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**How do citizens perceive centralization reforms?
Evidence from the merger of French regions**

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How do citizens perceive centralization reforms? Evidence from the merger of French regions

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Abstract

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

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

JEL Classification: H75; I31.

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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). There are voices in Catalunya, Flanders, Scotland and Wallonia asking for further autonomy, and even independence from Belgium, Spain or the UK. In metropolitan France, most emblematic examples of attachment to local culture, and sometimes to specific common law, are Alsace-Lorraine, the Basque Country, Brittany, and Corsica. 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.

Usual arguments in favor of large jurisdictions are related to efficiency gains and economies of scale, i.e., to 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 undermine feelings of regional identity. It is an empirical issue to assess the relative importance of each of the three mechanisms 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, leading possibly to more centralized policies less tailored to local needs and characteristics of the population, but also (iii) the loss of regional identity, if any, which might be more painful in regions that were absorbed

from both political and economical viewpoints.

This paper seeks to test empirically whether citizens were hurt by the merger or not. This unique natural experiment allows the researcher to recover the causal impact of reshaping subnational borders on subjective well-being as self-assessed by individuals. This study also aims at disentangling among the previous three channels at stake. Interestingly, this merger of regions was not announced during the 2012 electoral campaign; it was completely unexpected and came out as a surprise to political commentators and citizens at the time. The political debate that preceded the voting of the law implementing that merger 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 results from 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-differences approach that takes advantage of that 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, *the regions themselves* have changed over the period considered, which is rather unusual. 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 absorbed since they lost executive power: in that sense, these regions have experienced increased centralization due to political decisions being

taken further away from citizens.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. More strikingly, when focusing on the differential evolution of absorbed regions with respect to non-merging regions as a comparison group, a significant and positive impact, about .082 on the 0-10 scale, is obtained, which represents nearly 5% of one standard deviation. Put differently, everything happens roughly as if 8.2% of individuals had reported a +1 change in their life satisfaction. To get an alternate sense of the magnitude of this effect, it corresponds to one half of the overall change in subjective well-being observed between 2013 and 2018, which is thus far from negligible. 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.). As a falsification test, the simulation of placebo experiments, namely fake reforms occurring before the observed one, yields non significant estimates even in the absorbed regions, which supports the absence of pre-trend. 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 empirical evidence at stake sug-

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 below.

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

gests that the institutional setting matters: in particular, it is useful to remember that France, contrary to Switzerland, is not a federal state.

To understand the above results, I investigate the role played by several mechanisms. First, the economic attractiveness of merging regions has improved, in line with public incentives to foster local business development and to enhance economic performance. In particular, the unemployment rate experienced a faster decline in absorbed regions after 2016, which is consistent with poorer, absorbed regions benefiting from promotion activities of richer, absorbing regions. This finding is especially relevant when debating the possibility of grouping local authorities. Second, 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 regions are exclusively in charge of (upper secondary education, vocational training, and trains). This increase in local public spending consecutive to the merger might have led to further citizen discontent if the fiscal effect (citizens are forward-looking tax payers and should therefore anticipate higher local tax rates) dominates the residential effect (citizens are also residents of these regions, and they benefit from the provision of local public goods). 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) revalued 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 potential citizen discontent due to increased centralization. They are consistent with the relative importance of economic gains with respect to cultural norms. The positive effect obtained on absorbed regions indicates at least that the loss of regional identity, if any, has been limited and more than

compensated by economic mechanisms. Note also that absorbing, richer regions were not significantly hurt: this Pareto improvement is somehow reminiscent of the impact of immigration on the labor market (Card, 1990): local workers are not hurt by newcomers, and of peer effects in education (Guyon et al., 2012): high-achieving pupils are not hurt by low-achieving roommates benefiting from interactions with the latter.

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 possible mechanisms 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 both theoretical and empirical effects of integration of local jurisdictions, and another one concerned by spatial determinants of happiness.

First, in the theoretical framework built by Alesina and Spolaore (1997), the optimal number and size of countries result from a trade-off between economies of scale and heterogeneity of preferences. Empirical investigations on mergers of local jurisdictions (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 (2021) 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 an EU perspective. A number of papers are then 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, the causal relationship being estimated on longitudinal survey data at the individual level. It is also 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 have been 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 current French case may appear as an outlier in this respect since local public spending has actually increased consecutive to the merger, but [Frère et al. \(2014\)](#) had already pointed out that intermunicipal cooperation did not reduce public spending in that country. Finally, from a fiscal federalism viewpoint, [Oates et al. \(1972\)](#) decentralization theorem balances heterogeneity in citizens' preferences and externalities between jurisdictions; it 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, [Breuille 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 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 therefore the 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, subjective perceptions do a fair job when measuring quality of life. However, the concept of region used here 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

³Before the NOTRe law (see below), 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 allocated to municipalities or intermunicipal communities. After the NOTRe 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.

variation of centralization reforms in Switzerland to identify their effect on subjective well-being. In this federal state, she finds a negative relationship between life satisfaction and the degree of centralization, which she attributes to the corresponding loss of influence over political decisions: her results emphasize therefore the role played by the channel mentioned above, related to the heterogeneity of preferences, in this setting.

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 (mainland and Corsica) and overseas.⁵ Metropolitan France is divided into several layers: regions, *départements* and municipalities -the latter 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.

These local governments share various responsibilities in terms of education, public welfare, public transportation, economic development, youth, sports, etc. Turning to regions, the exclusive areas of jurisdiction of the latter include upper secondary education, vocational training and apprenticeship, as well as trains. 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

⁵Overseas are composed of five regions: French Guiana, Guadeloupe, Martinique, Mayotte, and Réunion, which were not required to merge by the reform at stake.

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, vocational training, apprenticeship, and trains. The third act of decentralization is precisely a set of laws including the merger as well as the *Loi portant Nouvelle Organisation Territoriale de la République* or NOTRe law, which 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 tends to increase the degree of decentralization. On the other hand, regions were merged into larger entities, which can be interpreted as a trend toward centralization -especially in absorbed regions, see below.

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 instead of the 22 existing ones. This substantial reduction in the number of regional jurisdictions led to reshape French subnational borders. In fact, 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, the *comité* advocated for the creation of 15 "super regions" in metropolitan France so as to meet European standards in terms of size (German, Italian and Spanish regions being larger, on average). 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 it 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, and two weeks later, the bill was under consideration by the Senate. However, the law was only adopted by the

National Assembly, namely the lower house of the French Parliament, on December 17, 2014, because of strong opposition to the project. The Constitutional Council nonetheless indicated that the law was conform to the constitution on January 15, 2015. Finally, the #2015-29 law, or *Loi relative à la délimitation des régions, aux élections régionales et départementales et modifiant le calendrier électoral*, which rules the current administrative division of France, was promulgated on January 16, 2015 and implemented from January 1st, 2016 onwards.

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 “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 were 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 9 in Appendix yields the correspondence between the 22 old regions (Figure 1a) and the 13 new regions (Figure 1b).

Importantly, the NOTRe law enforced the role played by regions in economic development by putting an end to the one played by *départements* in that domain. For instance, the priority has been given to promoting regional attractiveness through simplified administrative procedures for firms. It is therefore expected that this merger encouraged the promotion of local business development, and thus

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

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

has enhanced local economic performance. Following the NOTRe law, the regional share in local fiscal tax revenue increased, remember footnote 3; nevertheless, the lump-sum transfer from the State decreased accordingly so as to keep regions' revenue 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 (regions whose GDP per capita lies between 75% and 90% of the EU average) exists, especially in case of a 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 lead precisely 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. French regions, including overseas, received €12.5bn from the EU over the 2007-2013 period. Though the former trade-off results in an ambiguous overall effect, some economic improvement is expected, at least in the short run.

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

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).

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 geographic location. 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.⁹ As another interesting feature of SRCV, this survey enables the researcher to track individuals even when they move between two waves.

The comparability of the data related to subjective well-being before and after 2013 casts doubts, according to the very unit in charge of the SRCV survey at Insee. Though the survey started in 2010, 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 at that date.¹⁰ In what follows, I focus therefore on the 2013-2018 period.

Table 10 in Appendix contains some descriptive statistics related to the working sample, an unbalanced panel of 32,872 individuals (90,734 individual-year observations) followed from 2013 to 2018 and whose annual income exceeds €1. Women and elders are slightly over-represented (58% 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 sizable dispersion since its coefficient of variation is roughly 1.24, and the top 1% earns more than €86,000 a year.

A measure of subjective well-being is based on a question related to overall satisfaction with life, the answer being provided on a discrete 0-10 scale. The average life satisfaction amounts to 7.18; the distribution is rather concentrated

⁹According 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.

¹⁰See Figures 13 and 14 in Appendix.

around levels 7 and 8 but has whole support, see Figure 10 in Appendix. The cross-sectional coefficient of variation is as small as .24. From 2013 to 2018, a trend toward higher life satisfaction can be observed: subjective well-being increased on average by .16.

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, that is, to individuals who move from one region to another; or she may exploit natural experiments that involve some region change, focusing by contrast on stayers, i.e., on individuals who stay in the same region. The latter being *a priori* more numerous than movers, the former strategy looks fragile since it relies on a small sub-sample of individuals. On top of that, there are various, possibly endogenous reasons which make individuals move: endogenous spatial sorting might arise in the sense that such moves can 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 unhappy because they are unemployed and who search for better job opportunities, including outside their region of residence. Though more promising, the latter source of identification is extremely rare in practice but the current framework makes it plausible.

The identification strategy adopted in this paper consists in exploiting the merger described above as a natural experiment. Since the reform was unexpected, strategic individual behavior is hardly at stake.¹¹ The reform provides therefore with 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

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, or selective migration: see row (9) of Table 6.

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. Individuals in this control group represent 37% 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 By definition, merging regions and non-merging regions form a partition of metropolitan France. The first identification strategy relies on a comparison of the evolution of life satisfaction in merging regions with that of non-merging regions. 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 below). Merging regions differ from non-merging regions in that they were significantly poorer at onset: in 2015, individuals living in the latter report an annual income of €27,200 on average as opposed to €24,300 for residents of the former -the difference being statistically significant at 5%, allowing for unequal variances within the two groups.

Among the 16 merging regions forming 7 new regions, some are smaller from demographic, economic, geographic or political perspectives; these regions can be considered as absorbed. I define absorption in a conservative fashion: it means that administrative issues and local politics are held in a prefecture that is located in another region after the merger -the absorbing one. Put differently, the capital of (the) absorbed region(s)¹³ moved while the capital of the absorbing region did not: it is then fair to assume that absorbed regions have experienced increased centralization since their inhabitants have mechanically less control over local policy decisions. For this very same reason, it is also expected that the loss of regional identity, if any, would be more salient in these regions due to the transfer in local

¹²Absorbing regions may well be considered, too. The corresponding estimates are available on Tables 11 and 12 in the online Appendix. Overall, no significant impact is found in those regions. See also Figure 12.

¹³An absorbing region has possibly absorbed one or two regions.

executive power. Absorbed regions are Auvergne, Champagne-Ardenne, Limousin, Lorraine, Picardy and Poitou-Charentes; 18% of individuals lived there before the merger. Absorbing and absorbed regions look quite similar in terms of observable characteristics (Table 1), except that absorbed regions are significantly poorer: €23,300 on average against €25,000 in 2015, the difference being statistically significant at 5%, allowing for unequal variances within the two groups. Remember that the reform intended to reduce intraregional differences. The 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. In this exercise, the 26% of individuals living in an absorbing region (Alsace, Aquitaine, Nord-Pas-de-Calais, Rhône-Alpes) are excluded from the analysis; by definition, in such regions, not only has the former regional prefecture (or capital) 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 among the former regions. Burgundy and Franche-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 capitals of the new regional entities to which these (former) regions now belong to remained unchanged.¹⁴ In a conservative approach, I exclude such regions from the analysis. I provide a robustness check with respect to that methodological choice by including them as well in the treatment group, see section 8.2 on that issue.

As usual in difference-in-differences approaches, the identifying assumption is that individuals would have experienced a similar evolution of subjective well-being in both comparison and treatment groups, had the merger not happened. This common trend assumption cannot be tested, but its plausibility can be assessed.

¹⁴In a recent book, [Négrier and Simoulin \(2021\)](#) confirm that balancing executive powers between former Languedoc-Roussillon and Midi-Pyrénées within the new Occitanie has not been an easy task.

To that purpose, Table 1 summarizes observed differences between treatment and comparison groups, before and after the merger -the treatment considered here being absorption. Either both groups are comparable in terms of observable characteristics, or there are hardly any changes over time in the relative characteristics of the two groups, which supports empirically the identifying assumption.¹⁵ A remaining concern might be the lack of overlap in the covariate distribution; [Imbens and Wooldridge \(2009\)](#) recommend using a scale-invariant normalized difference, which is reported in the last two columns. For each covariate, the normalized difference is the difference in averages by treatment status, scaled by the square root of the sum of variances. Linear regressions tend to be sensitive to the functional form assumption if the normalized difference exceeds one quarter. The normalized differences for control covariates indicate that the lack of overlap should be of little concern here.

Focusing now on the dependent variable, Figures 2 and 3 depict the evolution of life satisfaction in both groups for the two different treatments considered (merger and absorption). As far as pre-trends are concerned, it is not possible to disregard any of the empirical strategies presented above. The event study analysis presented in Appendix (Figure 8) devoted to the second identification strategy, related to absorbed regions, will also confirm that the joint null hypothesis cannot be rejected before 2015; section 8.4 also simulates placebo experiments to demonstrate the absence of pre-trend. Last, despite the break in well-being's time series occurring in 2013, the common trend assumption is also plausible on the whole 2010-2015 period,¹⁶ which comforts the identifying assumption.

Interestingly, while life satisfaction has remained rather stable in both merging and non-merging regions after 2016 (or has even slightly increased in the latter with respect to the former), some sharp rise in self-assessed subjective well-being can be observed in absorbed regions, from about 7.12 in 2015 to 7.26 in 2017.

¹⁵It is also possible to check that difference-in-differences coefficients corresponding to equations where covariates are considered as the dependent variables lead to insignificant estimates.

¹⁶See Figure 13 in Appendix.

This empirical evidence suggests therefore that the impact of the merger on life satisfaction was positive for individuals living in absorbed regions, but small or/and not significant for those living in merging regions. An immediate computation from Table 1 suggests that the absorption had a causal impact of $-0.015 - (-0.075) = 0.06$ on life satisfaction, not controlling for any observed characteristics at this stage. The next section provides with an econometric model that enables to check whether this 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 could be related to endogenous border drawing -namely the possibility that new borders were designed to minimize discontent. Put differently, one may worry about selection into treatment. This would not threaten internal validity as long as the previous difference-in-differences identifying assumptions are valid. It must be acknowledged, though, that external validity would be more challenging, and that the results obtained in this particular setting would not generalize easily to other contexts.

6 Econometric specification

The difference-in-differences approach is now implemented based on a linear model. Since the dependent variable, self-assessed life satisfaction, is ordinal, it is tempting to resort to ordered models, which is done as a robustness check in section 8.5. However, on top of clarity, linearity permits an easy inclusion of individual fixed-effects, which enables the researcher to control better for unobserved heterogeneity. Even though it is possible to include fixed-effects in ordinal models, this is a more computationally demanding task.¹⁷ There are serious reasons to believe that unobserved heterogeneity is a first-order issue as far as subjective well-being is concerned, which claims rather for individual fixed-effects.

¹⁷See the Appendix on this topic.

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} (income, age, education, gender, occupation, labor force status, family status) on top of individual fixed-effects α_i , year dummies δ_t and regional dummies μ_r .¹⁸ The selection of controls in the estimating equation can be dealt with in several ways. 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 pick up the most relevant variables. In practice, both the literature and statistical criteria suggest that relevant covariates here correspond to labor force status, family status, income and age; education, gender and occupation dummies are included for the sake of completeness.¹⁹

The average treatment effect (ATE), β , is recovered by the coefficient corresponding to the interaction of the treatment dummy defined at the regional level with a dummy equal to 1 from 2016 onwards, i.e., during the post-reform period. Idiosyncratic shocks ε_{irt} follow a normal distribution, and I use robust standard errors with two-way clustering by individual and region (as in, e.g., [Allcott et al., 2019](#)) to take autocorrelation of residuals into account, among others; section 8.6 provides robustness checks with respect to that level of clustering.

The identification of the model is achieved by assuming strict exogeneity of the covariates conditional to the individual effects. The exogeneity follows from the reform being a natural experiment. A concern has been raised by a recent literature devoted to treatment effects, and could apply here if treatment effects were

¹⁸Municipality FE could be included instead without altering the results.

¹⁹The results remain unchanged when allowing these control variables to have distinct trends before and after the merger, see row (10) of Table 6 in the Appendix.

heterogeneous either across regions or over time.²⁰ In that case, the β coefficient of the linear regression above which includes two-way fixed-effects would estimate a weighted sum of the local average treatment effects (LATE) specific to each region and year. A problem would arise if some of these weights were negative, which occurs in many settings as shown by [De Chaisemartin and d’Haultfoeuille \(2020\)](#). However, the current analysis circumvents this problem thanks to the sharp design, namely to the fact that all concerned regions were treated after 2016.

7 Results

Tables 2 and 3 show the results obtained from the specification (1) for the two treatments, merger and absorption. For the sake of readability, only the average treatment effects (ATEs) are reported in these tables. The other estimates $\hat{\gamma}$ are available upon request; it is worth emphasizing that they are completely in line with the huge empirical literature devoted to subjective well-being. For instance, unemployment has a strong, depressing impact on life satisfaction: it tends to lower it by .4, on average, on the Cantril scale. By contrast, and even though money can’t buy happiness, it definitely contributes to it since the correlation between income and life satisfaction is strong and positive: increasing one’s standard of living by 1% raises subjective well-being by roughly .08. Having a partner increases life satisfaction by about .3; education, gender and occupation have hardly any effect. Thanks to the large sample size, $\hat{\gamma}$ is very precisely estimated, and there is little doubt that these well-known stylized facts hold in the data.

Turning now to the causal effect of the merger, one cannot reject the null hypothesis that the ATE is equal to zero at the 5% level. This empirical evidence suggests at least that the merger of regions has not hurt individuals, which brings me to state and test:

Prediction 1 (Heterogeneity of preferences / Taste for decentralization) Due to

²⁰The assumption that β does not vary over time is relaxed in an event study approach: see, for instance, section 8.1.

the creation of larger local government entities, subjective well-being should have decreased. *Rejected on the data.*

More interestingly, the main insight from Table 3 is that the absorption has significantly increased individual life satisfaction. 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 .082 in the preferred specification from column (6) that contains both individual fixed-effects and controls. In other words, everything happens roughly as if 8.2% of individuals reported a +1 change in their life satisfaction, or as if the standard of living of all individuals increased by 1%. This effect amounts to nearly 5% of one standard deviation. Remember also that subjective well-being has increased on average by .16 from 2013 to 2018 at the national level, hence absorption *per se* would account for one half of this change, which is rather substantial. This finding suggests that absorption had positive effects which outweighed individuals' attachment to their former administrative region, despite the presumed fear of losing one's regional identity put forward in the public debate. Hence one can reject formally:

Prediction 2 (Loss of regional identity) Consecutive to the merger, subjective well-being should have diminished, especially in absorbed regions where the loss of regional identity is *a priori* more pronounced. *Strongly rejected on the data.*

8 Robustness checks

This section provides several robustness tests in order to check that previous results can truly have a causal interpretation, and do not stem from statistical artifacts. All the results are presented in Appendix; they concern the second identification strategy, i.e., absorption as the treatment, since the absence of any significant

effect of the merger is also robust to the various specifications considered here. First, I resort to an event study analysis. Second, I consider alternative treatment and control groups, which helps me to determine the sensitivity to different common trend assumptions. Third, falsification tests are performed so as to guarantee the absence of pre-trend: I simulate placebo experiments, namely fake mergers that would have occurred before the actual one in 2016. Fourth, the sensitivity of the results is assessed with respect to the parametric specification. Fifth, from a statistical viewpoint, the results seem to be driven neither by attrition, nor by survey sampling issues; they are also rather robust to the clustering level considered. Finally, I investigate the possibility of intermunicipal cooperation acting as a confounding factor.

8.1 Event study analysis

I consider an event study framework in which regions become treated from $t = 2016$ onwards, or not -in the latter case, they belong to the comparison group. The normalization is that the null arises in 2015, just one year before the implementation of the merger. To this purpose, I interact year dummies with the treatment group indicator. By construction, this approach relaxes the assumption of a stable treatment effect over time. The results displayed by Figure 8 in Appendix suggest that the treatment effect stems mostly from the first two years following the absorption, namely 2016 and 2017, consistently with Figure 3. In 2017, it cannot be excluded that the point estimate is as high as .23, the upper bound of the confidence interval. Yet I cannot reject the null in 2018, as if the effect was short-lived.

8.2 Alternative treatment group

A less conservative definition of absorption is considered here, which includes former regions Franche-Comté, Lower Normandy and Languedoc-Roussillon that share some governance with their respective absorbing region within new regional

entities. Under this approach, I consider therefore 9 absorbed regions instead of 6, and absorbed regions defined under this broader sense account for 26% of individuals. On top of providing with a sensitivity analysis, this empirical strategy relies thus on a larger sample size, and yields more precise estimates. Row (1) of Table 6 shows that absorption has still a positive effect of the same magnitude.

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 conservative sense.

8.3 Alternative control groups

Another robustness check consists in verifying that the ATE does not depend too much on the definition of the comparison group. A potential concern is related to the effect being driven by the leading Île-de-France region which remained unaffected by the merger, and which looks like an outlier with respect to other regions: it is both the wealthiest and the most crowded region since Paris belongs to that region. To address this concern, I exclude Île-de-France from the comparison group. The results are displayed by row (2) of Table 6: reassuringly, the estimated ATE remains very close to the one found in Table 3.

Another concern could be that the comparison group is not perfectly comparable with the treatment group in the sense that both outcome and its predictors are not the same before the reform. First, a descriptive analysis shows that they are, in fact, pretty similar (except as regards income, as already mentioned), and the corresponding figures are available upon request. Second, a standard solution to fix this potential problem consists in resorting to a synthetic control approach (Abadie et al., 2010), i.e., in building a synthetic control group that mimics the treatment group in terms of both pre-treatment outcomes and observed characteristics. According to Figure 9 in Appendix, the short-run effect documented above would amount to .06 in 2016 and to .135 in 2017, before it vanishes in 2018. As in the event study analysis, this approach relaxes the stable treatment effect assumption.

8.4 Placebo experiments

I then simulate two placebo experiments, namely fake mergers in 2014 and 2015, that is, before the actual reform in 2016. If the common trend assumption holds, it should pass these falsification tests. The results are provided by rows (3) and (4) of Table 6. No significant effect is found, which is consistent with the absence of any differential pre-trend in subjective well-being across comparison and treatment groups. Hence it gives further credit to the finding that the improvement in self-assessed life satisfaction reflects a causal effect of the absorption. Note also that, if anything, the estimated ATE under a fake 2015 merger is slightly higher than the one corresponding to a fake 2014 merger, none of these effects being significant at usual levels, though; this could be consistent with some anticipation effect in 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 or Provence-Alpes-Côte d’Azur to a smaller extent, would be precisely the ones in which regional attachment was higher. Importantly, this would not be a problem for internal validity as long as the common trend assumption is valid.

8.5 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, an ordered Probit and an ordered Logit, are therefore estimated: see rows (5) and (6) of Table 6.²¹ 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, available upon request, are consistent with the ATE obtained under the linear specification: the probabilities of reporting a level of life satisfaction higher than 8 (resp. comprised

²¹The Appendix contains a discussion with respect to the inclusion of individual fixed-effects in such models.

between 5 and 7) increase (resp. decrease) by roughly .1pp.

8.6 Statistical issues, clustering and survey sampling

I investigate next 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 including a dummy indicating whether an individual belongs to the balanced panel as a supplementary explanatory covariate in the model, and in testing for its significance. Table 7 in Appendix shows that endogenous attrition is not too much of a concern here since this dummy is not significant at usual levels: the corresponding null hypothesis cannot be rejected.

Row (7) of Table 6 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. This approach removes any bias due to changes over time in the differences in the unobservable characteristics of the treatment and comparison group. The treatment effect is identified from changes in life satisfaction over time in the treatment group with respect to the comparison group. The estimated ATE turns out to be very close to the one estimated previously, or even higher (.094 in the preferred specification), but the lack of statistical power leads it to lose significance.

Last, I wonder whether the above empirical findings are robust to other levels of clustering than the two-way clustering approach used up to now. According to [Bertrand et al. \(2004\)](#), the ideal clustering level is the level of intervention, namely former regions. However, this choice is challenged in the current empirical setting since it would yield only a small number of clusters (22). Hence it would cast doubts as regards the consistent estimation of the asymptotic variance-covariance matrix: a rule of thumb suggests that at least 50 clusters are required (see, e.g., [Cameron and Miller, 2015](#)). Rather, I investigate hereafter whether significance remains when standard errors are clustered at individual, former region or new

region levels, i.e., taking autocorrelation of residuals into account. Table 8 in Appendix shows that the estimated standard errors vary from .03 to .038 depending on the considered level of clustering. I conclude that the significance of the estimated ATE at usual levels is verified empirically provided that the necessary condition of disposing of at least 50 clusters is fulfilled.

8.7 Intermunicipal cooperation as a confounding factor?

Finally, a remaining concern could be related to the gradual move toward intercommunality (Tricaud, 2021), for instance through an accrued importance of metropolis consecutive to the *Loi de Modernisation de l'Action Publique Territoriale et d’Affirmation des Métropoles* (the so-called MAPTAM law) implemented from 2014 onwards. Municipalities have indeed engaged into a cooperative process which led to a substantial reduction in their number during the considered period: -3.3% on average, -3.1% in merged regions and -2.9% in absorbed regions. To check that this change does not act as a confounding factor in the above estimations, I control further for the annual number of municipalities within regions. As can be seen on row (8) of Table 6, the results are completely robust to controlling for that factor, though.

9 Interpretation

This section is devoted to an 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 (i) why there is no negative impact, and (ii) why there is even an improvement in absorbed regions, as far as individual life satisfaction is concerned. A technical difficulty arises due to data limitations: 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 important exception is the unemployment rate since Insee provides

with series at the *département* level, and these figures can easily be aggregated at the former region level. In other cases, the absence of relevant data at the desired aggregation level prevents me from characterizing distinct changes in absorbed and absorbing regions; I am then forced to focus on the comparison between merging and non-merging regions only.

Before turning to mechanisms *per se*, I wonder whether the effects documented above are heterogeneous (i) across individuals, and (ii) across regions. In a triple differences approach, I specify now $\beta \equiv \beta_0 + \beta_X X_{irt}$ in equation (1). The top panel of Table 4 tests whether some individual characteristics tend to amplify, or to attenuate previous effects.²² Life satisfaction increased most for blue collars consecutive to an absorption: the estimated treatment effect for them amounts to slightly less than three times the ATE. For singles and for individuals holding a vocational degree, the point estimates are still about twice the ATE. The fact that low-qualified workers with weaker family ties, who are likely more mobile on the job market, benefited the most from this reform is rather consistent with some economic explanation (see below). To investigate further the possibility that the impact of the merger varies according to regional characteristics, I allow the treatment effect to depend on the corresponding GDP per capita, population size and area (all measured at the former region level), and to differ in rural and in urban areas. The only significant difference is found for the GDP per capita dimension,²³ and comes up with a negative sign which indicates that the effects of the merger were more pronounced in poorer regions (bottom panel of Table 4). This empirical finding is consistent with the intended, equalizing objective announced by the government at the time. It suggests further that the economic dimension helps explain current results.

A first clue to understand the economic impact of that merger is provided by

²²Rejections of $H_0 : \beta_X = 0$ only are reported in those Tables.

²³measured either in level or in log, the latter estimates being available upon request. Allowing the ATE to vary with the (highest) difference between former regional GDP per capita within new regional entities yields to insignificant point estimates, though.

changes in local unemployment rates. Column (1) of Table 5 shows that local unemployment rates have decreased by .18pp (-1.4%) more in merging regions than in non-merging regions after 2016. The decline in the unemployment rate is observed at various levels: new region, former region, *département*, or even at a more local level, within a *zone d'emploi*, literally a labour market area (there are about 3-4 *zones d'emploi* per *département*). Estimation results at the latter level turn out to be remarkably stable (-.18pp); they are available upon request. This empirical evidence suggests that the merger has created new economic opportunities which led to better prospects on local job markets, and which benefited mostly to young and prime-age women: the decline in unemployment rate reached up to 3.8% for these subpopulations. It is especially the case in absorbed regions: consecutive to the merger, the unemployment rate has decreased in those regions by .21pp more (-1.9%) than in non-merging regions (Figure 4 and column (2) of Table 5); this figure amounts to -.24pp (-2.1%) at the *zone d'emploi* level. Interestingly, the impact is null in absorbing regions, see column (3). This decline in the unemployment rate observed in France after a merger of local jurisdictions is consistent with [Wolfschuetz \(2020\)](#) who also finds some improvement in local economic performance as measured by the unemployment rate after municipalities enter intermunicipal cooperation in Germany, which she relates to a fostering of local business development. Indeed, from the above analysis devoted to the heterogeneity of treatment effects, the merger has favored poorer, absorbed regions, as if they had benefited from economic spillovers of the richer, absorbing regions. This improvement of local economic climate has likely concerned people characterized above, composed of blue collars, singles, and individuals with a vocational degree who may move more easily from one region to another, and might be more prone to benefit from new job opportunities. As a plausible channel for this improvement of local labor market conditions, the new regional boundaries might have facilitated commuting within these larger entities; remember that regions' scope includes trains, and that commuting could have been made easier thanks to synchronized rail trans-

portation. Also, new regions were bound to promote local economic performance, especially by simplifying administrative procedures for firms, which might have fostered local business development. Finally, suggestive evidence is provided in this respect by Figure 5 which shows that regional subsidies to firms have tended to increase consecutive to the merger in the concerned regions:²⁴ direct support to the private sector may be better tailored at the local level, rather than at the national level. Determining whether and how exactly such place-based policies have been effective at decreasing local labour markets is the object of future research; see, e.g. [Etzel et al. \(2020\)](#), in the case of the German reunification.

Another explanation could precisely be that there was 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 did not involve regions that were as heterogeneous in terms of GDP per capita as were West and East Germany. On top of that, Figure 6 suggests that the common trend assumption does not hold for individual income, and that the corresponding effect, if any, would be small.

On the whole, these investigations lead to:

Prediction 3 (Economic spillovers) Following the merger, subjective well-being should have increased through an improvement of the overall economic climate. *Cannot be rejected on the data.*

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 situation, an update of IT systems, etc.

²⁴Unfortunately, data is missing for 2013 as well as in Aquitaine before 2016 and in Brittany before 2015, hence these regions are removed from the working sample of Figure 5.

Moreover, merging regions raised their investment spending while non-merging regions hardly increased them at all over the post-merger period. From data issued by the DGCL, the French directorate in charge of statistics on local authorities, Figure 11 in Appendix 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 the current sample. Not only did the merger generate no economies of scale, but it resulted in an *increase* of local public spending, which is confirmed by Figure 7 and column (4) of Table 5. In a theoretical framework *à la* [Alesina and Spolaore \(1997\)](#), two different forces push thus in the same direction: (i) reinforced centralization, which should increase citizen discontent (Prediction 1), and (ii) a fiscal (or wealth) effect due to the increase in local public spending, hence in future local tax rates, which should be disliked by tax payers (Prediction 4 below). By contrast, the economic spillovers documented previously (Prediction 3) correspond to a "residential effect" related to the fact that citizens are not only tax payers, but also residents who value an extended provision of local public goods consecutive to higher local public spending. However, and despite the fact that any increase in local public spending should come along with (at least future) higher tax pressure, local tax revenues have hardly changed consecutive to the merger: see column (5) of Table 5.

To sum up, this brings me to:

Prediction 4 ((Un-)Efficiency channel) Consecutive to the merger, local public spending has increased, which should have depressed subjective well-being of tax payers anticipating a rise in local taxes. *Rejected on the data.*

10 Conclusion

This paper has assessed the causal impact of the 2016 merger of French regions on individual subjective well-being. This wide-scale reform, a perfect natural experiment, reduced the number of regions from 22 to 13, among others, and that

reduction in the number of regions was synonymous of both stronger centralization in absorbed regions and higher integration costs. 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 smaller, poorer regions, which can be explained by an improvement of local economic conditions; in particular, the unemployment rate declined more rapidly in absorbed regions, which might have benefited mobile and low-qualified workers.

The current empirical evidence suggests therefore that economic spillovers help explain citizen's attitude towards changes in subnational borders, on top of the usual trade-off between economies of scale and heterogeneity of preferences. By contrast, the analysis has proved fruitful in answering concerns related to the presumed "loss of regional identity": the data does not favor such a hypothesis. Regional belonging might be oversold in the public debate, and at the very least, economic outcomes are likely to outweigh cultural norms.

Given that the results obtained here look somehow different from those obtained in other settings, this research also emphasizes the role played by the institutional context, acknowledging yet that the latter is in fact endogenous to preferences. For instance, the taste for decentralization might be lower in a centralized country, like France, than in federal states, and act as the ultimate ground for the current form of institutions. It provides also some guidance to policy makers when deciding to merge local authorities. Further research is however needed to document and quantify the redistributive effects at stake between losers and winners of these policy changes, even within absorbed (or absorbing) regions. Such effects have been recently put forward by [Boyer et al. \(2020\)](#) and [Tricaud \(2021\)](#) as possible factors explaining the rise in protest movements like the Yellow Vests, or even populism.

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Figures



(a) Old regions (before the merger)



(b) New regions (after the merger)

Figure 1: Administrative division of France

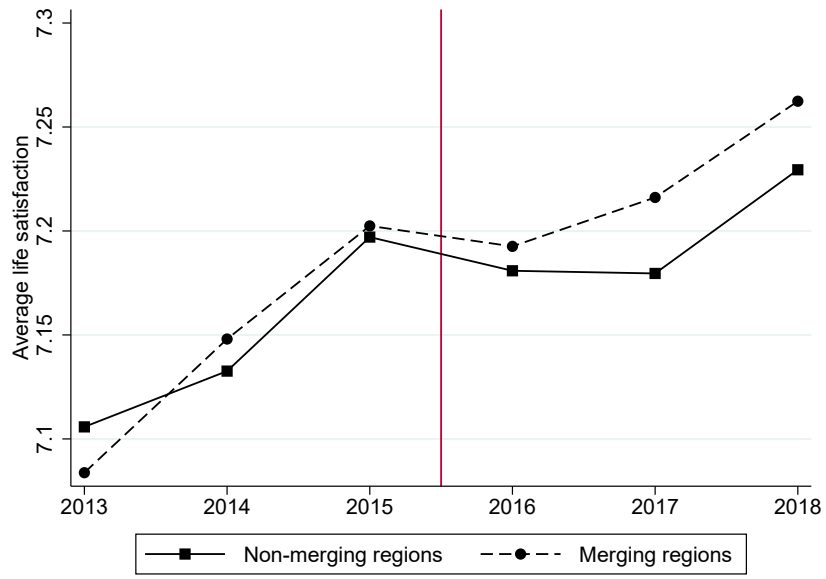


Figure 2: Evolution of life satisfaction across regions

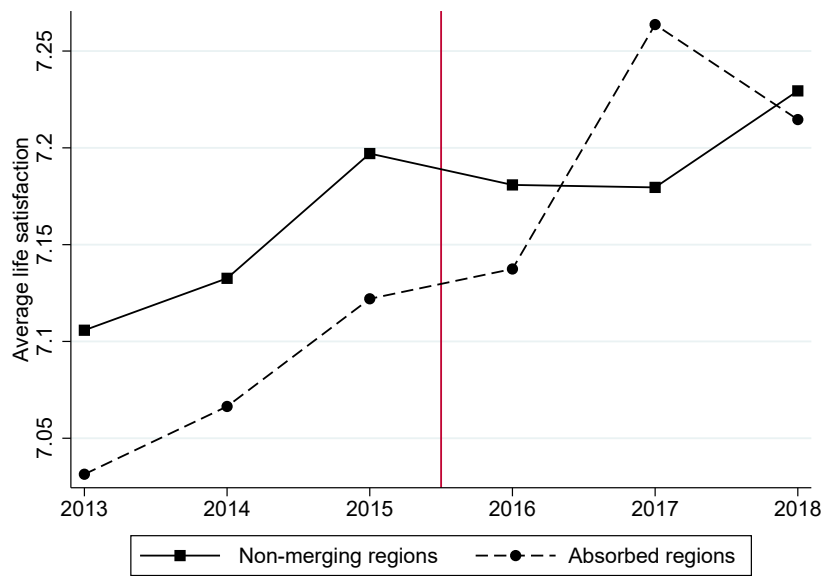


Figure 3: Evolution of life satisfaction across regions

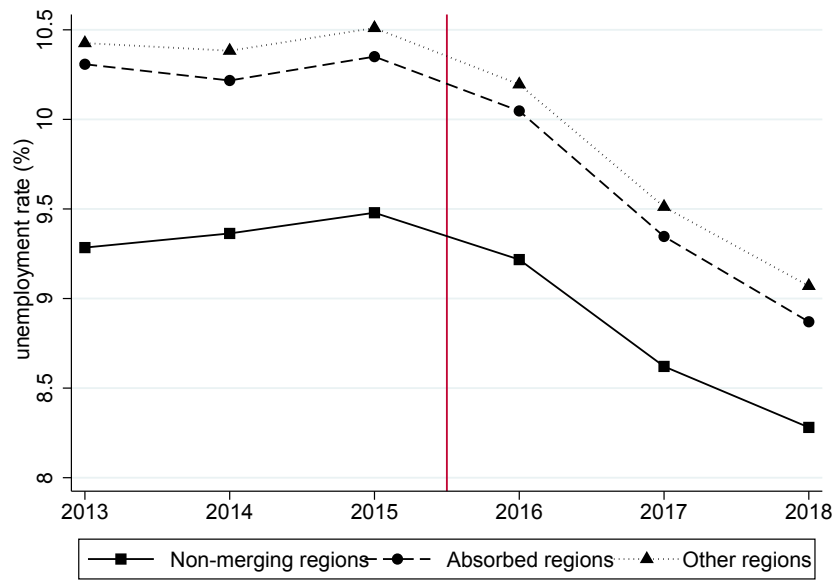


Figure 4: Evolution of unemployment rate across regions

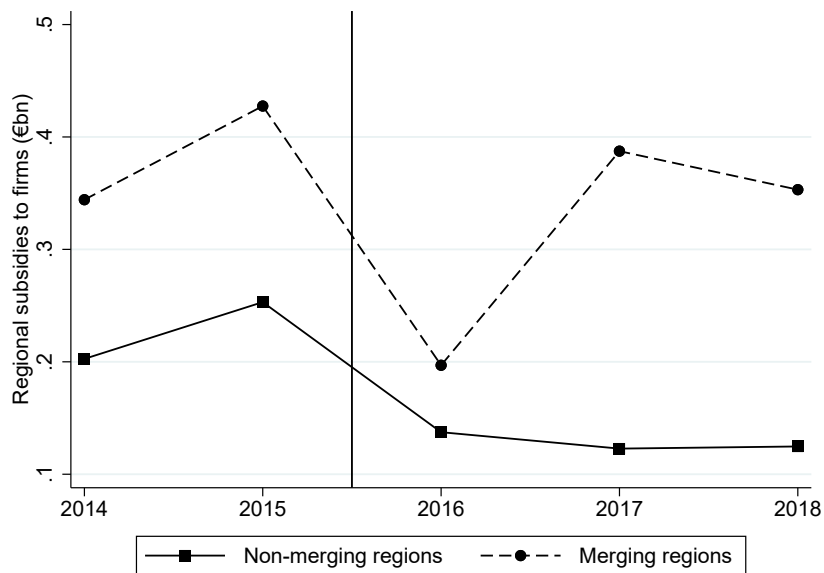


Figure 5: Evolution of regional subsidies to firms across regions

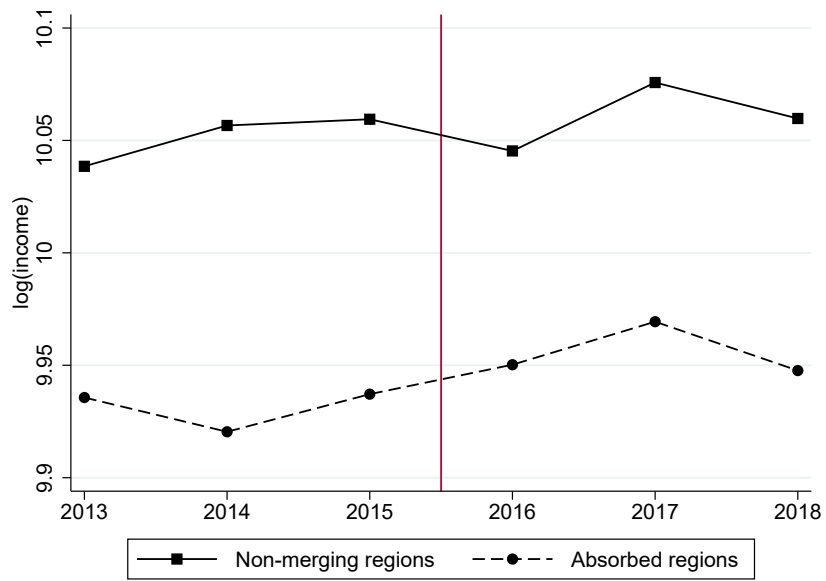


Figure 6: Evolution of income across regions

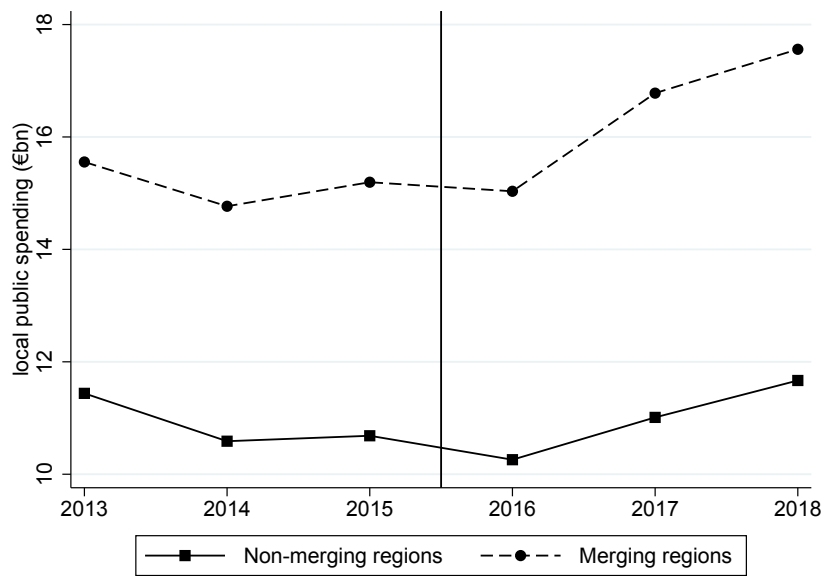


Figure 7: Evolution of local public spending across regions

Tables

Table 1: Descriptive statistics on treatment and comparison groups

Dependent variable	Absorbed, 2015		Diff. in means		Normalized diff.	
	Mean	SD	Absorbed (treat.) - Non merging (comp.) 2015	2018	Absorbed (treat.) - Non merging (comp.) 2015	2018
Life satisfaction	7.122	1.728	-0.075	-0.015	-0.031	-0.006
Control variables						
Income	23257	13456	-3927	-4022	-0.143	-0.063
Female	0.592	0.492	0.010	0.009	0.014	0.012
Age	53.454	17.650	-0.020	1.237	-0.001	0.050
Clerk	0.272	0.445	0.001	0.027	0.001	0.043
Farmer	0.044	0.205	0.021	0.011	0.084	0.047
Other	0.120	0.325	0.012	0.022	0.026	0.054
White collar	0.088	0.284	-0.075	-0.076	-0.162	-0.157
Self-employed	0.050	0.218	-0.006	0.001	-0.019	0.004
Intermediate	0.213	0.410	-0.009	-0.029	-0.016	-0.049
Undetermined	0.003	0.050	-0.002	-0.002	-0.019	-0.019
Blue collar	0.211	0.408	0.058	0.045	0.108	0.082
No degree	0.278	0.448	0.070	0.051	0.116	0.087
Other degree	0.003	0.050	0.000	0.002	0.001	0.024
High-school	0.276	0.447	-0.034	-0.024	-0.053	-0.038
Vocational	0.337	0.473	0.043	0.055	0.066	0.083
College	0.107	0.309	-0.079	-0.083	-0.160	-0.160
Employed	0.455	0.498	-0.037	-0.050	-0.053	-0.071
Unemployed	0.070	0.255	0.008	-0.002	0.022	-0.006
Student	0.029	0.169	-0.003	0.002	-0.014	0.006
Inactive	0.068	0.252	0.014	0.007	0.041	0.022
Undetermined	0.009	0.094	0.001	0.001	0.006	0.005
Retired	0.368	0.482	0.019	0.043	0.027	0.063
Single	0.211	0.408	-0.008	0.000	-0.015	0.000
Two adults. no child	0.397	0.489	0.035	0.015	0.050	0.022
Others. no child	0.061	0.240	0.010	-0.001	0.030	-0.004
Single parent	0.040	0.196	-0.006	-0.015	-0.020	-0.050
Two adults + 1 child	0.094	0.291	-0.005	0.012	-0.011	0.031
Two adults + 2 children	0.107	0.310	-0.012	-0.003	-0.028	-0.006
Two adults + 3 children	0.051	0.221	-0.007	-0.005	-0.021	-0.016
Others. with children	0.029	0.167	-0.008	0.000	-0.031	0.000
Undetermined	0.010	0.098	0.001	-0.004	0.010	-0.025
Sample size	Absorbed, 2015 2786	Absorbed, 2018 2409	Non-merging, 2015 5709	Non-merging, 2018 5341		

Source. French SRCV survey, 2013-2018.

Table 2: Effect of the merger on life satisfaction (Treatment: Merging regions)

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment \times Post	0.026 (0.024)	0.026 (0.023)	0.030 (0.020)	0.031 (0.019)	0.036* (0.021)	0.036* (0.021)
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	0.125	0.003	0.158	0.736	0.738

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.

Robust standard errors clustered by individual and region.

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

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

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment \times Post	0.082** (0.034)	0.075** (0.032)	0.076*** (0.027)	0.074*** (0.027)	0.083*** (0.030)	0.082*** (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	0.131	0.004	0.128	0.743	0.745

Same legend as Table 2.

Table 4: Heterogeneity of treatment effects

Treatment	absorption	merger
(1) Treatment \times Post \times Vocational	0.177*** (0.043)	
(2) Treatment \times Post \times Blue collar	0.229*** (0.063)	
(3) Treatment \times Post \times Single	0.182*** (0.058)	
Treatment \times Post		0.411** (0.187)
Treatment \times Post \times GDP/capita		-0.136** (0.067)
Controls	Yes	Yes
Time FE	Yes	Yes
Region FE	Yes	Yes
Individual FE	Yes	Yes
# of observations	49,279	90,734
# of individuals	18,187	32,872

Same legend as Table 2. Each specification in the top panel corresponds to a separate estimation.

Table 5: Effect of the merger on economic outcomes

	unemployment rate			public spending	local tax revenue
	(1)	(in pp) (2)	(3)	(in €bn) (4)	(in €bn) (5)
Merger \times Post	-0.178*** (0.032)			0.171** (0.068)	0.033 (0.047)
Absorbed \times Post		-0.210*** (0.044)			
Absorbing \times Post			-0.074* (0.041)		
Time FE	Yes	Yes	Yes	Yes	Yes
<i>Département</i> FE	Yes	Yes	Yes	No	No
Region FE	No	No	No	Yes	Yes
# of <i>départements</i>	96	53	48		
# of new regions				13	13
Observations	576	318	288	78	78
R^2	0.996	0.994	0.995	0.983	0.857

Appendix

Ordered models with random effects

The difference-in-differences 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 below). 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 (-1.65 against -1.66 in the specification including both controls and random effects), as shown by Table 6.²⁵ Even though point estimates cannot be compared, average partial effects, which are available upon request, can be, and 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 with 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.

²⁵Due 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

Table 6: Robustness checks (Treatment: Absorbed regions)

	Coef.	S.E.	Obs. N	R^2 or $\log(L)/N$
(1) Alternative treatment: less conservative definition	0.064**	(0.027)	57,198	0.745
(2) Alternative comparison group: excluding Île-de-France	0.077**	(0.032)	38,167	0.739
(3) Placebo experiment: fake merger in 2014	0.002	(0.039)	49,279	0.745
(4) Placebo experiment: fake merger in 2015	0.048	(0.032)	49,279	0.745
(5) Parametric specification: ordered Probit with RE	0.065**	(0.027)	49,279	-1.663
(6) Parametric specification: ordered Logit with RE	0.109**	(0.027)	49,279	-1.651
(7) Balanced panel estimation	0.094*	(0.054)	8,502	0.646
(8) Intermunicipal cooperation as a confounding factor?	0.082***	(0.030)	49,279	0.745
(9) Endogenous residential sorting	0.083***	(0.030)	49,279	0.747
(10) Differential impact of controls after the merger	0.065**	(0.030)	49,279	0.746

Same legend as Table 2. All specifications include controls, region FE, year FE, and individual FE (unless otherwise mentioned).

Event study analysis

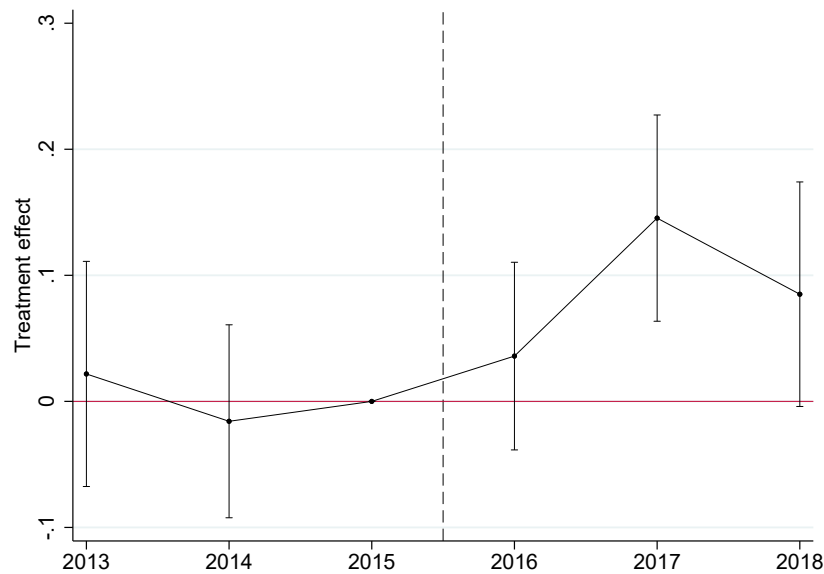


Figure 8: Event study approach (Treatment: Absorbed regions)

Synthetic control approach

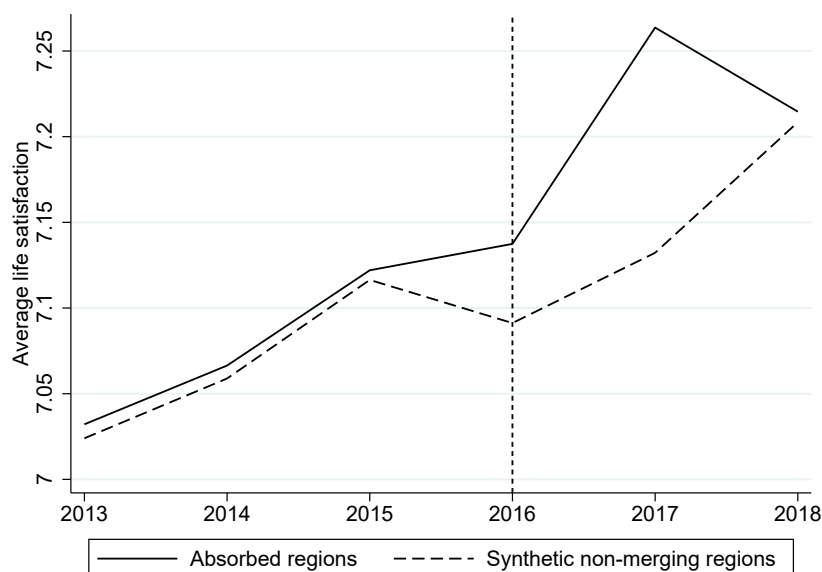


Figure 9: Synthetic control approach

Statistical issues, survey sampling and clustering

Table 7: Verbeek and Nijman tests for attrition (Treatment: Absorbed regions)

	(1)	(2)	(3)	(4)	(5)	(6)
Belongs to balanced panel	-0.042 (0.048)	-0.029 (0.043)	-0.053 (0.048)	-0.050 (0.043)	.	.
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	0.131	0.004	0.128	0.743	0.745

Same legend as Table 2.

Table 8: Impact of the clustering level on the precision of the estimated ATE (Treatment: Absorbed regions)

	(1)	(2)	(3)
Level of clustering	individual	former region	new region
Treatment \times Post	0.082*** (0.030)	0.082* (0.038)	0.082** (0.034)
Controls	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Region FE	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
# of observations	49,279	49,279	49,279
# of individuals	18,187	18,187	18,187
# of clusters	18,187	12	10

Same legend as Table 2 (one-way clustering instead of two-way clustering).

Online Appendix

A Supplementary figures

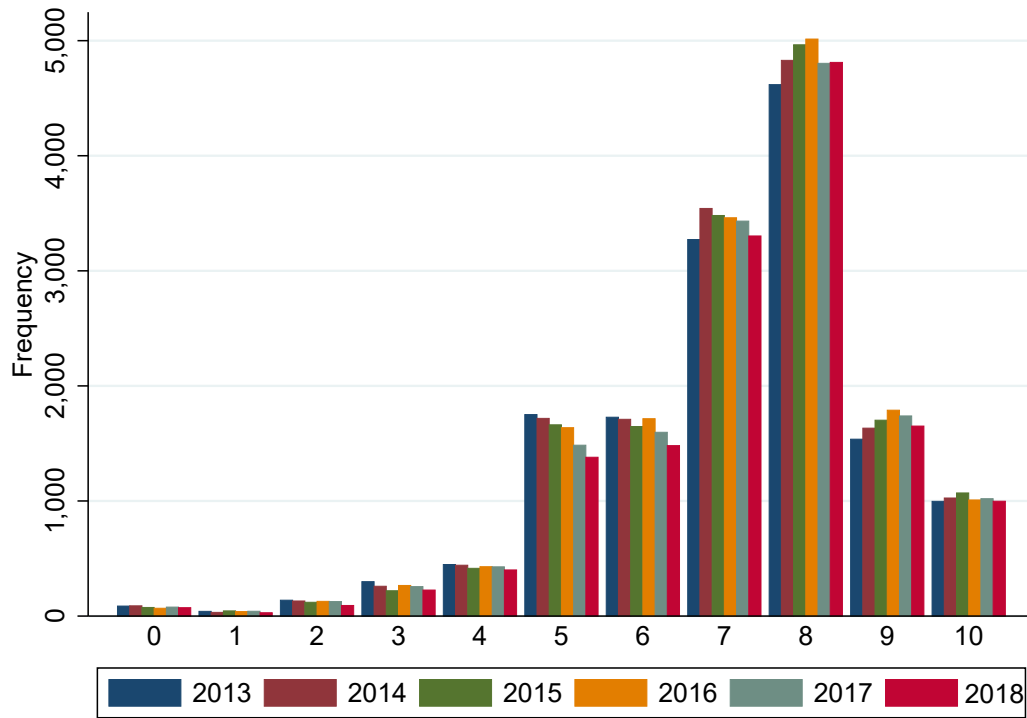


Figure 10: Evolution of life satisfaction

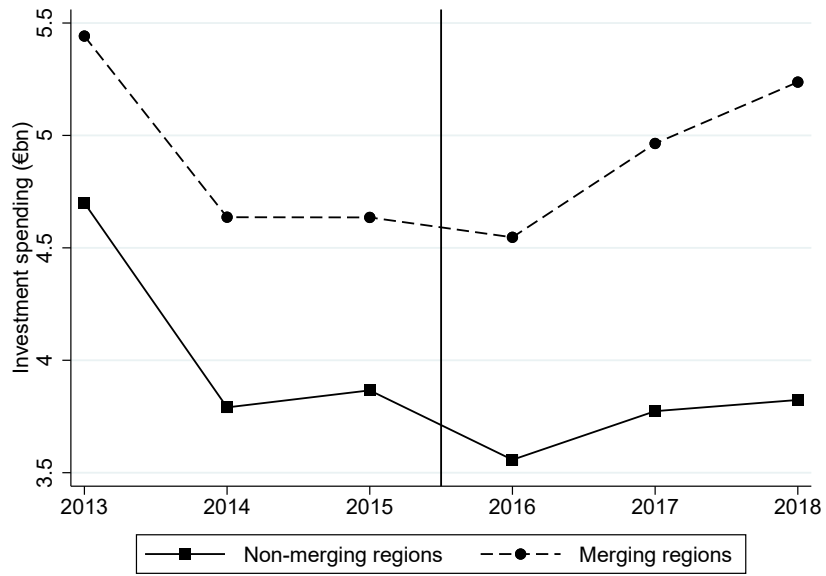


Figure 11: Evolution of investment spending across regions

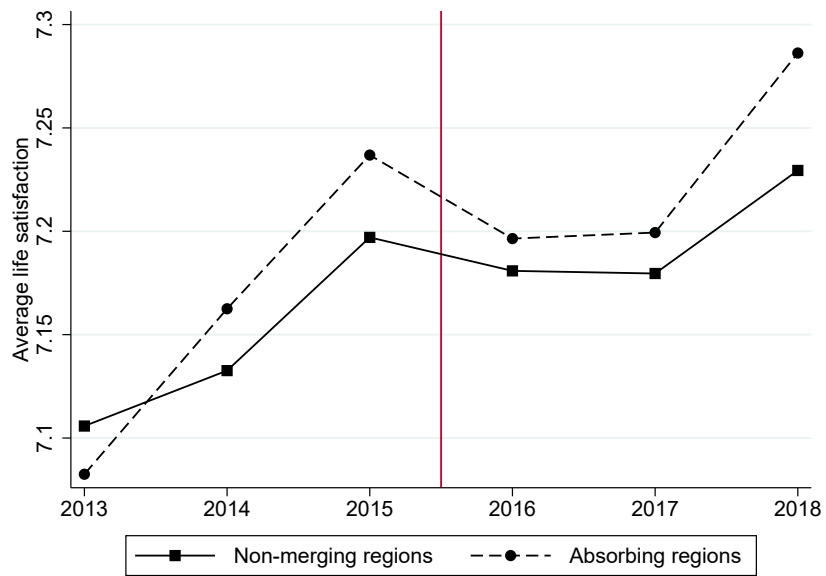


Figure 12: Evolution of life satisfaction across regions

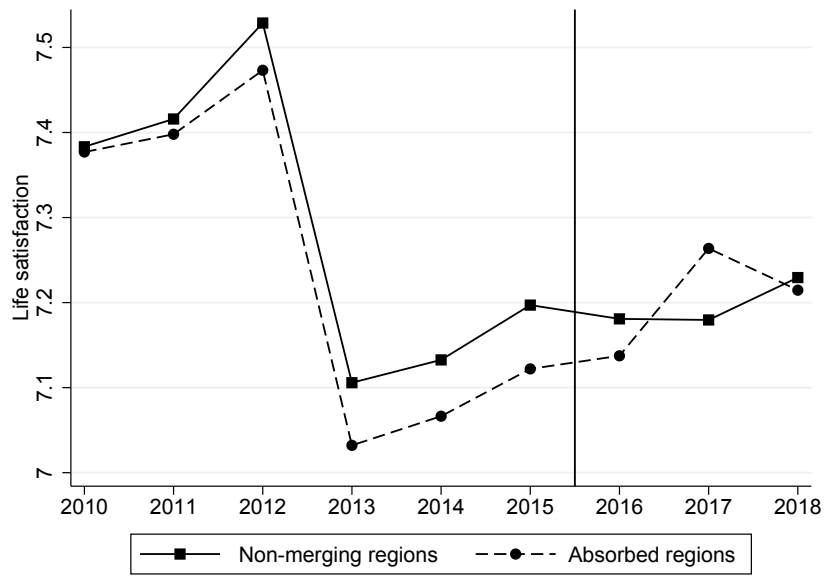


Figure 13: Evolution of life satisfaction across regions (2010-2018)

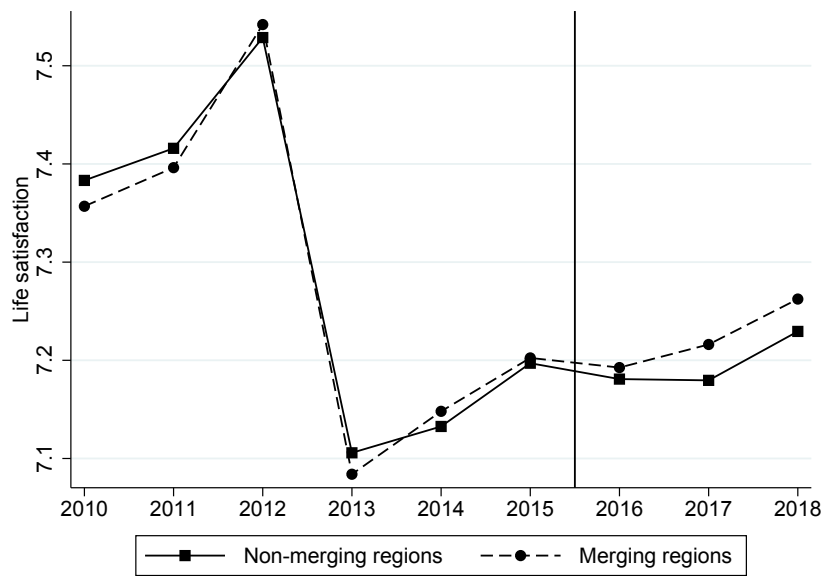


Figure 14: Evolution of life satisfaction across regions (2010-2018)

B Supplementary tables

Table 9: 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

Note. Old regions have a (*) when their capital became the capital of the new regional entity.

Table 10: Summary statistics on the working sample

	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	0.37	0.48	0	1
Absorbing (conservative)	0.26	0.44	0	1
No merger	0.37	0.48	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
Observations			90,734	

Source. French SRCV survey, 2013-2018.

Sample. Unbalanced panel of 32,872 individuals.

Table 11: Effect of the merger on life satisfaction (Treatment: Absorbing regions)

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment \times Post	0.014 (0.030)	0.021 (0.028)	-0.008 (0.024)	0.001 (0.024)	-0.010 (0.026)	-0.009 (0.026)
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	57,033	57,033	57,033	57,033	57,033	57,033
# of individuals	20,899	20,899	20,899	20,899	20,899	20,899
R^2	0.004	0.128	0.004	0.125	0.744	0.746

Same legend as Table 2.

Table 12: Effect of regional integration on life satisfaction (Treatment: Absorbing regions, less conservative definition)

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment \times Post	0.024 (0.027)	0.026 (0.025)	0.012 (0.022)	0.018 (0.021)	0.015 (0.024)	0.017 (0.024)
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	66,804	66,804	66,804	66,804	66,804	66,804
# of individuals	24,387	24,387	24,387	24,387	24,387	24,387
R^2	0.004	0.125	0.004	0.122	0.740	0.742

Same legend as Table 2.