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Découpage administratif et bien-être subjectif : l'exemple de la fusion des régions

Résumé

Cette étude utilise la fusion des régions de 2016 comme une expérience naturelle ; elle évalue son impact causal sur le bien-être subjectif déclaré par les individus en s'appuyant sur une stratégie d'identification en doubles différences. En dépit de la création de nouvelles régions plus vastes, dont le centre de décision est plus éloigné des administrés, aucun effet négatif n'est détecté sur la satisfaction dans la vie des individus. Au contraire, celle-ci a même augmenté dans les régions qui ont été absorbées d'un point de vue économique ou administratif. Ces régions ont en effet bénéficié d'une dynamique économique plus favorable consécutivement à la fusion, enregistrant notamment une baisse plus prononcée du taux de chômage. Ces meilleures perspectives économiques auraient ainsi plus que compensé un éventuel attachement à l'ancien découpage administratif.

Mots-clés : Fusion des régions ; expérience naturelle ; doubles différences ; bien-être subjectif ; centralisation.

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Abstract:

Using the 2016 merger of French regions as a natural experiment, this paper adopts a difference-in-difference identification strategy to recover its causal impact on individual subjective well-being. No depressing effect is found despite increased centralization; life satisfaction has even increased in regions that were absorbed from economic and political viewpoints. The empirical evidence also suggests that local economic performance improved in the concerned regions, which includes a faster decline in the unemployment rate. In this setting, economic gains have likely outweighed cultural attachment to administrative regions.

Keywords: Merger of regions; natural experiment; difference-in-difference; subjective well-being; centralization.

Classification JEL : H75; I31.

1 Introduction

Subnational integration is a hot topic in the EU where regions are very heterogeneous in terms of size and economic importance, due to historical reasons (Alesina et al., 2017). In such a context, France experienced a wide-scale merger of regions: in 2016, its metropolitan territory was massively reorganized from 22 to 13 administrative regions. Arguments in favor of larger jurisdictions include efficiency gains and economies of scale, i.e. fiscal considerations. However, mergers of regions are very rare in practice, mainly because local authorities are reluctant to lose autonomy and political power, but also because citizens have presumably a taste for being close to government decisions, i.e. for decentralization. In the framework of Alesina and Spolaore (1997), this trade-off between economies of scale and heterogeneity of preferences of the population determines the optimal number and size of regions. Remember Barro (1991): “a large country can spread the cost of public goods, ... over many taxpayers, but a large country is also likely to have diverse population that is difficult for the central government to satisfy”. *De facto*, the public opinion is frequently summoned by local governments to prevent integration, based on the argument that centralization would not make case of regional feelings. In metropolitan France, most emblematic examples of attachment to local culture, and sometimes to specific common law, include Alsace-Lorraine, the Basque Country, Brittany, and Corsica, not claiming to be exhaustive. In the rest of the EU, there are voices in Catalunya, Flanders, Scotland and Wallonia asking for further autonomy, and even independence from Belgium, Spain or the UK. To sum up, three mechanisms are likely at play consecutive to that merger: (i) the efficiency channel (economies of scale), which might improve public service delivery, reduce tax burden and increase residents’ well-being, (ii) the increased heterogeneity inside new regions (more centralized policies less tailored to local needs and characteristics of the population), which might be particularly costly for richer regions, and (iii) the loss of regional identity, which is likely to be more painful for regions that may be viewed as absorbed ones.

This paper seeks to test empirically whether citizens were hurt by the merger viewed as a unique natural experiment that allows me to recover the causal impact of reshaping subnational borders on subjective well-being as self-assessed by individuals. It also aims at disentangling among the three previous channels at stake. Interestingly, this merger of regions was not announced during the 2012

electoral campaign; it was totally unexpected and came out as a surprise to political commentators and citizens at the time. The political debate that preceded the voting of the *Loi numéro 2015-991 du 7 août 2015 portant sur la nouvelle organisation territoriale de la République* (NOTRé law hereafter), which implemented the merger of French regions, gave rise to much concerns about administrative belonging and the risk of losing one's regional identity. Initially motivated by the ambition to reach a critical size and to make efficiency gains in order to compete with European regions, this law is the result of a subtle political process during which the perimeter of the new regions has much evolved with respect to original plans. For instance, Aquitaine and Nord-Pas-de-Calais were bound to remain on their own at the beginning of the process, but turned out to be merged into two (distinct) new regions. On top of that, the initial project planned to extend even further the scope of regional authorities, giving them tasks and powers over roads and lower secondary education, among others.

The identification strategy adopted in this paper relies on a difference-in-difference approach that takes advantage of the reform viewed as a natural experiment. This method is particularly well suited to isolate the effect of a change in subnational borders on individual happiness. Individuals did not move from one region to another: by contrast, and most unusually, *the regions themselves* have changed over the period considered. The control group of this experiment is composed of individuals living in the 6 regions that did not participate to the merger. Different treatment groups may be defined: either individuals living in the 16 former regions which became 7 new regions, or in the 6 former regions that can be considered as having been absorbed, since they lost executive power.¹ The econometric specification is based on a linear model with individual fixed effects; the dependent variable, namely individual subjective well-being, is provided on a discrete 0-10 scale, a Cantril scale. The estimation proceeds from longitudinal survey data: I exploit the *enquête statistique sur les ressources et conditions de vie* (SRCV), where individuals are asked to report their overall satisfaction with life on top of usual information including sociodemographic characteristics and geographic location.

When the treated group is composed of individuals living in merging regions, the merger has no significant effect on individual happiness. However, when fo-

¹Four regions have absorbed them. The six remaining regions deserve special attention since the governance seems to be shared among the concerned entities after the merger, see *infra*.

cusing on the differential evolution of absorbed regions with respect to the control group, a significant and positive impact is obtained: about .076 on the 0-10 scale, equivalent to 4.4% of one standard deviation. Put differently, everything happens roughly as if 7.6% of individuals report an annual change of +1 in their life satisfaction. A number of tests are conducted in order to assess the plausibility of the identification strategy: the above results are robust to a variety of alternative assumptions, including different definitions of treatment or control groups, and sensitivity checks (ordered models, attrition, sampling issues, clustering, etc.). Simulating placebo experiments, namely fake reforms occurring before the observed one, yields non significant estimates even in the absorbed regions, which supports the identifying assumption. Though centralization is usually associated with lower levels of life satisfaction (see, e.g., [Flèche, 2020](#), about Switzerland), regional belonging (if any) does not have depressing effects on individual subjective well-being here.² Hence the French experience at stake suggests at least that the institutional setting matters in this respect, France not being a federal state. In the end, investigating whether previous effects are more or less pronounced for some individuals yields to conclude that those who reported higher levels of subjective well-being were likely to benefit from economic spillovers of the merger.

To understand the above results, I investigate the role played by economic mechanisms. First, an unintended effect of the merger has been to *increase* investment spending. As noticed by the French Court of Auditors ([Cour des Comptes, 2019](#)), investment spending has increased more in merging regions than elsewhere, especially as regards local public goods (trains and upper secondary education, among others) regions are in charge of. More generally, there is empirical evidence of an *increase* in local public spending consecutive to the merger, which might lead to further citizen discontent if the "fiscal effect" (citizens are tax payers; if they are forward-looking, they must anticipate higher local tax) dominates the "residential effect" (citizens are also residents of these regions and they benefit from the provision of local public goods). Second, the economic attractiveness of merging regions improved, in line with public incentives to promote local business development and to enhance economic performance. In particular, the unemployment rate experienced a faster decline in absorbed regions after 2016; the empirical evidence at stake is hence consistent with these poorer regions benefiting from the promo-

²In France, the *département* might be the relevant layer to which citizens are attached as [Boyer et al. \(2020\)](#) suggest in their analysis of the Gilets jaunes movement.

tion activities of the richer absorbing regions. This empirical finding is especially relevant when debating the possibility of grouping local authorities. Finally, this process has contributed to some income convergence within new regions: for instance, civil servants employed by local governments had the variable part of their compensation (bonuses) enhanced after the merger, according to the rules of the most advantaged regime; yet, the magnitude of this phenomenon is empirically too small to account for previous findings. Overall, these results suggest that economic spillovers which come along the creation of larger subnational jurisdictions play a substantial role in alleviating, and even outweighing citizen discontent due to further centralization. They are consistent with the relative importance of economic gains with respect to cultural norms through the example of regional belonging feelings. The positive effect obtained on absorbed regions tends to suggest that the loss of regional identity is dominated by the economic channels. Moreover, the impact on economic outcomes suggests that the efficiency channel does not play a role here given the increase in spending. Increased heterogeneity matters, but has contrasted implications depending on the regions considered: though theoretically painful for absorbing, richer ones, the residents of these regions were not significantly hurt while there is empirical evidence of a positive impact for residents of absorbed, poorer regions. This Pareto improvement is somehow reminiscent of peer effects in education (Guyon et al., 2012) where high-achieving pupils are not hurt by low-achieving roommates benefiting from interactions with the latter, and of the impact of immigration on the labor market (Card, 1990) where local workers are not hurt by newcomers.

The remainder of the paper is organized as follows. Section 2 is devoted to a literature review. Section 3 presents the institutional setting and the merger. Section 4 describes the data. Section 5 is devoted to the identification strategy. Section 6 presents the econometric specification. The results are exposed in section 7 while Section 8 provides some robustness checks. Section 9 investigates economic channels that help rationalize previous findings, and Section 10 concludes.

2 Literature

This paper lies at the intersection of two strands of literature: one devoted to theoretical and empirical effects of integration of local jurisdictions, and another

one concerned by spatial determinants of happiness.

First, the optimal number and size of countries results from a trade-off between economies of scale and heterogeneity of preferences in the theoretical framework built by [Alesina and Spolaore \(1997\)](#). Empirical investigations on mergers of local jurisdictions like municipalities, counties, or regions include [Jackson \(1987\)](#) in the USA, [Mouritzen \(2010\)](#) in Denmark and [Lidström \(2010\)](#) in Sweden. In Japan, [Weese \(2015\)](#) resorts to a structural model along with an asymmetric information problem between the national and local levels of government; he finds that the optimal number of subnational borders is about twice smaller than the actual one. In the French case, a recent contribution by [Tricaud \(2020\)](#) concludes that local integration costs, including a rise in housing supply and higher congestion costs in urban municipalities, combined with an increased distance to public service facilities in rural areas, are major factors against intermunicipal cooperation. By contrast, this paper focuses at the regional level, which is relevant from a EU perspective. A number of papers are also devoted to explaining which factors determine integration ([Gordon and Knight, 2009](#); [Di Porto et al., 2013](#)). In contrast, this paper provides empirical evidence on the impact of a merger of regions on well-being, this causal effect being estimated on longitudinal survey data at the individual level. It is natural to wonder whether such mergers have an impact on cost reduction in the newly formed jurisdictions. The literature has concluded to mixed results ([Bel and Warner, 2015](#)): overall efficiency gains are found in Israel by [Reingewertz \(2012\)](#), but according to [Blom-Hansen et al. \(2016\)](#) there is no effect in the Danish case, and at least not in jurisdictions with more than 100,000 inhabitants in Germany ([Roesel, 2017](#)). The French experience at stake appears as an outlier in this respect since local public spending has actually increased consecutive to the merger; [Frère et al. \(2014\)](#) had already shown that intermunicipal cooperation did not reduce public spending in that country. From a fiscal federalism viewpoint, [Oates et al. \(1972\)](#) decentralization theorem balances heterogeneity in citizens' preferences and externalities between jurisdictions, and states that "the provision of public services should be located at the lowest level of government encompassing, in a spatial sense, the relevant benefits and costs". Analyzing tax competition, [Breuillé and Zanaj \(2013\)](#) show that regional (resp. local) taxes should increase (resp. decrease) following the merger of regions by comparing pre- and post- merger equilibria in a two-tier territorial organization with local and regional authorities. This result is consistent with what has been implemented in

France by the decision maker: in practice, value-added contributions have been shifted from *départements* to regions after the merger.³

Second, this paper contributes to the identification of the impact of spatial determinants on subjective well-being.⁴ This task is all the more challenging as it requires observing individuals moving from one region to another, or seeing regions somehow changing when looking at the same individuals over time, which sounds even more difficult. From an econometric point of view, the former option requires to overcome the issue of endogenous location choice, which the latter option does not: the current paper exploits the unique 2016 merger of French regions in that very spirit. In the USA, [Oswald and Wu \(2010\)](#) showed that estimated state effects issued from subjective well-being equations were strongly correlated with objective measures of life quality, i.e., state rankings based on air quality, traffic, etc. As a result, these objective indicators do a fair job at capturing individual, subjective perception of life quality. In contrast, the concept of region used in this paper refers to an administrative region, net of all amenities that may be attached to that area. Other papers have wondered whether life was sweeter in the countryside: according to [Easterlin et al. \(2011\)](#), there is no marked difference between rural and urban areas in developed countries, while in developing countries cities are more frequently associated with higher reported levels of life satisfaction. Finally, other geographic determinants matter, including the price of gasoline ([Boyd-Swan and Herbst, 2012](#)) and housing costs ([Ala-Mantila et al., 2018](#)).

At the confluence of these two strands of literature, [Frijters et al. \(2004\)](#) exploit a wide-scale natural experiment, the German reunification, as an exogenous variation of income between West and East Germany to identify its causal impact on individual happiness. More recently, [Flèche \(2020\)](#) exploits an intertemporal variation of centralization reforms in Switzerland to identify their effect on subjective well-being; in particular, she acknowledges the role played by cultural attachment to one's region.

³Before the NOTRé law, local authorities (*départements*) received nearly one half of revenues issued from the *cotisation sur la valeur ajoutée des entreprises* (CVAE), a value-added contribution, while regions had exactly 1/4 of these revenues, the rest being granted to municipalities or intermunicipal communities. After the NOTRé law, the share of regions increased to one half while the *départements*' share decreased to slightly less than 1/4. The CVAE must be distinguished from the French VAT, an indirect tax with a regular rate of 20%, although both of them rely on the same tax base; the CVAE is progressive, with a 1.5% top marginal rate beyond €50m.

⁴See a recent survey by [Rentfrow \(2018\)](#) on that topic.

3 Institutional background

3.1 The administrative division of France

Contrary to the US, Germany or Switzerland, France is not a federation of states, Länder or cantons; it is unitary. Besides, it has always been a centralized country. The French territory is divided into metropolitan France and overseas.⁵ Metropolitan France is divided into several layers: regions, *départements* and municipalities which can gather into intermunicipal communities (EPCI). There are 96 *départements* in metropolitan France and more than 35,000 municipalities. Regions, *départements*, EPCI and municipalities constitute the administrative division of France, the so-called *mille-feuille*. This multi-level governance is often criticized for its presumed inefficiency in the public debate.

All local governments share various responsibilities in terms of education, public welfare, public transportation, economic development, youth, sports, etc. The exclusive areas of jurisdiction of regions include trains, upper secondary education as well as vocational training and apprenticeship. Regions are also bound to promote economic, social and cultural development. Finally, they are in charge of the management of European programmes, land planning, equality of territories, environmental issues, etc.

The so-called “first act” of decentralization in France dates back to the Deferre law voted in 1982. From that date onwards, the central authority transferred the executive power to the local authorities, namely the *départements*. The second part of this process occurred at the beginning of the 2000s when local authorities gained financial autonomy; regions then became responsible for upper secondary education, trains as well as vocational training and apprenticeship. The third act of decentralization is precisely the NOTRé law which included the merger of regions, and that aimed at enforcing the role played by regions: in particular, it increased the scope of their responsibilities. On the one hand, regions saw their tasks and powers widened, which indicates a higher level of decentralization. On the other hand, they were merged into larger entities, which can be seen as a trend toward centralization.

⁵Overseas are composed of five regions: French Guiana, Guadeloupe, Martinique, Mayotte, Réunion. The reform at stake did not require for those regions to merge.

3.2 The 2016 merger of regions

On January 14, 2014, President François Hollande announced a territorial reform that would eventually result in the creation of 13 new regions in order to replace the 22 existing ones. This substantial reduction in the number of regional jurisdictions led to reshape French subnational borders. The idea dated back to the *comité Balladur*, an administrative commission presided by former Prime Minister Edouard Balladur on the request of President Nicolas Sarkozy in 2008. At the time, that *comité* advocated for the creation of 15 super regions instead of the 22 current regions in metropolitan France, in order to meet European size standards (German, Italian and Spanish regions being on average larger). However, when Hollande made his announcement, it came as a surprise since the proposition was not part of his 2012 presidential campaign program. Hence the reform was completely unexpected. Hollande argued this reform would help simplify the complexity of administrative division, i.e., of the French *mille-feuille*. He wanted a swift legislative process: on June 3, 2014, he proposed the creation of 14 new regions instead of 22; two weeks later, the bill was under consideration by the Senate. However, the law was only adopted by the legislature, namely by the two chambers of the French Parliament, on July 16, 2015, because of strong opposition to the project. The Constitutional Council nonetheless indicated that the law was conform to the constitution on August 6, 2015. Finally, the #2015-991 law, also called *loi portant Nouvelle Organisation Territoriale de la République* (NOTRé law), which rules the current administrative division of France, was promulgated on August 7, 2015 and implemented from January 1st, 2016 onwards. Even though the merger was actively prepared for in 2015, in practice the new regions only became effective in the early days of 2016.

This reform is all the more unique as administrative borders had hardly changed over more than six decades in France, i.e., since their creation in 1956.⁶ There were many discussions from January to June 2014, a crucial phase during which those borders were still undetermined and when the government had to face many lobbyists. The public debate focused on the fear of losing one's regional identity; it opposed somehow “conservatives” and “liberals” in this respect: see, for instance, the various discussions on the borders of new regional entities and the possible

⁶The creation of regions dates back to a proposal by Serge Antoine, a magistrate at the French Court of Auditors (*Cour des Comptes*), i.e., the highest jurisdiction to audit and adjudicate accounts made by public, management, and government accountants.

“reunification” of Brittany and Pays de la Loire called for by some but rejected by others.⁷ According to many, citizens’ attachment toward their regions would be strong. Other issues are related to regionalism, including the teaching of regional languages and the autonomous status of Corsica. In the end, 13 new regions were created, the borders of which coincide neither with the initial project, nor with the one announced on June 3, 2014. Table 1 yields the correspondence between the 22 old regions (Figure 1a) and the 13 new regions (Figure 1b).

Importantly, the NOTRé law enforced the role played by regions in economic development by putting an end to the one played by *départements* in that domain. The priority has been given to promoting regional attractiveness through simplified administrative procedures for firms, for instance. It is therefore expected that this merger encouraged the promotion of local business development, hence that is enhanced local economic performance. Following the NOTRé law, regions obtained a higher share in local fiscal tax, namely indirect value-added taxes and contributions (remember footnote 3); nevertheless, the lump-sum transfer from the state decreased accordingly so as to keep regions’ revenues nearly constant.

The reform was designed to reduce interregional gaps by merging big, wealthy regions to small, poorer ones (Jouen, 2015). The boundaries of the new regions meet administrative proximity criteria, but the risk for merging regions to be excluded in 2020 from the “transition region” category (i.e., regions whose GDP per capita is between 75% and 90% of the EU average) exists, especially in case of some change in the NUTS 2 nomenclature:⁸ the eligibility for European funds is tailored to that level. However, this status is unpopular with respect to other governments than France within EU. On the one hand, since poorer regions are merged with wealthier regions, the economic situation of the former is expected to improve; on the other hand, this might precisely lead to lose eligibility to European Structural Funds (European Regional Development Fund or ERDF, and European Social Fund or ESF) based on convergence indicators at the EU-25 level).⁹ Though the former trade-off results in an ambiguous overall effect, some economic improvement is expected in the short run.

⁷ “Le sentiment d’appartenance est plus fort en Bretagne qu’ailleurs”, interview in *Libération*, 15 juillet 2014.

⁸NUTS is a geocode standard for referencing the subdivisions of countries for statistical purposes. NUTS 2 comprises areas with a population of 800,000 to 3 million people. For more details on that issue, see Antunez et al. (2017).

⁹French regions, including overseas, received b€12.5 from the EU over the 2007-2013 period.

4 Data

Following the recommendation of the [Stiglitz et al. \(2009\)](#) commission, France has started to ask individuals directly how they felt about their lives. The French institute of statistics and economic studies (Insee) is in charge of the SRCV survey targeting about 10,000 households every year. From 2010 onwards, it has included several questions related to individual life satisfaction, job satisfaction, as well as satisfaction with family and friends. On top of these measures of subjective well-being, it provides usual information at the individual level: gender, age, education, occupation, family status, labor force status and the administrative region where individuals live. Income is measured at the household level; in what follows, I consider the logarithm of the CPI-deflated annual household income, i.e., the sum of real incomes from all members in the household divided by the number of units of consumption as defined by the OECD scale.¹⁰ As another interesting feature of SRCV, in theory, this survey is able to track individuals even when they move between two waves.

According to the unit that produces the SRCV survey at Insee, the reliability of the longitudinal part related to subjective well-being casts doubts before 2013, though the survey started in 2010. Indeed, the questionnaire was modified in 2013: questions relative to life satisfaction were placed *after* those relative to income, while the reverse held before. *De facto*, a break in the time series of life satisfaction can be observed from that date. Hence I assume that there is no reliable wave before 2013 as far as life satisfaction is concerned, and I focus on the 2013-2018 period.

Table 2 contains some descriptive statistics related to the working sample, an unbalanced panel of 32,872 individuals (90,734 observations) followed from 2013 to 2018 and whose annual income exceeds €1. A measure of subjective well-being is available thanks to a question on overall satisfaction with life, the answer being provided on a discrete 0-10 scale. The answers have an average of 7.18 and are rather concentrated around levels 7 and 8 (see Figure 2), they nevertheless use the whole support of the distribution and they have a cross-sectional coefficient of variation as small as .24. Women and elders are slightly over-represented (58%

¹⁰According to this scale, the first adult in the household has weight 1, the other adults or children aged at least 14 have a weight equal to .5, and children aged less than 14 have a weight equal to .3.

of the sample aged 53.5 on average), which is usual in French household surveys. The average income amounts to nearly €25,000 per year; besides, income exhibits sizeable dispersion since its coefficient of variation is roughly 1.24, and the top 1% earns more than €86,000 a year. Finally, objective indicators measuring life quality are available in the survey, including the exposure to psycho-social hazard, health problems, environment troubles, poor living conditions, (economic or general) insecurity as well as the weakness of social ties. Between 3% and 15% of individuals suffer from at least one of such problems.

5 Identification strategy

To identify the causal impact of regional belonging on individual subjective well-being, two options are available to the researcher: either she may restrict her attention to movers, i.e., to individuals who move from one region to another; or she may exploit natural experiments that provide a region change, focusing by contrast on stayers, i.e., individuals who stay in the same region, *a priori* more numerous than movers. The former strategy looks fragile: first, it relies on a possibly small sub-sample of individuals; second, there may be various, possibly endogenous reasons which provide incentives for individuals to move, e.g., endogenous spatial sorting, which might be correlated with unobserved determinants of subjective well-being. For instance, the prior that “the grass is always greener on the other side of the fence” can be strong among movers; think also of individuals who are unsatisfied with their life because they are unemployed and who look for job opportunities outside their region of residence. The latter source of identification is extremely rare in practice.

The identification strategy of this paper consists in exploiting the merger described previously as a natural experiment. Since the reform was unexpected, strategic individual behavior is hardly at stake.¹¹ The reform thus provides a control group made up of individuals living in the six regions that remained unaffected by the merger: Brittany, Corsica, Île-de-France, Pays de la Loire, Provence-Alpes-Côte d’Azur, on top of the Centre region that had only been renamed Centre-Val de Loire on January 17, 2015. This control group is composed of individuals who

¹¹158 individuals (about .9% of the sample) are observed living in two former regions or more from 2013 to 2018. I nevertheless provide a robustness check with respect to endogenous location choice in the Online Appendix.

constitute 45% of the working sample.

Different treatment groups shall be considered, depending on the research question: individuals living (i) in merging regions, or (ii) in absorbed regions.¹² Note that merging regions and non-merging regions form a partition of metropolitan France. Besides, 16 regions merged during the process and became 7 new regions. The first identification strategy relies on a comparison of the evolution of life satisfaction in merging regions with that of non-merging regions. However, there are reasons to believe that merging regions are heterogeneous from demographic, economic, geographic and social viewpoints; in particular, they are made up of absorbed and absorbing regions which form a partition of merging regions (when defined in a broad sense, see *infra*).¹³

Among merging regions, some are smaller from an economic perspective and may be viewed as being absorbed. I define absorption in a conservative manner: absorption implies that administrative issues and local politics are held in a prefecture that is located in another region after the merger. Absorbed regions then include Auvergne, Champagne-Ardenne, Limousin, Lorraine, Picardy and Poitou-Charentes. According to that definition, 18 % of individuals lived in an absorbed region before the merger.¹⁴ A second identification strategy relies therefore on the comparison of the evolution of life satisfaction in regions that have been absorbed with the one that prevailed in regions that did not merge. Another 18 % of individuals live in an absorbing region (Alsace, Aquitaine, Nord-Pas-de-Calais, Rhône-Alpes), i.e., in a region that has absorbed another region (and possibly other regions when the merger involved more than two regions). By definition, in such regions, not only has the old prefecture remained a prefecture, but it has also become the one of the new regional entity.

What about the other regions? Some did merge, but the governance of the new region has been split between the former regions. Burgundy and Franche-

¹²Absorbing regions may well be considered, too. The corresponding estimates are available in the supplementary Appendix, see Tables 16 and 17. Overall, no significant impact is found in those regions.

¹³Merging regions differ from non-merging regions in that they are significantly poorer: in 2013, individuals living in the latter report on average an annual income of €26,700 as opposed to €24,400 for residents of the former. The difference is statistically significant at 5%, allowing for unequal variances within the two groups.

¹⁴Absorbing and absorbed regions look quite similar in terms of observable characteristics but absorbed regions are significantly poorer (€23,900 on average against €24,800 in 2013). Once again, the difference is statistically significant at 5%.

Comté, Lower Normandy and Upper Normandy as well as Languedoc-Roussillon and Midi-Pyrénées are concerned. The leading regions are Burgundy, Upper Normandy and Midi-Pyrénées in the sense that the prefectures of the new regions correspond to these former regions'. In a conservative approach, I exclude such regions from the analysis, but I provide robustness checks with respect to that choice by including them as well in the treatment group (see section 8.1 on that issue). Under the latter empirical strategy, we are bound to consider 9 absorbed regions instead of 6 (corresponding to 7 absorbing regions instead of 4). Absorbed (resp. absorbing) regions defined under this broader sense account for 26 % (resp. 29 %) of individuals.

As usual in difference-in-difference frameworks, the identification assumption is that individuals would have experienced a similar evolution of subjective well-being in both control and treatment groups, had the merger not happened. This common trend assumption cannot be tested, but its plausibility can be assessed. To that purpose, Figures 3 and 4 depict the evolution of life satisfaction in both control and treatment groups (for the two treatment groups considered). As far as pre-trends are concerned, it is not possible to disregard any of the empirical strategies presented above (see also section 8.3 on that issue). Interestingly, while life satisfaction has remained rather stable after the reform in non-merging regions, or has even slightly increased, there is a sharp rise in self-assessed subjective well-being in absorbed regions (from about 7.12 in 2015 to 7.26 in 2017). This empirical evidence suggests that the impact of the merger on life satisfaction was positive for individuals living in absorbed regions, but not significant for those living in merging regions. The next section provides an econometric model that enables to check whether this empirical finding holds *ceteris paribus*.

Last, even if the natural experiment at stake guarantees that the timing of subnational border changes can be considered as exogenous, a remaining concern has to do with endogenous border drawing, namely the possibility that the border design was made in order to minimize discontent. Put differently, one may worry about selection into treatment. However, this would not threaten internal validity as long as the previous difference-in-difference assumptions are valid. It must be acknowledged, though, that external validity is more challenging, and that the results obtained in this particular setting would not generalize easily to other contexts.

6 Econometric specification

A difference-in-difference strategy to evaluate the impact of the merger viewed as a natural experiment is implemented using a linear model. Since the dependent variable, self-assessed life satisfaction, is ordinal, it is tempting to resort to ordered models, which is done in section 8.4. However, for the sake of clarity, I consider rather a linear model and obtain similar conclusions: first, the signs of coefficients are nonparametrically identified, and second, both the significance and the magnitude of average marginal effects are close regardless of the specification chosen. Moreover, the linearity makes it simpler to include individual fixed-effects, hence to control better for unobserved heterogeneity. Even though it is possible to include fixed-effects in ordinal models,¹⁵ this is a more computationally demanding task. There are serious reasons to believe that unobserved heterogeneity is a first-order issue as far as subjective well-being is concerned, which claims for controlling for individual fixed-effects.

The estimating equation as regards the subjective well-being (SWB) of individual i living in region r on year t is:

$$\text{SWB}_{irt} = \beta \text{Treatment}_r \times \text{Post}_t + X'_{irt}\gamma + \alpha_i + \delta_t + \mu_r + \varepsilon_{irt}. \quad (1)$$

Explanatory covariates include a number of usual determinants of life satisfaction X_{irt} (objective indicators of life quality, labor force status, family status, income, age, education, gender, occupation), individual fixed-effects α_i , year dummies δ_t and regional dummies μ_r . The average treatment effect (ATE) β is recovered by the coefficient related to the interaction of the treatment dummy Treatment_r , defined at the regional level, with a dummy for the post-reform period, i.e., from 2016 onwards. Idiosyncratic shocks ε_{irt} follow a normal distribution. Standard errors are clustered at the region-year level: following [Bertrand et al. \(2004\)](#), the ideal level would be the level of intervention, namely the former region; however, this would yield a small number of clusters (less than 50), which would cast doubts in a consistent estimation of the variance-covariance matrix. Section 8.5 provides robustness checks with respect to the level of clustering.

The identification of the model is achieved by assuming strict exogeneity of the covariates conditional to the individual effects. This exogeneity follows from the

¹⁵See the Appendix on this topic.

reform being a natural experiment.

A remaining issue is the selection of controls X_{irt} in the estimating equation. First, the literature devoted to the individual determinants of subjective well-being provides some guidance (see [Layard et al., 2015](#), on that topic). Second, statistical methods based either on the BIC, on the rigorous Lasso, or on a stepwise algorithm provide useful tools to select the most relevant variables. In practice, both the literature and statistical criteria suggest that relevant covariates include objective measures of life quality, labor force status, family status, income and age; for the sake of completeness, education, gender and occupation dummies are also added up in the model.

7 Results

Tables 3 and 4 show the results obtained from the main specification, i.e., estimating equation (1) for the two treatment groups: merging and absorbed regions, the latter definition being conservative. For the sake of readability, only the ATEs are reported in these tables. The other estimates $\hat{\gamma}$ are available upon request, but it is worth emphasizing that they are completely consistent with the huge empirical evidence provided by the literature devoted to subjective well-being. For instance, objective deprivation indicators related to life quality have a strong, depressing impact on life satisfaction; so does unemployment. By contrast, and even though money can't buy happiness, it definitely contributes to it: the correlation between income and life satisfaction is strong and positive. Having a partner increases life satisfaction; education, gender and occupation have hardly any effect. Moreover, due to large sample size, this vector is very precisely estimated so that there is little doubt that the previous stylized facts hold in the data.

Turning now to the causal effect of the merger, one cannot reject the null hypothesis that the average treatment effect is equal to zero at the 5% level. This empirical evidence suggests that the merger of regions at stake has not hurt individuals' attachment to their region, if anything, despite the presumed fear of losing one's regional identity that has been put forward in the public debate.

Interestingly, the main lesson from Table 4 is that the merger has significantly increased the life satisfaction of individuals living in absorbed regions. This result is robust to the inclusion of controls and still holds after taking unobserved

heterogeneity into account thanks to individual fixed-effects. The point estimates of the ATE, which are all positive and significantly different from 0, correspond to an increase in self-assessed life satisfaction that lies slightly below .1 on the 0-10 scale, more precisely near .076 in the preferred specification from column (6) that includes both individual fixed-effects and controls. In other words, everything happens roughly as if 7.6% of individuals report an annual change in their life satisfaction of +1. Put differently, this effect amounts to 4.4% of one standard deviation. It also contributes to attenuate the depressing impact of poor health conditions by 7.5%; equivalently, individuals are better off as if they received about .3% more income.

It is worth emphasizing that the source of identifying variability is at the regional level (as opposed to the individual level). Aggregating the data at that level leads to an estimated ATE of .084 which is still significant at 5%.

Last, I investigate whether the effects documented previously are heterogeneous across individuals. In a triple difference approach, I specify now $\beta \equiv \beta_0 + \beta_X X_{irt}$ in equation (1). Table 5 tests whether some individual characteristics tend to amplify or attenuate the previous effects, i.e., $H_0 : \beta_X = 0$. It turns out that blue collars saw their self-assessed happiness increase most in the absorbed regions: the estimated effect for them amounts to three times the ATE; the same holds for singles and individuals holding a vocational degree with an estimated effect that is about twice the ATE. Finally, the effect is also more pronounced for individuals belonging to the third decile of the income distribution. On the whole, all these individuals may be more mobile on the job market; the heterogeneity depicted here sounds rather consistent with economic gains in absorbed regions (see section 9 on that issue) provided that low-qualified workers' mobility is higher.

8 Robustness checks

This section provides several robustness tests in order to check that previous results are truly causal and do not stem from statistical artifacts. First, both alternative treatment and control groups are considered, which helps determine the sensitivity to different common trend assumptions. Second, falsification tests are performed: placebo experiments taking the form of fake reforms occurring before the actual reform are simulated. Third, the sensitivity of the results is assessed with respect

to the parametric specification. Finally, from a statistical viewpoint, these results seem to be driven neither by attrition, nor by survey sampling issues, and they are robust to the clustering level.

8.1 Alternative treatment group

A less conservative definition of "absorbed regions" is considered here. It now includes the former regions that share some governance within new regional entities, e.g., Franche-Comté, Lower Normandy and Languedoc-Roussillon. On top of providing with a sensitivity analysis, this empirical strategy relies on a larger sample size and yields even more precise estimates. Table 7 shows that the merger has still a positive effect on absorbed regions defined in a broader sense, though slightly attenuated with respect to previous estimates.

In the remainder of that section, an individual belongs to the treatment group if she lives in a region that has been absorbed in the most conservative sense.

8.2 Alternative control groups

Another robustness check consists in verifying that the ATE does not depend too much on the definition of the control group. A potential concern is related to the effect being driven by the Île-de-France region which remained unaffected by the merger, and which looks somehow like an outlier with respect to the other regions (because it is both the richest and the most crowded region due to the Paris area). To address this concern, I exclude Île-de-France from the control group. The results are displayed by Table 8: reassuringly, the estimated ATE turns out to be very close to the one found in Table 4.

Another concern could be that the control group, including Île-de-France or not, is not comparable with the treatment group in the sense that both the outcome and its predictors might not be similar before the reform. A standard solution to fix this potential problem consists in resorting to a synthetic control approach (Abadie et al., 2010), which yields Figure 8. According to that method, the short-run effect documented above would amount to .06 in 2016, to .135 in 2017 but would vanish in 2018.

8.3 Placebo experiments

Two placebo experiments, namely fake mergers in 2014 and 2015, are simulated. These counterfactual reforms must take place before the actual reform in 2016. If the common trend assumption holds with the actual merger, it should also be verified under these falsification tests. The results are provided by Tables 9 and 10. No significant effect is found, which is consistent with an absence of differential pre-trend in subjective well-being across control and treatment groups. Hence it gives further credit to the idea that the slight improvement in self-assessed life satisfaction reflects a causal effect of the merger. If anything, the estimated ATE under a fake 2015 merger is higher than the one corresponding to a fake 2014 merger (none of these effects being significant at usual levels), which could be consistent with some anticipation of the merger during the year 2015.

A remaining concern is related to possible endogenous selection into treatment: non-merging regions like Brittany, Corsica, but also Île-de-France, Pays de la Loire and Provence-Alpes-Côte d’Azur to a lesser extent would be precisely the ones in which regional attachment is higher. Importantly, this would not be a problem for internal validity as long as the common trend assumption is valid.¹⁶

8.4 Parametric specification

Due to the ordinal nature of the dependent variable, it is worth investigating how the estimated ATEs depend on the choice of the functional form relating life satisfaction to observed covariates. Two nonlinear models are thus estimated: an ordered Probit (Table 11) and an ordered Logit (Table 12).¹⁷ Results hardly vary from a qualitative point of view: the effect in absorbed regions remains positive and significant at usual levels. The average partial effects (APE), available upon request, are consistent with the ATE obtained under the linear specification, nearly .08: the probabilities of reporting a level of life satisfaction higher than 8 (resp. comprised between 5 and 7) increase (resp. decrease) by roughly .1 pp.

¹⁶On top of that, the estimations based on a control group composed of individuals living in Centre-Val de Loire yield an ATE that does not differ much from previously.

¹⁷The Appendix contains a discussion with respect to the inclusion of individual fixed-effects in such models.

8.5 Statistical issues, clustering and survey sampling

Finally, I investigate whether my results are robust to statistical concerns, essentially: (i) endogenous attrition, (ii) panel balancing, and (iii) clustering.

To test for endogenous attrition, I resort to the method suggested by [Verbeek and Nijman \(1992\)](#) which consists in considering further explanatory covariates (the number of times an individual is present in the panel and a dummy that indicates whether the individual belongs to the balanced panel) in the model estimated on the unbalanced panel, and in testing for their significance. Table 13 indicates that endogenous attrition is not too much of a concern here: these variables are not significant at usual levels and thus the corresponding null hypotheses cannot be rejected.

Table 14 provides estimates on the balanced panel composed of a much smaller number of individuals (1,417 instead of 18,187) who are always present in the survey from 2013 to 2018. The estimated ATE turns out to be close to the one estimated previously, and even higher: about .1.

Last, I wonder whether the above empirical findings are robust to alternative levels of clustering: individual, individual-year, (former) region, (new) region and (new) region-year. The baseline specification is issued from clustering at the former region-year level: even though standard practice suggests to opt for the level of policy intervention, i.e., former regions, a small number of corresponding clusters (namely, 22) challenges that empirical choice in this setting. Table 15 shows that the estimated standard errors vary from .02 to .04 depending on the considered level of clustering, but the significance of the estimated ATE at usual levels remains, provided that the usual condition of disposing of at least 50 clusters (see, e.g., [Cameron and Miller, 2015](#)) is fulfilled in order to estimate consistently the asymptotic variance-covariance matrix.

9 Interpretation

This section is devoted to an empirical investigation of the mechanisms underlying previous results. In particular, I try to find out which regional attributes might have changed consecutive to the merger that help rationalize, first, why there is no negative impact; second, why there is even an improvement in absorbed regions,

especially for the subpopulation described at the end of section 7. By construction, official statistics are available in former regions before the merger, in new regions after the merger, but not in former regions after the merger. An exception is the unemployment rate since Insee provides series at the *département* level; these figures can easily be aggregated to the former region level -or not. In the other cases, the absence of data prevents me from determining whether some features have evolved differently in absorbed and in absorbing regions; I focus therefore on a comparison between merging and non-merging regions only, which is already sufficient to provide some explanations about the absence of negative impact of the merger.

A first clue to understand the impact of that merger is provided by the unemployment rate measured at the *département* level. Column (1) of Table 6 shows that the policy change would have lowered it significantly by .19pp (about 2%); this decline in the unemployment rate is observed whatever the level of aggregation considered, i.e., either at the former region or at the new region levels. This empirical evidence suggests that the merger has created new economic opportunities which led to better prospects on local job markets. This is especially the case in absorbed regions: according to Column (2) of Table 6, the unemployment rate has decreased in those regions by .18pp consecutive to the merger and with respect to the regions that did not merge. Interestingly, the impact is null in absorbing regions (column (3)). Since absorbed regions are poorer, this improvement of the economic climate has likely concerned the subpopulation described above (singles, blue collars, with some vocational degree, located in the bottom of the income distribution) who may move more easily and may be more prone to benefit from these new prospects. Indeed, a plausible channel for this improvement of local labor market conditions is the facilitation of commuting within the new regional boundaries; in particular, regions' scope includes trains, hence commuting could have been made easier thanks to better rail transportation. Remember that the new regions were bound to promote local economic performance, especially by simplifying administrative procedures for firms, which might have favored local business development; as a plausible spillover of that merger, poorer, absorbed regions might have benefited from the promotion activities of the richer, absorbing regions. This empirical evidence of a decline in the unemployment rate is also consistent with [Wolfschuetz \(2020\)](#) who finds some improvement in local economic performance as measured by the unemployment rate, which she relates to further

promoting of local business development in municipalities entering intermunicipal cooperation in Germany.

A second point has to do with unintended effects of the merger on public spending. The French Supreme Court of Auditors, the *Cour des Comptes*, in charge of an audit of public funds, worries about pecuniary costs consecutive to the merger. According to *Cour des Comptes* (2019), efficiency gains have not been visible so far: on the contrary, and by comparison with non-merging regions, pecuniary costs have increased due to a convergence in civil servants' bonuses based on the rule of the most favorable cases, an update of IT systems, etc. Moreover, merging regions raised their investment spending while non-merging regions hardly increased them at all over the post-merger period from 2016 to 2018. From data issued by the DGCL, the bureau in charge of statistics on local authorities, Figure 5 can be obtained, which replicates Figure 9 p133 of the report established by *Cour des Comptes* (2019) –except that the perimeter of non-merging regions does not include overseas in my sample. Not only did the merger generate no economies of scale, but it resulted in an *increase* of public spending: see Figure 6, which is also confirmed by Column (4) of Table 6. Part of that money might have translated into higher subsidies granted to the French railroad monopoly (SNCF) since regions are in charge of train transportation, but Column (5) of Table 6 concludes to no significant impact in this respect. It is therefore striking that, in a theoretical vision *à la* Alesina and Spolaore (1997), two different forces push in the same direction: (i) reinforced centralization, which should increase citizen discontent, and (ii) the unexpected increase in local public spending, hence in future local taxes, which should also be disliked by a population of tax payers: the "fiscal effect" –a wealth effect. In contrast, benefits from the merger can be mostly found in the economic spillovers documented previously, and in a "residential effect" related to the fact that citizens are not only tax payers, but also residents: hence they may benefit from an extended provision of local public goods following the previous increase in public spending.

Related to the latter point, I wonder whether local tax revenues have changed consecutive to the merger. Indeed, some increase in local public spending should come along with (at least future) higher tax pressure. However, I find no empirical evidence of significant variation in this respect (see Column (6), Table 6). As a consequence of the rise in public spending and the stagnation of fiscal revenue,

financial debt ratios have increased.

Last, another explanation could have trait to some income convergence in the same spirit as what occurred after the German reunification ([Frijters et al., 2004](#)). However, consistent with the rationale, the merger at stake does not involve regions which were as heterogeneous in terms of GDP per capita as were West and East Germany. On top of that, Figure 7 suggests that the common trend assumption does not hold for individual income, and that the effect, if any, would be small. In other words, using an instrumental variable strategy here to identify the causal effect of income on happiness based on the merger as an exogenous source of variation of the former would require to overcome a weak instruments problem.

10 Conclusion

This paper has assessed the causal impact of the 2016 merger of French regions on individual subjective well-being. I exploit the NOTRé law as a natural experiment: this reform reduced the number of regions from 22 to 13. Contrary to what the public debate suggested at the time by putting much emphasis on citizens' regional attachment, and contrary to other empirical findings on the relationship between centralization and life satisfaction, I find no significantly depressing impact of that merger. The merger had even a positive effect in the economically smaller (and poorer) regions that were likely to benefit most from attractiveness gains; in particular, the unemployment rate declined more rapidly in merging regions, which might have benefited to a subpopulation of mobile workers.

This empirical evidence suggests therefore that economic spillovers help explain citizen's attitude towards changes in subnational borders on top of the expected trade-off between economies of scale and heterogeneity of preferences. Indeed, in the current context, the reduction in the number of regions was synonymous of both stronger centralization and higher integration costs. Besides, this analysis proves fruitful in answering concerns related to the presumed "loss of regional identity": the data at stake does not favor such a hypothesis. Regional belonging might be oversold in the public debate; at the very least, economics is likely to outweigh such a cultural norm. More precisely, given the three theoretical mechanisms following the merger (efficiency, increased heterogeneity and loss of identity), this paper has shown that there was no evidence of economies of scale, that the latter

channel was not likely to prevail and that the increased heterogeneity has not been detrimental to absorbing (richer) regions while benefiting to absorbed (poorer) regions, converging to the richer ones.

Given that the results obtained here look somehow different from those obtained in other settings (e.g., federal states), this research provides empirical evidence of the role played by the institutional context in this matter. It is also of some guidance to the policy-maker about mergers of other local authorities, including municipalities. Further research is however needed in order to quantify the redistributive effects at stake between losers and winners of such a policy change within absorbing and absorbed regions.

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Figures



(a) Old regions (before the merger)



(b) New regions (after the merger)

Figure 1: Administrative division of France

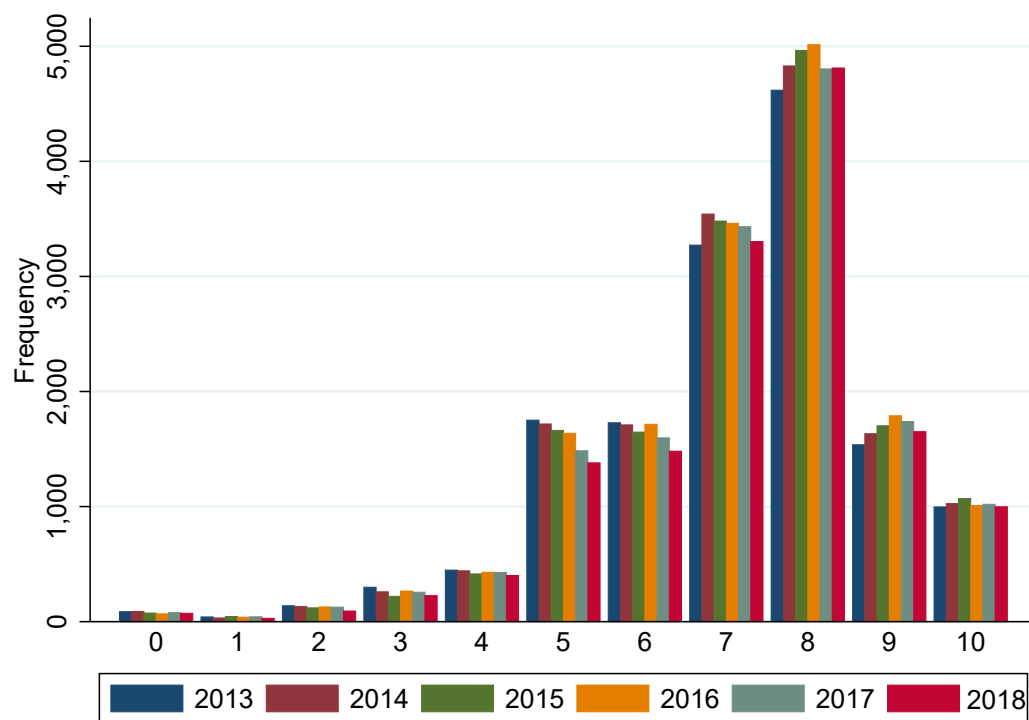


Figure 2: Evolution of life satisfaction

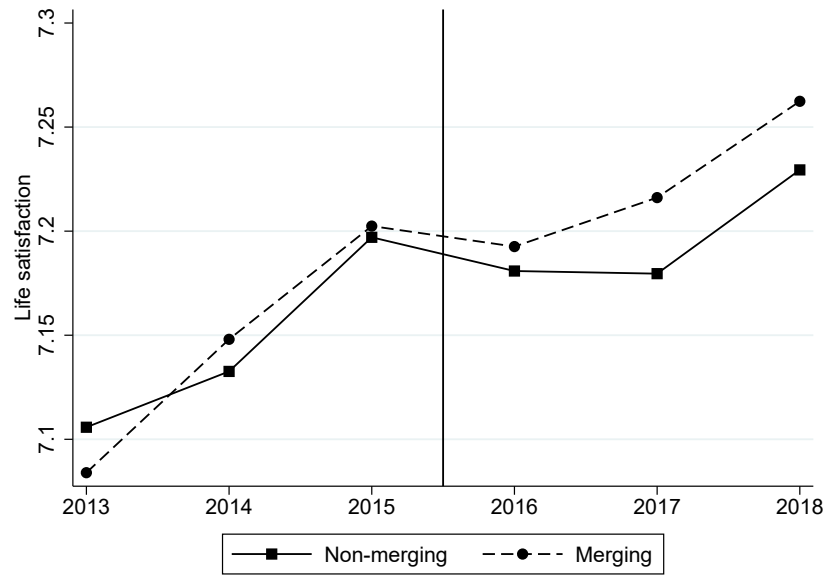


Figure 3: Evolution of life satisfaction across regions

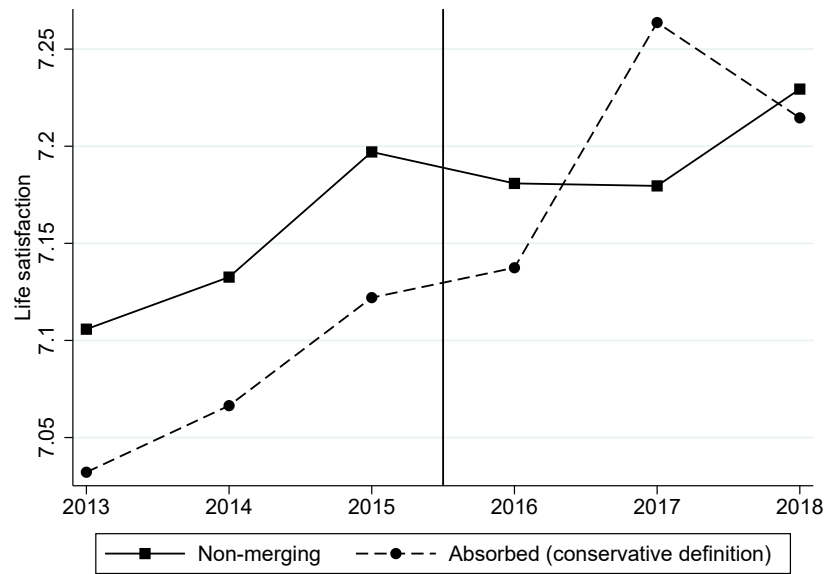


Figure 4: Evolution of life satisfaction across regions

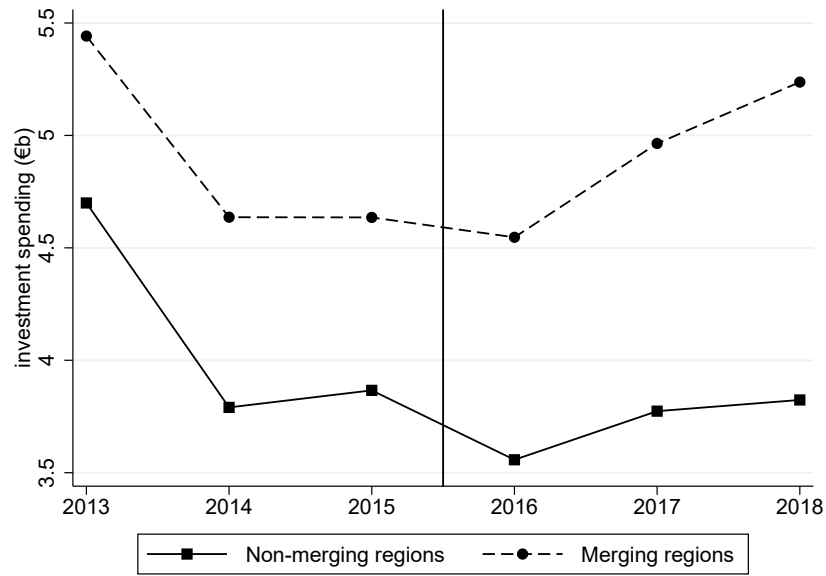


Figure 5: Evolution of investment spending across regions

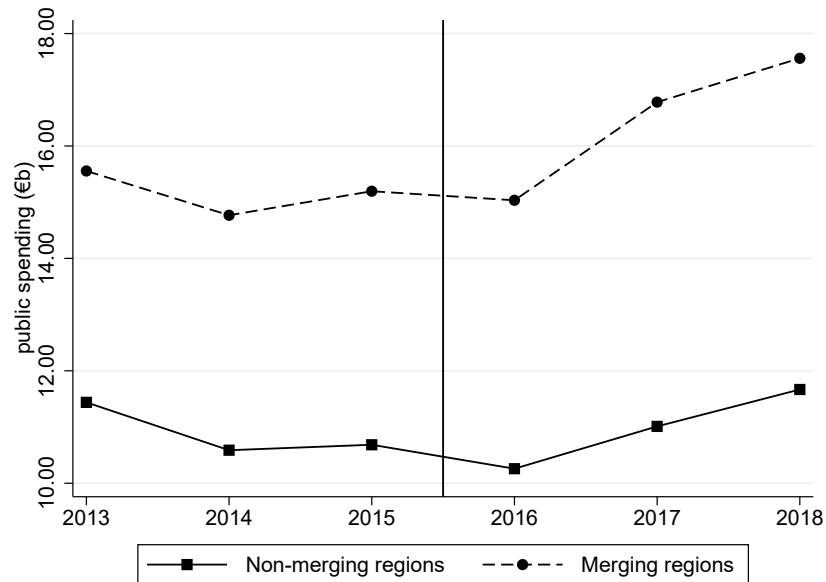


Figure 6: Evolution of total public spending across regions

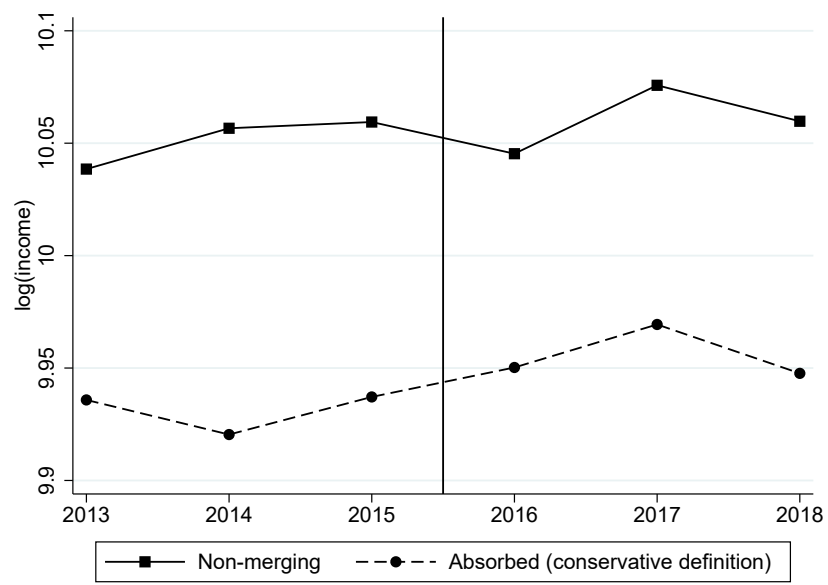


Figure 7: Evolution of income across regions

Tables

Table 1: Correspondence between old and new French regions

New region	Old region
Auvergne-Rhône-Alpes	Auvergne
	Rhône-Alpes
Bourgogne-Franche-Comté	Burgundy
	Franche-Comté
Brittany	Brittany
Centre-Val de Loire	Centre
Corsica	Corsica
Grand Est	Alsace
	Champagne-Ardenne
	Lorraine
Hauts-de-France	Nord-Pas de Calais
	Picardy
Normandy	Lower Normandy
	Upper Normandy
Nouvelle-Aquitaine	Aquitaine
	Limousin
	Poitou-Charentes
Île-de-France	Île-de-France
Occitanie	Languedoc-Roussillon
	Midi-Pyrénées
Pays de la Loire	Pays de la Loire
Provence-Alpes-Côte d’Azur	Provence-Alpes-Côte d’Azur

Table 2: Summary statistics

	mean	sd	min	max
Life satisfaction	7.18	1.72	0	10
Female	0.58	0.49	0	1
Age	53.5	17.6	16	101
Income	25,495	31,585	4	4,468,733
Status of the region wrt merger				
Absorbed	0.26	0.44	0	1
Absorbed (conservative)	0.18	0.38	0	1
Absorbing	0.29	0.45	0	1
Absorbing (conservative)	0.18	0.39	0	1
No merger	0.44	0.50	0	1
Education				
No degree	0.20	0.40	0	1
High-school	0.29	0.45	0	1
Vocational	0.31	0.46	0	1
College	0.16	0.37	0	1
Other degree	0.04	0.19	0	1
Labor force status				
Employed	0.47	0.50	0	1
Unemployed	0.06	0.24	0	1
Student	0.03	0.18	0	1
Inactive	0.05	0.23	0	1
Retired	0.36	0.48	0	1
Undetermined	0.02	0.12	0	1
Occupation				
Clerk	0.27	0.44	0	1
Farmer	0.03	0.16	0	1
White collar	0.13	0.34	0	1
Self-employed	0.06	0.23	0	1
Intermediate	0.22	0.41	0	1
Blue collar	0.18	0.38	0	1
Other	0.11	0.32	0	1
Undetermined	0.01	0.07	0	1
Family status				
Single	0.22	0.41	0	1
Two adults, w/o child	0.38	0.49	0	1
Two adults, 1 child	0.09	0.29	0	1
Two adults, 2 children	0.12	0.32	0	1
Two adults, 3+ children	0.05	0.23	0	1
Single parent	0.05	0.21	0	1
Others w/o child	0.05	0.22	0	1
Others with children	0.03	0.18	0	1
Undetermined	0.01	0.09	0	1
Quality of life				
Poor living conditions	0.11	0.31	0	1
Environmental troubles	0.04	0.18	0	1
Psycho-social hazard	0.12	0.33	0	1
Economic insecurity	0.03	0.16	0	1
Poor health	0.09	0.29	0	1
Insecurity	0.15	0.35	0	1
Weak social ties	0.14	0.35	0	1
Observations	90,734			

Source. French SRCV survey, 2013-2018.

Sample. Unbalanced panel of 32,872 individuals.

Table 3: Effect of the merger on life satisfaction (Treatment: Merging regions)

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment $\times$ Post	0.026 (0.019)	0.029* (0.018)	0.030 (0.018)	0.030 (0.018)	0.036* (0.020)	0.037* (0.019)
Controls	No	Yes	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual effects	No	No	RE	RE	FE	FE
# of observations	90,734	90,734	90,734	90,734	90,734	90,734
# of individuals	32,872	32,872	32,872	32,872	32,872	32,872
R^2	0.003	0.259	0.003	0.255	0.736	0.747

Source. French SRCV survey, 2013-2018, unbalanced panel.

Model. Linear model estimated by OLS.

Dependent variable. Life satisfaction on a 0-10 Cantril scale.

Controls. Income, age, gender, education, occupation, labor force status, family status, objective quality of life indicators.

Robust standard errors clustered at the region-year level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Effect of the merger on life satisfaction (Treatment: Absorbed regions)

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment $\times$ Post	0.082*** (0.027)	0.084*** (0.025)	0.076*** (0.027)	0.072*** (0.026)	0.083*** (0.031)	0.076** (0.031)
Controls	No	Yes	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual effects	No	No	RE	RE	FE	FE
# of observations	49,279	49,279	49,279	49,279	49,279	49,279
# of individuals	18,187	18,187	18,187	18,187	18,187	18,187
R^2	0.004	0.266	0.004	0.262	0.743	0.753

Same legend as Table 3.

Table 5: Heterogeneity of effects (Treatment: Absorbed regions)

Treatment $\times$ Post $\times$ Vocational	0.163*** (0.039)
Treatment $\times$ Post $\times$ Blue collar	0.215*** (0.078)
Treatment $\times$ Post $\times$ 3rd decile (income)	0.127*** (0.055)
Treatment $\times$ Post $\times$ Single	0.176*** (0.051)
Controls	Yes
Time FE	Yes
Region FE	Yes
Individual FE	Yes
# of observations	49,279
# of individuals	18,187
Same legend as Table 3.	

Table 6: Effect of the merger on other outcomes

	unemployment rate			public spending	train spending	local tax revenue
	(1)	(in pp) (2)	(3)	(in €b) (4)	(in €b) (5)	(in €b) (6)
Merger $\times$ Post	-0.193*** (0.024)			0.171** (0.068)	0.028 (0.017)	0.033 (0.047)
Absorbed $\times$ Post		-0.177*** (0.029)				
Absorbing $\times$ Post			-0.043 (0.029)			
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Département FE	Yes	Yes	Yes	No	No	No
Region FE	No	No	No	Yes	Yes	Yes
# of départements	96	53	48			
# of new regions				13	13	13
Observations	576	318	288	78	78	78
R ²	0.996	0.994	0.995	0.983	0.967	0.857

Appendix

Ordered models with random effects

The difference-in-difference strategy adopted in this paper to evaluate the reform viewed as a natural experiment can be embedded within an ordered model. The unobserved (or latent) life satisfaction SWB_{irt}^* of individual i living in region r on year t can be modelled as:

$$\text{SWB}_{irt}^* = \beta \text{Treatment}_r \times \text{Post}_t + X'_{irt} \gamma + \alpha_i + \delta_t + \mu_r + \varepsilon_{irt}. \quad (2)$$

The observed life satisfaction SWB_{irt} , available on a discrete 0-10 scale, can be related to the latent variable through:

$$\text{SWB}_{irt} = k \iff \text{SWB}_{irt}^* \in [s_k, s_{k+1}[, \quad \forall k \in \llbracket 0, K \rrbracket,$$

or equivalently

$$\text{SWB}_{irt} = \sum_{k=0}^K k \mathbb{1}\{s_k \leq \text{SWB}_{irt}^* < s_{k+1}\}, \quad (3)$$

where $s = (s_1, \dots, s_K)$ is a vector of unknown thresholds to be estimated, $s_0 = -\infty$, $s_{K+1} = +\infty$ and $K = 10$.

The notations are the same as above, but instead of allowing for fixed-effects, I impose a supplementary parametric restriction: $\alpha_i \sim \mathcal{N}(0, \sigma_\alpha^2)$, i.e., I allow for random effects only (see *infra*). Idiosyncratic shocks ε_{irt} may follow either a logistic distribution with mean 0 and variance $\frac{\pi^2}{3}$ (Logit), or the standard normal distribution (Probit).

The identification of the model is achieved by assuming strict exogeneity of the covariates conditional to the individual effects. Once again, this exogeneity follows from the reform being a natural experiment. Nevertheless, two normalizations are now required for the joint identification of agents preferences and of unknown thresholds viewed as incidental parameters: (i) location: $\gamma_0 = 0$, for shifting the constant and the thresholds simultaneously by some constant yields an observationally equivalent model, and (ii) scale: $\sigma_\varepsilon^2 = \pi^2/3$ (Logit) or $\sigma_\varepsilon^2 = 1$ (Probit), for multiplying the latent and all its parameters yields the same likelihood. Under these normalizations, the vector of parameters $\theta = (\beta, \gamma, s, \sigma_\alpha)$ is identified.

The estimation is performed by maximum likelihood; the estimator is consistent and asymptotically normal (CAN) as the number of individuals grows large even for small, fixed number of observations per individual. Two ways may still be ahead as far as the idiosyncratic shocks ε_{irt} are concerned, a normal distribution (Probit) or a logistic distribution (Logit). Empirically, the latter produces a better fit, i.e., yields a higher average log-likelihood (-1.61 against -1.63 in the specification including both controls and random effects), as shown by Tables 11 and 12.¹⁸ Even though point estimates cannot be compared, average partial effects (available upon request) can be: reassuringly, they turn out to be close to each other.

A last issue has to do with unobserved heterogeneity. Though identified, ordered Logits with fixed effects are not easy to estimate, since they involve a conditional approach based on an adequate sufficient statistics that requires a burdensome computation of the likelihood (see, e.g., Frijters et al., 2004, for such an estimation). As a supplementary drawback, this approach relies on a small sub-sample of individuals who have non-constant sequences of life satisfaction. Random effects are admittedly a second-best solution that requires a supplementary, parametric assumption on the form of unobserved heterogeneity, but the identification of the model is then based on the whole sample. As far as Probit models are concerned, there is no simple way to avoid the incidental parameters problem. In practice, an insight from the linear model is that fixed-effects (FE) and random-effects (RE) specifications yield close ATEs.

¹⁸Due to the fatter tails of the logistic distribution, the Logit model puts more weight on extreme events. Moreover, the Logit allows an interpretation in terms of odds ratios, which the Probit does not permit.

Robustness checks

Alternative treatment group

Table 7: Effect of the merger on life satisfaction (Treatment: Absorbed regions, less conservative definition)

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment $\times$ Post	0.029 (0.027)	0.044* (0.024)	0.051** (0.024)	0.046* (0.024)	0.065** (0.026)	0.060** (0.025)
Controls	No	Yes	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual effects	No	No	RE	RE	FE	FE
# of observations	57,198	57,198	57,198	57,198	57,198	57,198
# of individuals	21,064	21,064	21,064	21,064	21,064	21,064
R^2	0.003	0.267	0.003	0.263	0.743	0.753

Same legend as Table 3.

Alternative control groups

Table 8: Alternative definition of the control group (Treatment: Absorbed regions)

	(1)	(2)	(3)	(4)	(5)	(6)
Including Île-de-France	0.082*** (0.027)	0.084*** (0.025)	0.076*** (0.027)	0.072*** (0.026)	0.083*** (0.031)	0.076** (0.031)
# of observations	49,279	49,279	49,279	49,279	49,279	49,279
R^2	0.005	0.266	0.004	0.262	0.737	0.748
Excluding Île-de-France	0.086*** (0.031)	0.088*** (0.029)	0.079*** (0.029)	0.075** (0.030)	0.081** (0.034)	0.073** (0.034)
# of observations	38,167	38,167	38,167	38,167	38,167	38,167
R^2	0.004	0.266	0.004	0.262	0.743	0.753
Controls	No	Yes	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual effects	No	No	RE	RE	FE	FE

Same legend as Table 3.

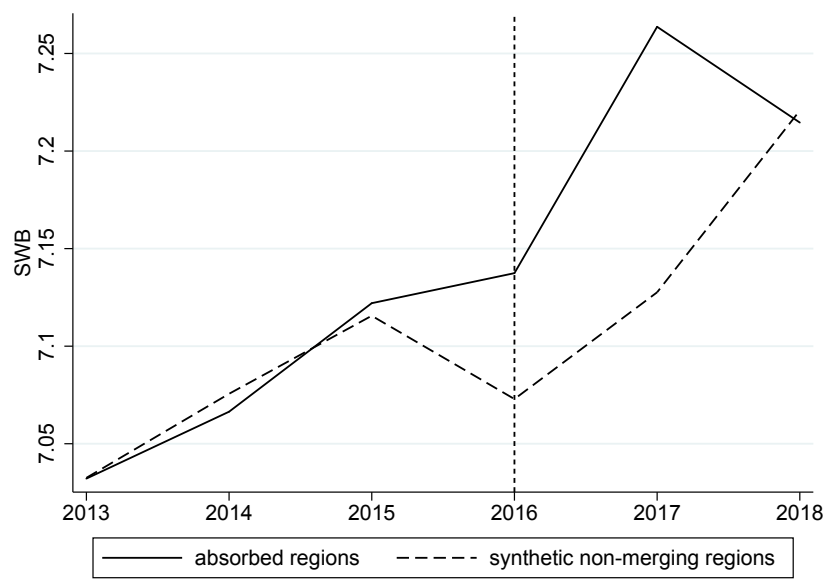


Figure 8: Synthetic control approach

Placebo experiments

Table 9: Placebo experiment: fake merger in 2014 (Treatment: Absorbed regions)

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment $\times$ Post	0.049 (0.039)	0.061* (0.033)	0.010 (0.035)	0.027 (0.029)	-0.003 (0.034)	0.001 (0.030)
Controls	No	Yes	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual effects	No	No	RE	RE	FE	FE
# of observations	49,279	49,279	49,279	49,279	49,279	49,279
# of individuals	18,187	18,187	18,187	18,187	18,187	18,187
R^2	0.004	0.266	0.004	0.262	0.743	0.753

Same legend as Table 3.

Table 10: Placebo experiment: fake merger in 2015 (Treatment: Absorbed regions)

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment $\times$ Post	0.058* (0.029)	0.064** (0.027)	0.044 (0.029)	0.042 (0.027)	0.047* (0.028)	0.041 (0.029)
Controls	No	Yes	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual effects	No	No	RE	RE	FE	FE
# of observations	49,279	49,279	49,279	49,279	49,279	49,279
# of individuals	18,187	18,187	18,187	18,187	18,187	18,187
R^2	0.004	0.266	0.004	0.262	0.743	0.753

Same legend as Table 3.

Parametric specification

Table 11: Ordered Probit model (Treatment: Absorbed regions)

	(1)	(2)	(3)	(4)
Treatment $\times$ Post	0.050** (0.020)	0.060*** (0.020)	0.067** (0.026)	0.066*** (0.025)
Controls	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Individual effects	No	No	RE	RE
# of observations	49,279	49,279	49,279	49,279
# of individuals	18,187	18,187	18,187	18,187
$\log(L)/N$	-1.862	-1.725	-1.693	-1.625

Same legend as Table 3.

Note. Ordered Probit instead of linear model.

Robust standard errors clustered at the individual level.

Table 12: Ordered Logit model (Treatment: Absorbed regions)

	(1)	(2)	(3)	(4)
Treatment $\times$ Post	0.080** (0.035)	0.087** (0.035)	0.116** (0.046)	0.110** (0.044)
Controls	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Individual effects	No	No	RE	RE
# of observations	49,279	49,279	49,279	49,279
# of individuals	18,187	18,187	18,187	18,187
$\log(L)/N$	-1.862	-1.721	-1.681	-1.614

Same legend as Table 3.

Note. Ordered Logit instead of linear model.

Robust standard errors clustered at the individual level.

Statistical issues, survey sampling and clustering

Table 13: Verbeek and Nijman tests for attrition (Treatment: Absorbed regions)

	(1)	(2)	(3)	(4)	(5)	(6)
Belongs to balanced panel	-0.042 (0.028)	-0.006 (0.021)	-0.053 (0.052)	-0.025 (0.038)	. (.)	. (.)
# of years in sample	-0.008 (0.007)	0.007 (0.006)	-0.006 (0.011)	0.014 (0.009)	. (.)	. (.)
Controls	No	Yes	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual effects	No	No	RE	RE	FE	FE
# of observations	49,279	49,279	49,279	49,279	49,279	49,279
# of individuals	18,187	18,187	18,187	18,187	18,187	18,187
R^2	0.004	0.266	0.004	0.262	0.743	0.753

Same legend as Table 3.

Table 14: Estimations on balanced panel (Treatment: Absorbed regions)

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment $\times$ Post	0.107** (0.041)	0.118*** (0.043)	0.098** (0.042)	0.110*** (0.042)	0.094** (0.042)	0.101** (0.042)
Controls	No	Yes	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual effects	No	No	RE	RE	FE	FE
# of observations	8,502	8,502	8,502	8,502	8,502	8,502
# of individuals	1,417	1,417	1,417	1,417	1,417	1,417
R^2	0.015	0.297	0.014	0.288	0.642	0.661

Same legend as Table 3 (balanced panel instead of unbalanced panel).

Table 15: Impact of the clustering level on the precision of the estimated ATE
(Treatment: Absorbed regions)

	(1)	(2)	(3)	(4)	(5)	(6)
Level of clustering	individual	individual $\times$ year	former region	former region $\times$ year (baseline)	new region	new region $\times$ year
Treatment $\times$ Post	0.076*** (0.029)	0.076*** (0.025)	0.076* (0.041)	0.076** (0.031)	0.076* (0.039)	0.076*** (0.028)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
# of observations	49,279	49,279	49,279	49,279	49,279	49,279
# of individuals	18,187	18,187	18,187	18,187	18,187	18,187
# of clusters	18,187	49,279	12	72	10	60

Same legend as Table 3.

Online Appendix

A Supplementary figures

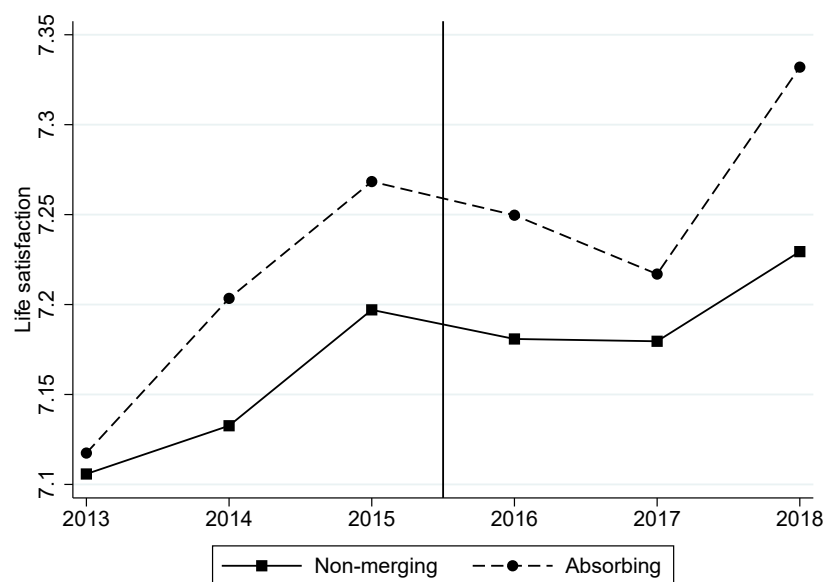


Figure 9: Evolution of life satisfaction across regions

B Supplementary robustness checks

Table 16: Effect of the merger on life satisfaction (Treatment: Absorbing regions)

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment $\times$ Post	-0.001 (0.028)	0.001 (0.028)	-0.024 (0.025)	-0.023 (0.025)	-0.036 (0.036)	-0.037 (0.036)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	Yes	No	Yes	No	Yes
Individual effects	No	No	RE	RE	FE	FE
# of observations	49,937	49,937	49,937	49,937	49,937	49,937
# of individuals	18,466	18,466	18,466	18,466	18,466	18,466
R^2	0.261	0.262	0.699	0.699	0.755	0.756

Same legend as Table 3.

Table 17: Effect of regional integration on life satisfaction (Treatment: Absorbing regions, less conservative definition)

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment $\times$ Post	0.013 (0.025)	0.015 (0.025)	0.004 (0.022)	0.005 (0.022)	0.006 (0.031)	0.006 (0.031)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	Yes	No	Yes	No	Yes
Individual effects	No	No	RE	RE	FE	FE
# of observations	59,708	59,708	59,708	59,708	59,708	59,708
# of individuals	21,958	21,958	21,958	21,958	21,958	21,958
R^2	0.256	0.257	0.693	0.693	0.750	0.750

Same legend as Table 3.

B.1 Endogenous residential sorting

Table 18: Effect of the merger on life satisfaction, controlling for endogenous spatial sorting (Treatment: Absorbed regions)

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment $\times$ Post	0.082*** (0.027)	0.084*** (0.025)	0.078*** (0.026)	0.072*** (0.026)	0.085*** (0.031)	0.076** (0.031)
Controls	No	Yes	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual $\times$ region effects	No	No	RE	RE	FE	FE
# of observations	49,279	49,279	49,279	49,279	49,279	49,279
# of individuals	18,353	18,353	18,353	18,353	18,353	18,353
R^2	0.004	0.266	0.004	0.262	0.743	0.755

Same legend as Table 3.

B.2 Differential impact of controls post-merger

Table 19: Effect of the merger on life satisfaction, allowing for differential impact of controls post-merger (Treatment: Absorbed regions)

	(1)	(2)	(3)
Treatment $\times$ Post	0.082*** (0.025)	0.063** (0.026)	0.064** (0.030)
Controls	Yes	Yes	Yes
Controls $\times$ Post	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Region FE	Yes	Yes	Yes
Individual effects	No	RE	FE
# of observations	49,279	49,279	49,279
# of individuals	18,187	18,187	18,187
R^2	0.267	0.263	0.754

Same legend as Table 3.

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