakia during 2016-2017. I study both conception and abortion effects. Pro-life persuasion campaigns that focus on abortion also, at least implicitly, target conception. While a "pro-life" message may make having children more attractive, an "anti-abortion" message may make women more cautious. Conception is also an important outcome from a socio-demographic perspective. Additional conceptions may translate into aggregate changes in labor-market outcomes of persuaded women. Therefore, I study the effects on conception rates and then, conditional on conception, the effect on abortion rates.

The results suggest that the campaign did not have a statistically significant effect on conception rates or abortion rates. These results are robust to various specifications and checks.

3.2 Literature review

Only a few papers study the effects of one-shot persuasion campaigns aiming at changing reproductive behavior.[4] Dupas (2011) evaluates the effect of an informational campaign in the context of sex education. While they do not find an effect on the frequency of reproductive behavior, the campaign influenced the age of sexual partners as students learned that older partners are riskier for transmitting AIDS. Bassi and Rasul (2017) used a natural experiment of the Pope's visit to Brazil in the middle of the collection of a Demography and Health Survey. This allowed them to measure changes in fertility behavior following speeches encouraging marital conception. They find that exposure to the Pope's speeches reduced intention to use contraceptives and increased births nine months later. This increase was, however, short-term. Swigger (2017) studied whether norms present in nonpolitical sitcoms could influence support for abortions and access to contraception. The study finds that those who were not already polarized on these questions became more accepting of abortions and use of contraception.

3. In Slovakia, abortions are legal for any reason until 12 weeks of pregnancy. In contrast to health and reproductive surveys common in the literature, Slovak administrative data is recorded officially by gynecologists and abortion statistics have been complete since the previous regime of Czechoslovakia since 1958 (Kocourkova 2016, p. 888).

4. There is a larger literature focusing on telenovelas in Latin America and campaigns against sexually transmitted diseases. These interventions were not one-shot, as the campaign studied in this paper is.

Standard pro-life leaflets using pro-woman framing state that abortion poses significant physical and psychological health risks for the pregnant woman[5] and often also promote NGOs offering counseling to pregnant women.[6] This is also the case in the campaign I study.

Finer et al. (2005) surveyed American abortion patients to ascertain their reasons for abortion. Among other findings, 73% of women claimed that one of their reasons for abortion was that they could not afford a baby at that time. The leaflet campaign studied in this project provides information about financial help available to mothers from state benefits and nonprofit organizations. Other reasons cited were that having a child would interfere with a woman's education, work, or ability to care for dependents (74%); and that she did not want to be a single mother or was having relationship problems (48%).

Any study of abortion needs to condition on socioeconomic factors such as religious affiliation since there is much research suggesting these factors drive abortion choices. For example, Adamczyk (2008) studies women who had their first pregnancy out-of-wedlock from the National Longitudinal Study of Adolescent Health and finds that while identification with a conservative protestant denomination is associated with a lower abortion rate, education seems a more important predictor in this sample. Kocourkova (2016) studied the European Value Survey for the Czech Republic and Slovakia for 1991 and 2008. Using logistic models, she finds that religion was an important factor in lowering abortion rates in Slovakia after the Velvet revolution, while in Czechia, prescribed contraception was a more significant driver of lowering abortion rates. Therefore, my work uses religious affiliation as a control variable in some of the models.

3.3 The Campaign

In Slovakia, abortion was liberalized in the 1980s.[7] Slovak abortion rates have fallen from 14.2 in 1997 to 5.5 in 2008 and 4.5 in 2019.[8]

5. The extent of these risks is a controversial issue. Some psychological studies suggest that there may be substantial psychological risks associated with abortion as opposed to childbirth in some demographic groups. For a recent review, see Bellieni and Buonocore (2013). For an opposing view, see Dadlez and Andrews (2010). Another strand of literature focuses on the possible psychological costs of pro-life persuasion (recently Berglas et al. 2017; Hoctor and Lamačková 2017).

6. There have been a few studies recently analyzing the persuasive content of pro-life and pro-choice campaigns. Hill (2017) Ntontis and Hopkins (2018) Van Erven (2018)

7. For more information about development of Slovak abortion laws, see Stănescu and Nemtanu (2015)

8. Source: NCZI 2015. Conception and abortion rates are defined as number of conceptions, resp. abortions per 1 000 women of reproductive age.

In Europe, the abortion rate fell from 48 in 1995 to 27 abortions per 1000 women in 2008. This decline was mostly driven by Eastern Europe while in Western Europe abortion rates were stagnant in the same period, at the level of about 12 abortions per 1000 women. Abortion numbers also fell in terms of percentage of all pregnancies.[9] More recently in 2018, abortion rates in countries where abortion on request is legal vary from 4.5 per 1000 in Slovakia to 19.2 per 1000 in Sweden.[10] Several recent legislative reforms in Slovakia were aimed at curbing abortion rates including a law about informed consent and mandatory waiting periods of 48 hours. The leaflets in the campaign we study are inspired by these laws, according to communication with the NGO.[11]

The leaflet employs widely used pro-life frames (documented in Trumpy 2014; Rose 2011), including information about fetal development and arguments supporting the notion that abortion hurts women, including anecdotes from the clinical experience of a psychologist. Personal anecdotes of parental happiness and of overcoming difficulties of unplanned pregnancy also appeared in the leaflet. The campaign does not include any images of a fetus. This is because the NGO is primarily trying to persuade a secular audience using facts and an overall positive message.[12] There is also a technical description of the artificial interruption medical procedure. The NGO wanted to transmit all that should be included in informed consent to an abortion.[13]

The two waves of the campaign used distinct versions of leaflets that were produced by the same NGO and highly similar in terms of content and structure (the identity of the celebrities interviewed differed, for example). In the first pages of both leaflets, there is a full-page photo of a young woman, a full-page photo of a couple with a newborn infant, and an editorial. Then, there is an interview with a singer about his or her family life and another interview with a student who became a mother during her college studies.

9. Sedgh et al. 2012

10. National Board of Health and Welfare. Swedish Ministry of Health. Statistics on abortion in 2018. Available: https://www.socialstyrelsen.se/globalassets/sharepoint-dokument/artikelkatalog/statistik/2019-5-18.pdf [Accessed 18 May 2020].Google Scholar

11. For a criticism of these measures, see Hoctor and Lamačková (2017).

12. See the translated frontpage headlines:“New woman", “Psychologist: Many women suffer after abortion", “The correct choice" “I have been grateful for my father’s decision for my whole life". Intentions of the NGO are reported based on email and phone communication with the NGO.

13. The informed consent of artificial interruption of pregnancy (induced abortion) means that the woman signs that she was informed, according to law No. 345/2009 z.z. (http://www.zakonypreludi.sk/zz/2009-345) about a) the goal, nature, process and effects of the artificial interruption of the pregnancy b) the physical and mental risks of the artificial interruption of the pregnancy c) the actual development stage of the fetus or embryo whose development is to be terminated and her right to obtain a picture from the sonographic examination, and d) alternatives to the artificial interruption of the pregnancy, including secret birth, adoption and financial, material or psychological help in pregnancy supplied by associations, non-profits, foundations, churches and religious societies.

Both stories are optimistic, which reflects the general mood of the leaflet. However, the following pages are devoted to explaining the potential risk of abortion for a pregnant woman. A female psychologist and a psychiatrist are interviewed about their experience counseling women with experience of abortion, and a professor of medicine adds a shorter column about the physical health risks of abortion.

The salient content of the leaflet was based on the requirements of the informed consent law. Also included was an overview of social benefits available to mothers, information about burials of aborted children, advice for individuals approached by a pregnant woman considering abortion, and personal encouragement, including a testimony from a student who became pregnant during her college studies. There was also an interview with a singer about their family. The leaflet also contained A4 photos of babies and families with a caption resembling a magazine.

According to the NGO, the leaflet content was also influenced by a series of American studies about the decision-making of pro-choice women experiencing unwanted pregnancies (Vitae Foundation, Paul F. Swope 1998). These studies show that pregnant women may experience difficult emotions and look for a resolution of the pregnancy that preserves their sense of identity. According to Paul F. Swope (1998), effective communication strategies of pro-life NGOs should then provide them with encouragement and reassurance that they can cope with giving birth and be proud of themselves in the end.

The NGO decided to use a professional ad-distribution company for leaflet distribution. According to information provided by the NGO, this service is more cost effective than the national post. Two distributional companies were used. Company A regularly puts regional ad magazines into mailboxes and Company B regularly distributes large retail catalogues into mailboxes.

The campaign had three features that make it uniquely useful for the purpose of estimating the effects of pro-life persuasion campaigns. First, the campaign is geographically limited to 295 municipalities out of the total of 2927 municipalities of Slovakia, but large enough to reach hundreds of thousands of households. The leaflets are mailed directly to households, in contrast to most leaflet campaigns in which volunteers hand leaflets to passers-by. As abortions and births are recorded according to addresses of the pregnant women, this campaign provided precise identification at the municipal level of the population reached. One limitation of our study is possible migration for work and study. Abortions and births are recorded with address of permanent residence instead of address of current or temporary residence. This should bias our estimates of any effects of the

campaign downwards.

The NGO wanted to increase the chances that the leaflet would reach women considering abortion and selected districts based on the previous year's abortion rate. Municipalities were then selected based on the availability of distribution by Company A in Wave 1 and Companies A and B in Wave 2. The leaflets were distributed solely based on district-level abortion rates and based on the availability of the leaflet distribution service at the municipal level. This distribution mechanism allows me to estimate the effect of the campaign assuming that municipalities were randomly selected into the campaign, conditional on district abortion rates in 2015 and receiving regular mailing ads.

Second, the campaign is relatively large, covering 230 000 households in the first wave and 470 000 households in the second wave. Most anti-abortion leaflet campaigns have been smaller in scope, which makes establishing their effectiveness difficult.[14]

Third, the leaflet campaign targeted personal behavior rather than political opinions. Accordingly, the target audience of the leaflet are women of childbearing age. In this project, the NGO aims to persuade pregnant women experiencing difficulties to imagine themselves as successful mothers. This framing is increasingly being used by pro-life organizations (Trumpy 2014; Rose 2011). Unintentionally, this framing could also be effective in persuading women who might become pregnant to be more careful not to become pregnant.

3.4 Identification Strategy

This study aims to answer the following question: Could the persuasive pro-life campaign influence conception and abortion rates? The well-defined selection of treatment municipalities allows me to answer these questions based on a well-defined control group. I construct a comparison group of municipalities who were never contacted by the NGO. If selection into this group is conditionally unconfounded, I can interpret the difference in fertility outcomes as causal.

There are several types of control groups that could be relied on. First, one could use Wave-2 municipalities as controls for Wave 1 municipalities. However, adding more untreated municipalities as controls improves sample size and increases the external validity of a study. Second, one could choose municipalities within districts picked by the

14. Source: Annual reports of Fórum Života, a Slovak pro-life platform https://forumzivota.sk/vyrocna-sprava/.

NGO without the ad-distribution service. However, the mechanism of ad-distribution selection is not entirely clear. Factors coming into play are the buying power of the population, population size and density and presence of a labor market for ad delivery services. A third alternative, which I rely on in this paper, is to select control municipalities in untreated districts with an ad-delivery service with similar pre-treatment abortion levels and trends. This ensures that ad delivery is constant in my analysis and the only variable that determines selection is district-wide abortion rates.

The NGO sent two waves of leaflets. The first was in November (2016) when 230 000 leaflets were sent to 13 Slovakian districts (out of a total of 79). According to the 2011 census, there are about 1,372,647 women of fertile age in Slovakia and 177,286 of them live in the districts selected for the first wave. These districts had the highest abortion rates in 2015. In the second wave in 2017, approximately 470 000 leaflets were sent to the next 19 districts, sorted according to abortion rates in 2016, and with a population of about 328,990 women of fertile age. Therefore, if every leaflet reached one woman, the whole population of women of fertile age in these districts could have been treated.

It would be best to use municipalities treated in both waves of the campaign. However, Wave 1 municipalities were more balanced with control municipalities[15] than Wave 2 municipalities were with untreated municipalities. Therefore, I only look at the impact of the first wave of the campaign. In 2016 I consider untreated and wave 2 municipalities as controls and for 2017 I consider untreated municipalities as controls and exclude the wave 1 municipalities. I also exclude the district of Levice from the analysis sample. This district has the highest abortion rate in the country and anecdotal evidence suggests this may be due to misreported addresses of foreign women undergoing an abortion there. I also omit the capital city of Bratislava, which hosted a March for Life in the summer of 2015, which handed out many pro-life leaflets and other advertisements.

	control districts	wave 1 districts	wave 2 districts
no ads	906	307	304
ads	**847**	**194**	**140**

Table 3.1: Table of municipalities according to advertisement-firm availability and selection into leafletting waves, excluding Levice and Bratislava

Within districts, the leaflets only potentially reached households in areas where the leaflet distribution company regularly distributes its ads. The advertisement company

15. wave 2 and untreated

distributed the pro-life campaign with its monthly ad magazines to 244 municipalities in wave 1 districts and 144 municipalities in wave 2 districts. After excluding Levice and Bratislava, we have 194 treated wave 1 municipalities, 140 treated wave 2 municipalities and 847 potential control municipalities, which received only the regular ad magazine during both waves of the campaign (see Table 3.1). The final sample size of municipalities reached by the regular mail-ad campaigns after weighting on district abortion rates in 2018 is 1083.

The campaign began on Nov 12 in both 2016 and 2017. For this study, I consider its effects on conceptions by counting all registered births and abortions, that were conceived in a 12-week window after the campaign. I also study its effects on legally induced abortions conditional on conceptions by considering births and abortions that were conceived in a 12-week window before the campaign. To remove seasonality, I focus on the same time windows for consecutive years from 2013 until 2017.

The size of the campaign was relatively large, covering 13 (of 79) districts, with 230 000 leaflets in the first wave and 20 more districts with 427 000 more leaflets in the second wave.[16]

We obtained data on abortion and conception municipality level counts for 12-week periods[17] from the National Health Information Centre (NHIC). Information about campaign dates, content and locations was obtained from the pro-life NGO and the websites of companies used for mailing campaign distribution. Regional characteristics, in particular, the demographic and religious composition of the municipalities, were obtained from the Slovak Census 2011; the Stata version was provided by Hlavac (2016).

My identification strategy relies on inverse probability weighting of municipalities based on the district abortion rate in 2015. Figure 3.1 shows district selection into treatment based on district-level abortion rates in 2015 and 2016 in waves 1 and 2, respectively. However, while the district abortion rate is calculated from all abortions registered in a given district, only a portion of municipalities in selected districts were reached by the campaign.[18] In my identification strategy I thus study municipalities reached by the advertising companies (see figure 3.2) and within them I calculate the

16. See the leaflet pdf and commentary in Slovak here:
https://www.hfi.sk/clanky/pravo-na-zivot/320-nova-zena-chrani-pred-potratom.

17. Periods were defined as follows: abortions conceived 12 weeks before November 12 of a given year and births conceived 12 weeks after November 12 of a given year from 2009-2017.

18. Registred abortions do not include illegal abortions, for example chemically induced abortions or abortions potentially caused by the "day-after pill".

propensity to treatment using district abortion rates in 2015.[19] Figure 3.3 shows the distribution of ad delivery services and district-level selection into waves in one map. To further balance the control and treatment samples, I use inverse propensity score weighting on the district abortion rate in 2015 and its square. Assuming conditional unconfoudedness of selection into campaign municipalities, I can interpret the results of this study as causal.

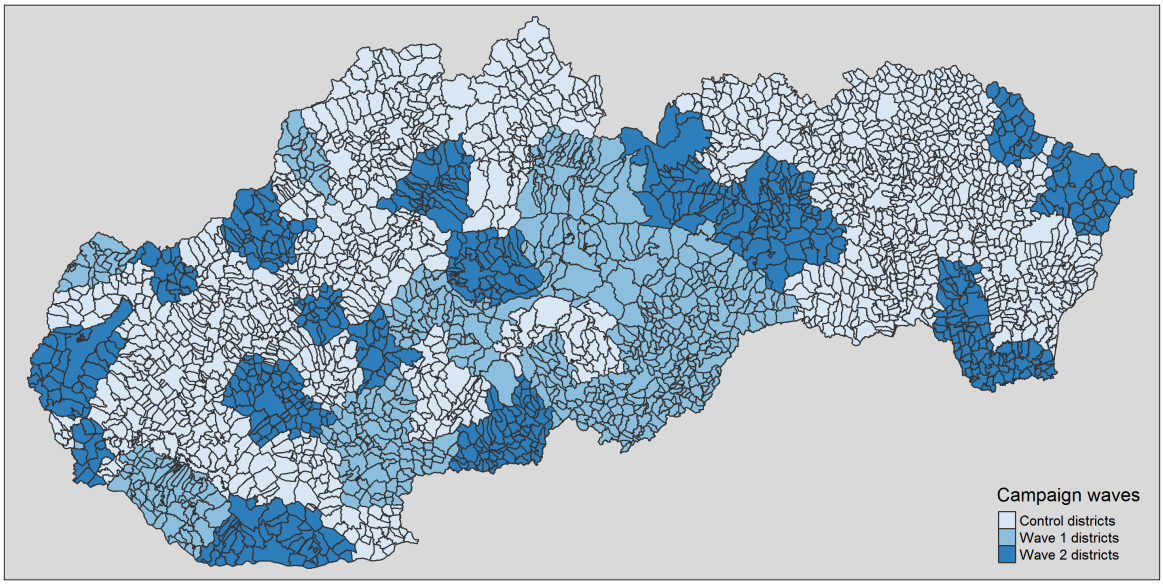

Figure 3.1: District selection for the two waves of the campaign

I compute abortion rates, birth rates, and estimated conception rates [20] for each municipality for all the abortions and births conceived in the specific 12 week periods in 2015, 2016 and 2017: 12 weeks before and after Nov 12.[21] Then I use an inverse probability weighting to compare the abortion, birth, and conception rates for the municipalities that received the leaflets and weighted control municipalities that did not receive them. To account for the inference consequences of selection into treatment being based on the district level, I perform the analysis with clustered standard errors at the district level.

The leaflets were sent through two professional marketing agencies. One of them regularly sends a local advertisement magazine to mailboxes of households, the other works with large retailers and mails their catalogs. The law prohibits sending an advertisement

19. Grey areas in figure 3.2 include Bratislava, Levice and military regions, which are excluded from the analysis.

20. Conception rates are estimated as a sum of birth rates, spontaneous and induced abortion rates matched to conception in a given period per 10 000 women of fertile age. This is a lower bound of total conception rate.

21. The start date of the campaigns in 2016 and 2017.

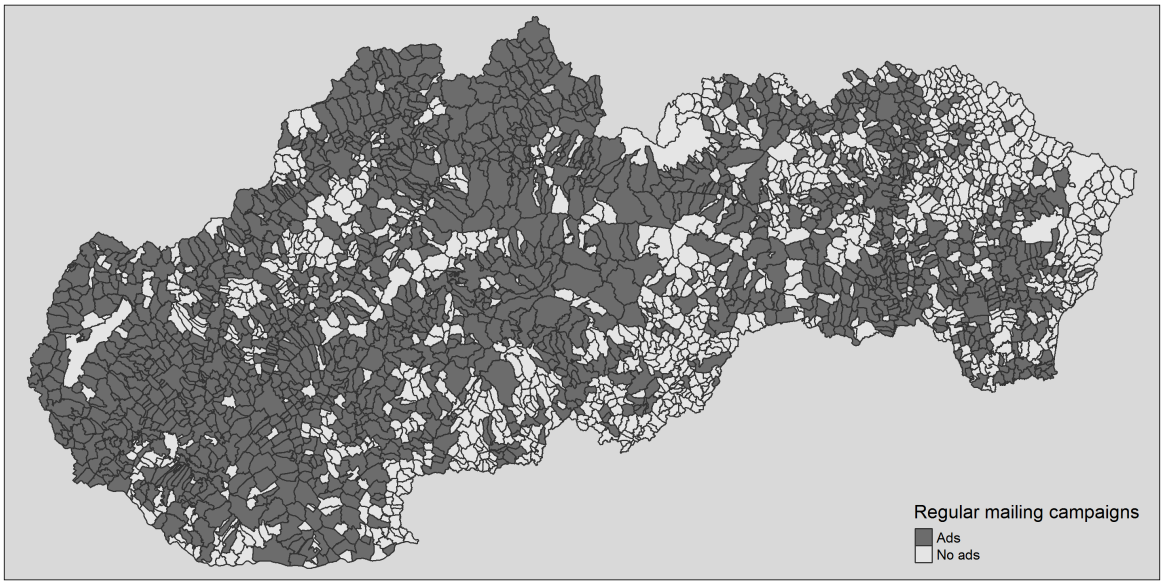

Figure 3.2: Availability of regular advertisement distribution

to those that show visibly that they do not wish advertisements. I assume that no-leaflet stickers are balanced across treatment and control municipalities. This assumption could be checked in principle but is not possible within this study. Residents who opt out of leaflets would likely throw them out anyway. Therefore, they are not influenced by any leaflet campaign so evaluation of any mailing campaign would not be influenced by them.

3.5 Estimation strategy

According to Hirano, Imbens, and Ridder (2003), assuming conditional unconfoundedness, ATE is estimated most efficiently and consistently using weighted least-squares regression with inverse propensity score weighting (IPW). As the name of the technique implies, the municipalities are weighted by the inverse of estimated probability of their treatment status. Intuitively, this method gives more weight to those treatment and control municipalities that are more similar to each other and less to municipalities that are more different in observable variables. In my application of inverse probability weighting, I estimate propensity scores using logistic regression.

$$\frac{p(T_m)}{1-p(T_m)} = e^{\alpha+\theta X_m+\epsilon_m} \tag{3.1}$$

where m denotes municipality m, p denotes probability, T_m is 1 if the municipality

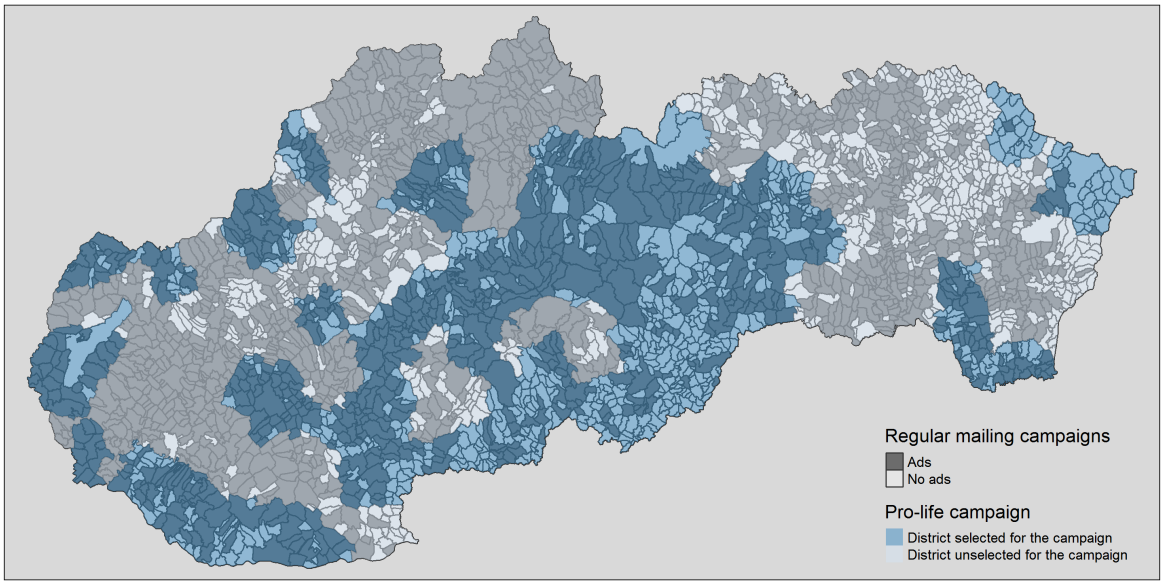

Figure 3.3: Ads coverage across treatment and control districts. Treated municipalities are on the intersection of grey and dark blue. Control municipalities are colored gray with no tint of blue.

received a pro-life mailing campaign, and 0 otherwise, X_m includes the district abortion rate in 2015. The ATE estimator is calculated as

$$\hat{ATE}_{IPW} = \frac{1}{N}\sum_{m=1}^{N}\left(\frac{T_m Y_m}{\hat{p}(T_m)} - \frac{(1-T_m)Y_m}{(1-\hat{p}(T_m))}\right) \tag{3.2}$$

where outcomes Y_m are quarterly conception rates and abortion rates given that conception has occurred.[22]

The augmented inverse probability weighting (AIPW) allows me to specify not only the equation for the propensity score but also a regression for the other determinants of abortion and conception rates. The ATE estimator is calculated as

$$\hat{ATE}_{AIPW} = \frac{1}{N}\sum_{m=1}^{N}\left(\frac{T_m Y_m}{\hat{p}(T_m)} - \frac{(1-T_m)Y_m}{1-\hat{p}(T_m)}\right) \tag{3.3}$$

22. I prefer estimates from the IPW method as it uses the whole sample of 1083 municipalities covered by regular mailing ad campaigns while matching estimators select only 316 control municipalities and are less robust to specification errors. The results of matching are comparable, and included in a robustness check.

$$-\frac{1}{N}\sum_{m=1}^{N}\frac{T_m-\hat{p}(T_m)}{\hat{p}(T_m)(1-\hat{p}(T_m))}[(1-\hat{p}(T_m))\hat{E}(Y_m|X_m=1,Z_m)+\hat{p}(T_m)\hat{E}(Y_m|X_m=0,Z_m)] \tag{3.4}$$

where Z is a vector of other municipal characteristics that could influence fertility outcomes, including variables related to religious composition. This estimator is doubly robust: If either the propensity score regression or the outcome regressions are well specified, the result is a consistent estimate of ATE.[23]

3.6 Descriptive statistics

Table 3.2 shows the balance of key variables prior to weighting; and Table 3.3 shows the balance after weighting with propensity to receive the mailing campaign. The standard errors are based on clusters at the district level. The overall balance of the control and treatment samples improves with weighting. The only statistically significant imbalance at the 5% level remains in the proportion of citizens stating they are Catholic. This gives a reason to additionally consider religious composition in the analysis.

	(1)	(2)	(3)
	Means		Difference
Variable	Control	Treatment	Control vs Treatment
concr2015	131.435	124.552	-6.883
	(88.647)	(115.298)	(11.248)
Abortion rate 2015	11.378	16.181	4.802
	(20.837)	(45.909)	(4.769)
No religion	0.097	0.123	0.027*
	(0.072)	(0.080)	(0.015)
Reformed christian	0.037	0.032	-0.005
	(0.111)	(0.083)	(0.021)
Catholic	0.692	0.580	-0.112*
	(0.211)	(0.232)	(0.064)
College and higher education	0.267	0.215	-0.051
	(0.099)	(0.067)	(0.031)
Abortion rate 2015	11.378	16.181	4.802
	(20.837)	(45.909)	(4.769)
Observations	552	200	759

*$p < 0.10$, *$p < 0.05$, *$p < 0.01$

Table 3.2: Raw sample balance

23. For proof, see Wooldridge (2010) or Glynn and Quinn (2010).

	(1)	(2)	(3)
	Means		Difference
Variable	Control	Treatment	Control vs Treatment
concr2015	131.720	125.345	-6.375
	(89.607)	(118.823)	(11.646)
Abortion rate 2015	11.002	18.564	7.562
	(20.650)	(62.304)	(8.875)
No religion	0.094	0.115	0.021
	(0.069)	(0.074)	(0.015)
Reformed christian	0.035	0.030	-0.004
	(0.107)	(0.082)	(0.022)
Catholic	0.701	0.550	-0.151**
	(0.206)	(0.233)	(0.070)
College and higher education	0.272	0.197	-0.075*
	(0.100)	(0.067)	(0.037)
Abortion rate 2015	11.002	18.564	7.562
	(20.650)	(62.304)	(8.875)
Observations	552	200	752

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

Table 3.3: Weighted sample balance

3.7 Results

Table 3.4 shows the estimated ATE effect of the campaign. There is no significant effect on the difference in abortion rates, or conception rates at the 5% level. The propensity score model used for the selection propensity of the municipalities explains about 37% of variation in treatment assignment according to McFadden's Adjusted R^2. The district abortion rate in 2015 emerges as a strong predictor of selection into treatment. The estimated local average treatment effect on abortions and conceptions is not statistically significant and their size is relatively small. While the treatment effect on abortion rates conditional on conception is not significant in these specifications, 3.4 abortions per 10 000 women represents less then 16 % of a standard deviation in abortion rates and about 7 % of the annual mean abortion rates.[24] The estimated local average treatment effect on conceptions — 3.8 fewer conceptions per 10 000 women of reproductive age in the first year — represents about 2 per cent of a standard deviation in conception rates or 0.9 % of mean annual conception rates.[25] These results are not statistically significant and

24. Standard deviation in quarterly abortion rates in the analytical sample in 2015 was 21 per 10 000 women. The annual mean abortion rate in 2016 was 49 legally induced abortions per 10 000 women. Source: https://www.nczisk.sk/Documents/publikacie/2016/zs1707.pdf , page 9.

25. Annual mean birthrate in 2016 was 433 births per 10 000 women. Source: https://data.nczisk.sk/statisticke_vystupy/gynekologia_porodnictvo/Starostlivost_o_rodicku_a_

their sign and magnitude vary according to specification.

In contrast to the NGOs expectations that were driving the campaign, there is no statistically significant evidence that the campaign decreased the likelihood of abortion conditional on conception before the campaign in the treated municipalities. What could explain this result? The campaign aimed to reach the secular population. It is possible that this population does not respond to pro-life campaigns. There is also the possibility that women cannot be easily persuaded by a simple mailing campaign.

Table 3.4: Inversed propensity weighting, average treatment effect, wave 1.

	(1)	(2)	(3)	(4)
	Δ abortion Y0	Δ conception Y0	Δ abortion Y0	Δ conception Y0
ATE				
r1vs0.treated	3.283	4.032	-3.697	7.854
	(3.195)	(3.412)	(13.63)	(13.98)
POmean				
r0.treated	-5.372**	-6.540***	-14.98**	1.458
	(1.654)	(1.800)	(4.888)	(8.160)
TME1				
District abortion rate	9.953***	7.749***	9.953***	7.749***
	(1.184)	(1.295)	(0.949)	(1.390)
District abortion rate^2	-0.484***	-0.379***	-0.484***	-0.379***
	(0.0603)	(0.0676)	(0.0492)	(0.0779)
No religion	0.851	2.316	0.851	2.316
	(3.305)	(3.013)	(3.234)	(4.346)
Constant	-48.18***	-37.15***	-48.18***	-37.15***
	(5.503)	(5.993)	(4.390)	(6.080)
Observations	752	552	752	552
r2				

Standard errors in parentheses

* $p < 0.050$, ** $p < 0.01$, *** $p < 0.001$

3.8 Robustness

Because the district abortion rate is a significant predictor of treatment, there is a concern about whether the municipal abortion rates are also correlated with treatment status. I therefore regressed the treatment assignment on the district abortion rate in 2015 and

novorodenca_v_SR_2016.pdf, page 10.

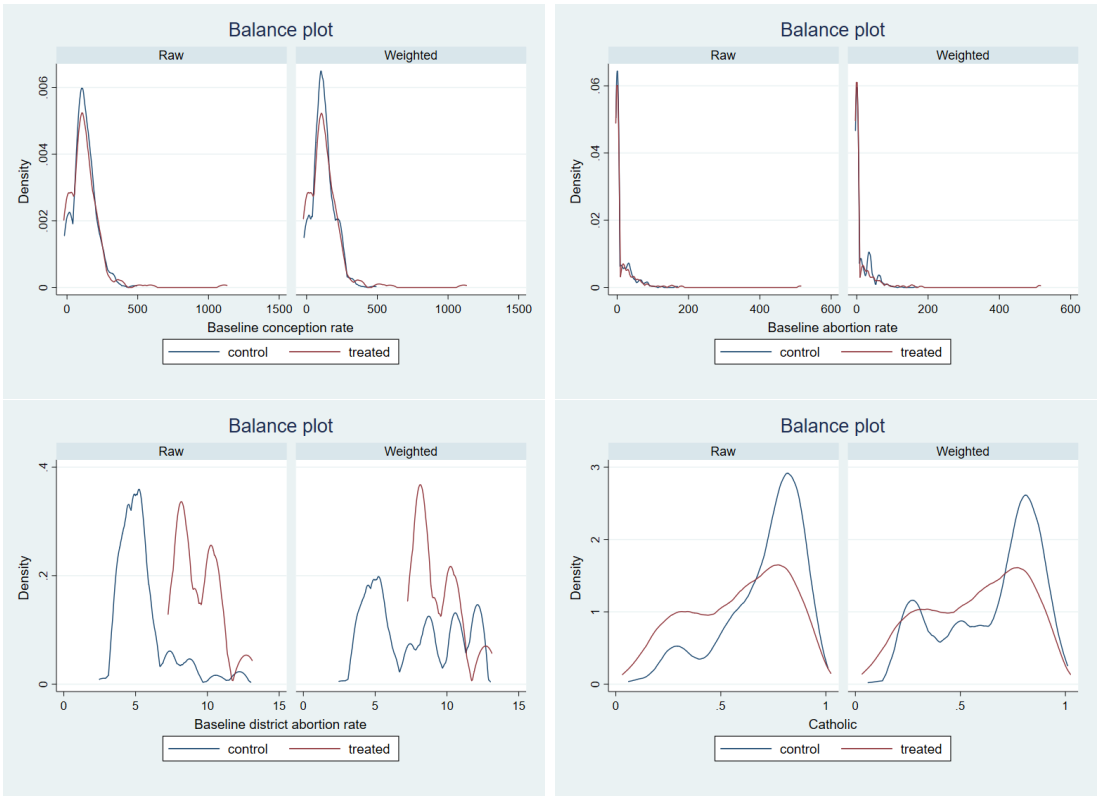

Figure 3.4: Balance samples based on Wave 1 weighted on the basis of a quadratic function of district abortion rates in 2015.

November quarterly abortion rates in 2009 – 2015, on population counts, and on religious controls. The p-value of a joint significance F test of municipal quarterly abortion rates was 0.67 and I therefore conclude that municipal abortion rates do not predict treatment status when district abortion rates are taken into account.

3.8.1 Removing municipalities where more than 80% population are catholic

Religion, in particular, is connected in the sociological literature to abortion as a major influence. In Slovakia, the major religion is Catholicism, which forbids both contraception and abortion. Municipalities with a large majority Catholic population also have lower abortion rates than the average. Accordingly, the Catholic population was not targeted by the leaflet campaign. Although there are regular religious-based pro-life campaigns including the "candle for life" and "40 days for life" prayer vigil, the leaflet campaign we analyze aimed to reach a secular audience. Removing the most-catholic municipalities with a catholic population in excess of 80%, decreased the magnitudes of the effect on

abortions and increased the magnitude of the effect on conceptions. However, it did not change the statistical significance of the results. Tables 3.5 and 3.6 show the results of this check.

Table 3.5: Inverse propensity weighting, average treatment effect. Dropped where catholics form more than 80 percent of the population.

	(1) Δ abortion Y0	(2) Δ abortion Y1	(3) Δ conception Y0	(4) Δ conception Y1
ATE				
r1vs0.treated	-0.722	1.025	12.12	25.95
	(3.197)	(3.307)	(18.77)	(19.04)
POmean				
treated=0	-3.198	-7.169***	-37.11***	-26.01**
	(1.853)	(1.973)	(7.732)	(9.373)
TME1				
District abortion rate	11.05***	8.558***	11.05***	8.558***
	(1.222)	(1.709)	(1.181)	(1.802)
District abortion rate^2	-0.540***	-0.422***	-0.540***	-0.422***
	(0.0621)	(0.0883)	(0.0596)	(0.0954)
No religion	-3.032	-1.219	-3.032	-1.219
	(2.544)	(2.493)	(2.055)	(2.598)
Abortion rate 2015	0.00180	0.000625	0.00180	0.000625
	(0.00203)	(0.00293)	(0.00205)	(0.00237)
Constant	-52.46***	-40.12***	-52.46***	-40.12***
	(5.678)	(7.839)	(5.565)	(8.162)
Observations	428	329	428	329
r2				

Standard errors in parentheses

* $p < 0.050$, ** $p < 0.01$, *** $p < 0.001$

3.8.2 Removing municipalities where more than half of mothers are college educated

College-educated women may be more informed about abortion and its mental health risks, so if the campaign had any effect, it might be reduced for them. Removing municipalities where more than 50% of pregnant women were college educated does not change the statistical significance of the study. Tables 3.7 and 3.8 show the results of this check.

Table 3.6: Inverse propensity weighting, average treatment effect on the treated. Dropped where catholics form more than 80 percent of the population.

	(1) Δ abortion Y0	(2) Δ abortion Y1	(3) Δ conception Y0	(4) Δ conception Y1
ATET				
r1vs0.treated	0.680	4.900	28.56	36.81
	(4.591)	(6.694)	(27.98)	(31.24)
POmean				
treated=0	-5.165	-10.03*	-53.21	-38.14
	(4.094)	(3.893)	(28.43)	(23.59)
TME1				
District abortion rate	11.05	8.574	11.05	8.574
	(12.42)	(12.59)	(17.26)	(20.21)
District abortion rate^2	-0.540	-0.423	-0.540	-0.423
	(0.698)	(0.695)	(0.954)	(1.155)
No religion	-3.032	-1.225	-3.032	-1.225
	(3.627)	(3.878)	(4.098)	(4.078)
Abortion rate 2015	0.00180	0.000629	0.00180	0.000629
	(0.00568)	(0.00448)	(0.00507)	(0.00509)
Constant	-52.46	-40.20	-52.46	-40.20
	(54.01)	(55.66)	(76.43)	(88.37)
Observations	737	616	737	616
r2				

Standard errors in parentheses

* $p < 0.050$, ** $p < 0.01$, *** $p < 0.001$

Table 3.7: Inverse propensity weighting, average treatment effect. College educated municipalities dropped.

	(1) Δ abortion Y0	(2) Δ conception Y0	(3) Δ abortion Y0	(4) Δ conception Y0
ATE				
r1vs0.treated	5.576	-3.451	3.208	35.62
	(4.311)	(6.430)	(10.98)	(35.04)
POmean				
r0.treated	-8.444**	1.837	-21.88**	-27.48
	(2.883)	(6.146)	(6.731)	(26.29)
TME1				
District abortion rate	15.28***	10.84**	15.28***	10.84**
	(2.724)	(3.627)	(2.773)	(3.724)
District abortion rate^2	-0.709***	-0.489*	-0.709***	-0.489*
	(0.153)	(0.195)	(0.154)	(0.207)
No religion	3.840	5.670	3.840	5.670
	(4.710)	(7.187)	(5.542)	(7.703)
Constant	-76.18***	-54.87**	-76.18***	-54.87**
	(12.39)	(16.69)	(12.43)	(16.78)
Observations	389	295	389	295
r2				

Standard errors in parentheses

* $p < 0.050$, ** $p < 0.01$, *** $p < 0.001$

Table 3.8: Inverse propensity weighting, average treatment effect on the treated. College educated municipalities dropped.

	(1) Δ abortion Y0	(2) Δ conception Y0	(3) Δ abortion Y0	(4) Δ conception Y0
ATET				
r1vs0.treated	9.847	-6.015	12.56	46.78
	(6.483)	(8.276)	(15.56)	(40.43)
POmean				
r0.treated	-12.34*	4.247	-30.17	-38.41
	(5.527)	(8.077)	(19.21)	(33.79)
TME1				
District abortion rate	15.29	10.87	15.29	10.87
	(8.186)	(8.039)	(11.46)	(17.06)
District abortion rate^2	-0.709	-0.490	-0.709	-0.490
	(0.455)	(0.446)	(0.633)	(1.047)
No religion	3.841	5.674	3.841	5.674
	(7.468)	(8.289)	(7.648)	(7.852)
Constant	-76.19*	-54.98	-76.19	-54.98
	(35.81)	(35.72)	(50.67)	(69.34)
Observations	735	535	735	535
r2				

Standard errors in parentheses

* $p < 0.050$, ** $p < 0.01$, *** $p < 0.001$

3.8.3 Other variable selections used for weighting

Many alternative weighting variables were tried; in particular, various combinations of municipal level variables from the 2011 census and employment and socio-economical variables used in Hrehova, Domonkos, et al. 2022. However, in this case, better weighting balance than shown was not obtained from any of the various combinations. Lasso regression was also tried but the exercise did not result in a better balanced sample.

3.9 Conclusion

This paper studies whether a leaflet campaign could influence abortion and conception rates. I study this question using quasi-experimental variation in the case of a mailing campaign in Slovakia. Using inverse probability weighting and considering the first wave of a leaflet-mailing campaign, I find that the campaign did not change abortion or conception rates in a statistically significant way. These results suggest that the campaign's aim to prevent abortions could not be detected with statistical significance. Mailing campaigns may not be the most effective method of influencing women on abortion. In line with this, the NGO discontinued the campaign after the second wave. This study may be of use for other organizations considering the possible impact of a mailing campaign.

3.A Appendix A: Analysis with placebo year

Analysis with a placebo year (Tables 3.9 and 3.10) and analysis of total conceptions conceived during a placebo period of 12 weeks before the first wave of the campaign (Tables 3.11 and 3.12) found no statistically significant results for the first wave, while it was significant for the second wave. This is one of the resons why wave 2 muncipalities were not used in analysing reproductive outcomes in 2017 and 2018.

3.B Appendix B: Analysis with placebo treatment

Analysis with a randomly assigned placebo treatment (Table 3.13 and 3.14) found no statistically significant results for the ATEs. This confirms the validity of our analysis.

Table 3.9: Inverse propensity weighting, ATET placebo minus t, w1.

	(1) Diff in abortion rates	(2) Diff in conception rates
ATE		
r1vs0.Treated	1.398	7.557
	(2.577)	(6.952)
POmean		
Treated=0	0.351	-11.29*
	(1.144)	(4.609)
TME1		
District abortion rate	2.943***	2.943***
	(0.464)	(0.415)
District abortion rate^2	-0.153***	-0.153***
	(0.0296)	(0.0268)
No religion	2.793	2.793
	(2.878)	(2.370)
Constant	-12.41***	-12.41***
	(1.613)	(1.695)
Observations	1573	1573
r2		

Standard errors in parentheses

Source: National Health Information Center (SR) data, own calculations. Placebo regression, earlier years.

* $p < 0.050$, ** $p < 0.01$, *** $p < 0.001$

Table 3.10: Inverse propensity weighting, ATET placebo minus t, w1.

	(1) Diff in abortion rates	(2) Diff in conception rates
ATET		
r1vs0.Treated	2.048	-3.523
	(3.161)	(8.885)
POmean		
Treated=0	-1.591	-2.189
	(2.132)	(4.751)
TME1		
District abortion rate	2.714*	2.714**
	(1.210)	(0.975)
District abortion rate^2	-0.135	-0.135*
	(0.0814)	(0.0640)
No religion	2.806	2.806
	(2.830)	(2.891)
Constant	-11.71**	-11.71***
	(4.080)	(3.346)
Observations	1312	1312
r2		

Standard errors in parentheses

Source: National Health Information Center (SR) data, own calculations. Placebo regression year earlier.

* $p < 0.050$, ** $p < 0.01$, *** $p < 0.001$

Table 3.11: Inverse propensity weighting, placebo ATE.

	(1) Diff in conception rates	(2) Diff in conception rates
ATE		
r1vs0.treated	14.96	
	(10.53)	
r1vs0.treated2		-1.707
		(8.197)
POmean		
r0.treated	-12.44*	
	(5.971)	
treated2=0		3.698
		(2.835)
TME1		
District abortion rate	9.953***	7.251***
	(0.796)	(1.519)
District abortion rate^2	-0.484***	-0.547***
	(0.0417)	(0.117)
No religion	0.851	3.410
	(2.671)	(2.909)
Constant	-48.18***	-23.99***
	(3.293)	(4.620)
Observations	752	1112
r2		

Source: National Health Information Center (SR) data, own calculations. Standard errors in parentheses
* $p < 0.050$, ** $p < 0.01$, *** $p < 0.001$

Table 3.12: Inverse propensity weighting, placebo ATET.

	(1)	(2)
	Diff in conception rates	Diff in conception rates
ATET		
r1vs0.treated	30.86	
	(20.25)	
r1vs0.treated2		-18.55**
		(7.053)
POmean		
r0.treated	-32.11	
	(17.69)	
r0.treated2		7.257
		(3.944)
TME1		
District abortion	9.954	7.252*
rate	(15.16)	(2.996)
District abortion	-0.484	-0.547*
rate^2	(0.838)	(0.248)
No religion	0.851	3.410
	(4.365)	(3.378)
Constant	-48.18	-24.00**
	(67.00)	(8.920)
Observations	1573	1373
r2		

Source: National Health Information Center (SR) data, own calculations. Standard errors in parentheses
* $p < 0.050$, ** $p < 0.01$, *** $p < 0.001$

Table 3.13: Inverse propensity weighting, average treatment effect.

	(1) Δ abortion Y0	(2) Δ conception Y0	(3) Δ abortion Y0	(4) Δ conception Y0
ATE				
r1vs0.randomTreat	-0.388	1.262	-0.381	1.145
	(1.483)	(10.32)	(1.670)	(10.69)
POmean				
randomTreat=0	-1.781	-6.450	-1.785	-6.473
	(0.973)	(4.724)	(1.075)	(5.086)
TME1				
District abortion	0.147	0.147	0.147	0.147
rate	(0.152)	(0.142)	(0.151)	(0.165)
District abortion	-0.0162	-0.0162	-0.0162	-0.0162
rate^2	(0.0120)	(0.0117)	(0.0121)	(0.0135)
No religion	0.852	0.852	0.852	0.852
	(0.944)	(0.886)	(0.958)	(0.960)
Constant	-1.218**	-1.218**	-1.218**	-1.218**
	(0.433)	(0.393)	(0.425)	(0.459)
OME0				
Reformed christian			-12.38	-75.93
			(22.54)	(87.84)
Constant			-1.513	-4.702
			(1.202)	(5.047)
OME1				
Reformed christian			3.360	-124.5
			(18.68)	(73.62)
Constant			-2.203	-2.359
			(1.316)	(11.03)
Observations	1079	1079	1079	1079
r2				

Standard errors in parentheses

Source: National Health Information Center (SR) data, own calculations.

* $p < 0.050$, ** $p < 0.01$, *** $p < 0.001$

Table 3.14: Inverse propensity weighting, average treatment effect on the treated.

	(1) Δ abortion Y0	(2) Δ conception Y0
ATET		
r1vs0.randomTreat	-0.510	-0.00233
	(1.455)	(10.24)
POmean		
randomTreat=0	-1.605	-5.626
	(0.966)	(4.653)
TME1		
District abortion	0.147	0.147
rate	(0.152)	(0.142)
District abortion	-0.0162	-0.0162
rate^2	(0.0120)	(0.0117)
No religion	0.852	0.852
	(0.944)	(0.886)
Constant	-1.218**	-1.218**
	(0.433)	(0.393)
Observations	1079	1079
r2		

Standard errors in parentheses

Source: National Health Information Center (SR) data, own calculations.

* $p < 0.050$, ** $p < 0.01$, *** $p < 0.001$

3.C Appendix C: Analysis with propensity score matching

Table 3.15: Nearest neighbor matching, average treatment effect, w1.

	(1) Δ abortion Y0	(2) Δ abortion Y1	(3) Δ conception Y0	(4) Δ conception Y1
ATE				
r1vs0.Treated	3.255	3.405	6.283	-5.781
	(2.456)	(20.68)	(9.163)	(25.08)
Observations	1573	821	1056	821
r2				

Standard errors in parentheses
Source: National Health Information Center (SR) data, own calculations.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.16: Nearest neighbor matching, average treatment effect on the treated, w1.

	(1) Δ abortion Y0	(2) Δ abortion Y1	(3) Δ conception Y0	(4) Δ conception Y1
ATET				
r1vs0.Treated	7.243*	5.604	12.35	-10.32
	(3.768)	(6.358)	(11.40)	(21.49)
Observations	1573	1338	1573	1338
r2				

Standard errors in parentheses
Source: National Health Information Center (SR) data, own calculations.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Tables 3.15 (ATE) and 3.16 (ATET) shows quantitatively and qualitatively similar results to the weighting exercise. Adding caliper 0.1 did not change the results of matching. It increased the estimates of the short-term effect of the campaign and made a weakly (10%) significant increase in abortion rates.

3.D Appendix D: Power calculations

Power calculations using baseline variation in the outcome variable and assuming cluster randomization show that for the conception rates, the effect I am estimating can be detected with more than 80% power.

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