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MAKING JOBS OUT OF THE ENERGY TRANSITION: EVIDENCE FROM THE FRENCH ENERGY EFFICIENCY OBLIGATIONS SCHEME

Guillaume Wald, François Cohen, Victor Kahn

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Postal Address:
Institut d'Economia de Barcelona
Facultat d'Economia i Empresa - Universitat de Barcelona
C/ John M. Keynes, 1-11
(08034) Barcelona, Spain
Tel.: + 34 93 403 46 46
ieb@ub.edu
http://www.ieb.ub.edu

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ABSTRACT: Vast amounts are being invested in the energy transition worldwide, with optimistic expectations of economic growth and green job creation. Yet, we crucially lack ex-post validations of the multiplier effects widely used to quantify new green jobs. Focusing on the French Energy Efficiency Obligations scheme, this paper provides the first ex-post estimate of the employment effect of a large energy-retrofit investment program. We exploit a discontinuity in the provision of subsidies and use a novel synthetic control method on disaggregated data to estimate regional-level employment effects. We estimate that the scheme created 1.4 jobs per million euros invested.

JEL Codes: J21, H23, Q43, Q48

Keywords: Energy Efficiency, Green Jobs, Employment, Energy Transition, Subsidies, Certificates.

Guillaume Wald
Mines Paris PSL

François Cohen
UB Department of Economics,
Energy Sustainability Chair & IEB

Victor Kahn
Mines Paris PSL

1. Introduction

Vast amounts are being invested in the energy transition worldwide. For instance, recovery packages[1] following COVID-19 have committed nearly one trillion dollars of public spending to green investments globally, amounting to a third of all recovery spending (O'Callaghan, Murdock and Yau 2021). Policymakers have defended green investment programs not only for environmental reasons, but also with the conviction that they could significantly boost economic growth and employment. The European Commission believes that meeting the 2030 climate and energy targets could create almost one million new green jobs (European Commission 2020). Such positive expectations for the impact of green investment on employment are supported by several ex-ante forecasts using input-output models (Mikulić, Rašić Bakarić and Slijepčević 2016, Markandya, et al. 2016, Dell'Anna 2021) or computed general equilibrium models (Sooriyaarachchi, et al. 2015, Wei, Patadia and Kammen 2010). However, we considerably lack ex-post confirmations.

Focusing on the French Energy Efficiency Obligations scheme, this paper provides the first ex-post estimate of the job-creation impact of a large-scale energy retrofit program. This French scheme is among the largest energy efficiency policies in Europe (Broc, Stańczyk and Reidlinger 2020). Moreover, a study on retrofits is particularly relevant since they correspond to one of the largest green investment categories. Buildings alone account for 25% of global emissions (International Energy Agency 2021). Besides, retrofits are particularly important in the debate on green job creation since they are believed to have a higher job-creation potential than other green investment types, such as wind energy.[2]

[1] e.g. NextGenerationEU (European Commission 2021) or the American Rescue Plan Act (Office of the Federal Register, National Archives and Records Administration 2021).

[2] Retrofits are one of the main green investment types. The Renovation Wave in the European Union includes a EUR 200 billion investment in "greening […] buildings, improving lives and creating jobs" (European Commission 2020). Moreover, ex-ante studies have forecasted impacts in the range of 12 to 29 direct and indirect jobs created per million dollars invested in energy retrofits, of which about half would be direct hires in the energy retrofit sector (BPIE, Buildings Performance Institute Europe 2020). Energy retrofits are often performed by SMEs (European Commission 2019) and involve manual workers who are most likely to be negatively impacted by other environmental policies (Walker 2011, Vona, Marin, et al. 2018, Yip 2018, Marin and Vona 2019, Vona 2019, Marin and Vona 2021).

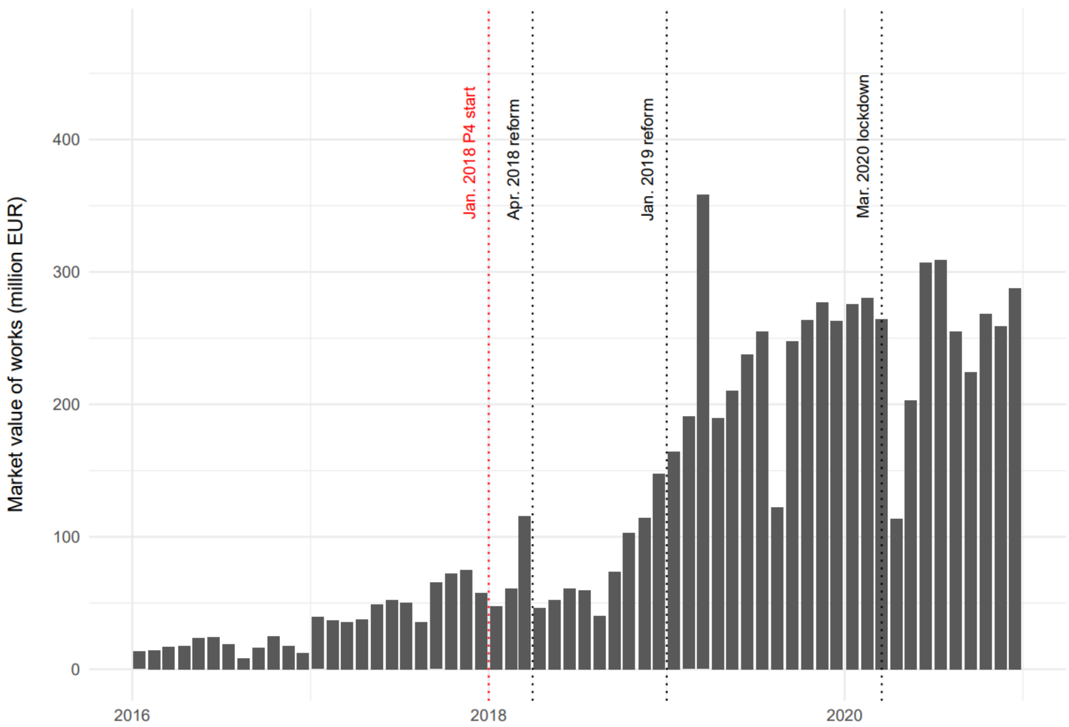
Our analysis makes two main contributions. The first one is to provide an estimate that is specifically valid for energy retrofits, with a breakdown by employment type between short-term and long-term contracts, and regional disaggregation across 13 French regions. The ex-post evidence on green job creation from investment programmes is scarce. It includes, first and foremost, the evaluation of the 2009 American Recovery and Reinvestment Act (ARRA) by Popp et al. (2021), who found that ARRA created 2 to 4 jobs in the construction sector. This U.S. estimate is all-encompassing since ARRA included energy retrofits, but also green infrastructure, or the installation of renewable energy technologies. A limittion is that, if different investment types entail different levels of job creation, then the analysis by Popp et al. (2021) is not specific enough to inform sectoral investment programs. In that regard, focusing on job creations in Spain, Fabra et al. (2023) find radically different impacts between solar versus wind energy investments, with the job content of solar energy being much higher than for wind.[3] Under these circumstances, we need sector-level analyses, and one is clearly missing for energy retrofits. Ex-ante studies have not been challenged and suggest very high employment effects. The multiplier effect used by the European Commission for energy retrofits is currently at 8.52 jobs per million euros, which is more than twice as high as the upper bond estimate in Popp et al. (2021). Our results suggest that effects are much smaller, calling for a downward revision of all employment effects from energy retrofits.

We also make a methodological contribution by using a discontinuity in the provision of the policy and applying a state-of-the-art synthetic control model on disaggregated data, allowing us, for instance, to capture regional heterogeneities. More specifically, we exploit a discontinuity in the provision of subsidies to French households and businesses through the French Energy Efficiency Obligation (EEO) scheme. The discontinuity is due to a set of policy changes between January 2018 and January 2019. It is observable in **Figure 1**, where we estimate that the monthly investment for insulation and heating retrofit operations through the scheme went from around EUR 30 million before January 2018 to more than EUR 160 million after this date, a 5-fold increase. This discontinuity reduces identification issues when

[3] For each million dollars invested in solar panels, 0.65 local jobs were created during the construction phase. In contrast, they find very small and non-significant effect on local employment for investments in wind energy.

looking at the impact of the policy on employment levels. This allows us to use a state-of-the-art method to build a synthetic control estimator with regional disaggregation (Abadie and L'Hour 2021) and compute synthetic employment levels for each regional retrofitting industry. At the level of each French region, our analysis compares affected sectors with unaffected sectors that are pooled and weighted to create a synthetic control whose pre-reform employment trend matches the employment trend of the affected sectors. The model is in spirit similar to standard synthetic control models (Abadie and Gardeazabal 2003, Abadie, Diamond and Hainmueller 2010), except for the fact that it uses disaggregated data and hence relies on more variation, making it is less subject to spurious findings. It also allows us to provide regional estimates of the employment effect of the policy across France.

Figure 1: Total market value of works certified by the French EEO scheme, in million EUR / month



Source: The figure displays the monthly market value of works driven by the EEO scheme. We used data from the French Ministry of Ecological Transition to compute the monthly number of certificates generated by projects within the EEO scheme. We then multiply this number by the contemporaneous price of certificates (at the beginning of the project) to compute a monthly value of investment driven by the policy.

We discuss our methods and hypotheses in detail, including robustness checks to ensure that our findings are not driven by potential biases or shortcomings in the method employed, such as a violation of the stable unit treatment value assumption (SUTVA) or anticipatory effects. We also perform inference tests to ensure our results are robust to specification choices.

Stopping the analysis just before the COVID-19 pandemic in February 2020, we observe a clear impact of the French energy retrofit policy on employment in energy retrofit companies 26 months after the discontinuity in January 2018. This impact seems to continue until after the pandemic. We estimate that at least 4,800 jobs were created thanks to the policy, equivalent to about 1.4 direct jobs created per million euros invested annually. This figure is much lower than ex-ante estimates for direct jobs (at about 4.3 to 9.2 direct jobs per million dollars invested) (BPIE, 2020), and would therefore suggest that the expectations of job creation through retrofits should be updated downwards. Since Fabra et al. (2023) find 0.65 jobs in the solar industry, energy retrofits could have a higher potential for job creation. However, these figures do not constitute homogeneous comparisons since these studies have a different scope and use different methods.[4]

We dig further, taking advantage of the sharp difference in job stability between fixed-term and open-ended contracts in France to assess how permanent job creations from energy retrofit stimuli might be. A problem is that jobs could be terminated as soon as public aid is removed. Short employment contracts could also affect the quality of the energy retrofits. This is because poor workmanship quality is often mentioned as a reason for the energy performance gap (Giraudet, Houde and Maher 2018). In contrast, temporary subsidies could help structure value chains, stimulate innovation, and lead to long-term green job creation.

In that domain, results from previous studies are mixed. Popp et al. (2021) found stronger impacts in the long term compared to the short term, possibly because investments may allow structuring value chains. In contrast, the analysis by Fabra et al. (2023) suggests lower job creation in the long term for industries that rely on the installation of equipment, especially since maintenance is likely to be less labor intensive. In the case of energy retrofits, we find

[4] For instance, we focus on direct jobs at national scale whereas Fabra et al. (2023) focus on direct jobs at a very local scale (finer than our regional resolution). Popp et al. (2021) use a method that should provide overall results for both indirect and direct job creations.

that the majority of new hirings were performed with durable, open-ended contracts, suggesting that the temporary stimulus helped consolidate a relatively new sector.

Finally, recent economic studies have questioned the effectiveness of energy retrofit programs, with realized energy savings being significantly lower than predicted savings (Fowlie, Greenstone and Wolfram 2018, Liang, et al. 2018, Lang and Lanz 2022, Davis, Fuchs and Gertler 2014). Our paper contributes to the growing literature pointing to significant co-benefits of energy retrofit programs despite lower-than-expected savings, including: comfort gains (Aydin, Kok and Brounen 2017); public health benefits (Howden-Chapman 2007); economic transfers from high-income households to low-income ones (Darmais, Glachant and Kahn 2022); and, in the case of our paper, job creation. Furthermore, empirical research on the green transition has focused on the employment effect of restrictive policies to cut down emissions (Walker 2011, Hafstead and Williams 2018, Vona, Marin, et al. 2018, Yip 2018, Marin and Vona 2019, Vona 2019, Metcalf and Stock 2020, Marin and Vona 2021), with much fewer analyses looking at investment policies. This paper contributes to filling this gap, finding a moderate job potential of investments in energy retrofits.

The remainder of this paper is organized as follows. Section 2 describes the studied policy. Section 3 presents our data and Section 4 our method. Section 5 presents our results, which we discuss in Section 6; Section 7 concludes.

2. The French Energy Efficiency Obligation scheme

In 2006, the French government established a system of energy efficiency obligations (*Certificats d'Economies d'Energie* in French) under the supervision of the General Directorate of Energy and Climate (*Direction Générale de l'Energie et du Climat, GDEC* in French). The scheme, still ongoing today, consists of periods of four years during which a national energy savings target must be met. It is in its 5th period since January 1st, 2022, with a total energy savings target of 2,500 cumulative TWh 2022-2025.[5] Each period-specific

[5] TWh are cumulative because the energy savings are calculated on the lifetime of the energy operation achieved. Part of this target (730 cumulative TWh during the 5th period) must go to projects benefiting to low-income households, as explained in the following pages.

national energy savings target is broken down into individual energy savings targets for each obligated party. The obligated parties are energy providers, mainly gasoline, electricity, and natural gas providers. They must fulfill their individual obligations by obtaining energy savings certificates delivered by the regulator for efficiency improvements performed in either the residential, the industrial or the tertiary sectors. Each certificate is worth 1 cumulative kWh, corresponding to a proportional decrease in future energy use. Individual obligations depend on the amount and type of fuel sold by providers during the period in the residential and tertiary sectors.[6] In addition, since 2016, a share of certificates must be obtained from subsidizing renovation efforts in lower-income households with annual income roughly below the median income in France. There are therefore two individual obligations per obligated party (a general obligation and a low-income obligation) and two types of certificates (general and low-income). For instance, during the 4th period (2018-2021), for each kWh of electricity sold, energy providers had to obtain 0.463 general certificates and 0.154 low-income certificates (Art. R221-4-1, French Energy Code). It is possible to fulfil a general obligation with low-income certificates, but it is not possible to use general certificates to fulfil low-income obligations.

To obtain certificates, the obligated parties must have an active role in providing an incentive to renovation projects, i.e., by funding entirely or in part renovation projects. They must be mentioned as such on each project invoice. Renovation projects can be undertaken to the benefit of residential, industrial, or tertiary stakeholders. Once a renovation is complete, the obligated party claims the quantity of certificates corresponding to the retrofit operations undertaken. The number of certificates associated with each energy retrofit operation is set in advance by the regulator. This quantity essentially depends on the energy savings that each operation conveys. There are more than two hundred standard energy retrofit operations that

[6] Each fuel has a different coefficient converting sales (in kWh) into obligations (in certificates). The calculation can be complex. For the fourth period, for instance, the regulator first calculated the total share of energy provided by each fuel (from sales in MWh) and its market share (from sales in euros). These two shares were then weighted (with a weight of 75 percent for the energy share and 25 percent for the market share) to calculate the required contribution of a given fuel to the total obligation during the fourth period (of 2,133 cumulative TWh for 2018-2022). Finally, the regulator forecasted total energy sales per fuel during the fourth period. The coefficient converting sales into obligations is the ratio between the required contribution (in cumulative TWh, and therefore in certificates) and the forecasted sales (in MWh) of each fuel. It is therefore expressed in certificates per MWh.

can provide a set number of certificates. For instance, in January 2018, one square meter of insulated wall in an electricity-heated house in the north of France was associated with 2,400 certificates. If the renovation effort benefits a household with income below a threshold close to the national median, then the certificate obtained is a low-income certificate. Moreover, the number of certificates obtained from the same renovation effort is doubled if the renovation benefits a household that belongs to the first quartile of income.

The obligated parties can delegate all or part of their obligations to third-party companies, called delegated parties, usually energy service providers or simply traders. Obligated and delegated parties are allowed to exchange certificates through over-the-counter operations. Therefore, while there is no organized market for certificates, these can still be traded between different parties. Monthly price indices for general and low-income certificates are publicly available from the national register of EEOs (called EMMY).[7] They correspond to the average price of all the certificates sold during a month.[8] These indices are used as a signal by businesses, who may monitor their activities and make decisions under the scheme based on the evolution of these indices. Even though obligated and delegated parties freely set the financial conditions for the home improvements that they subsidize, energy efficiency grants to households ultimately depend on the number of certificates associated with each energy retrofit operation, and the price of certificates as signaled by the price indices of certificates. This is considering that obligated parties can always buy certificates from others through over-the-counter operations.

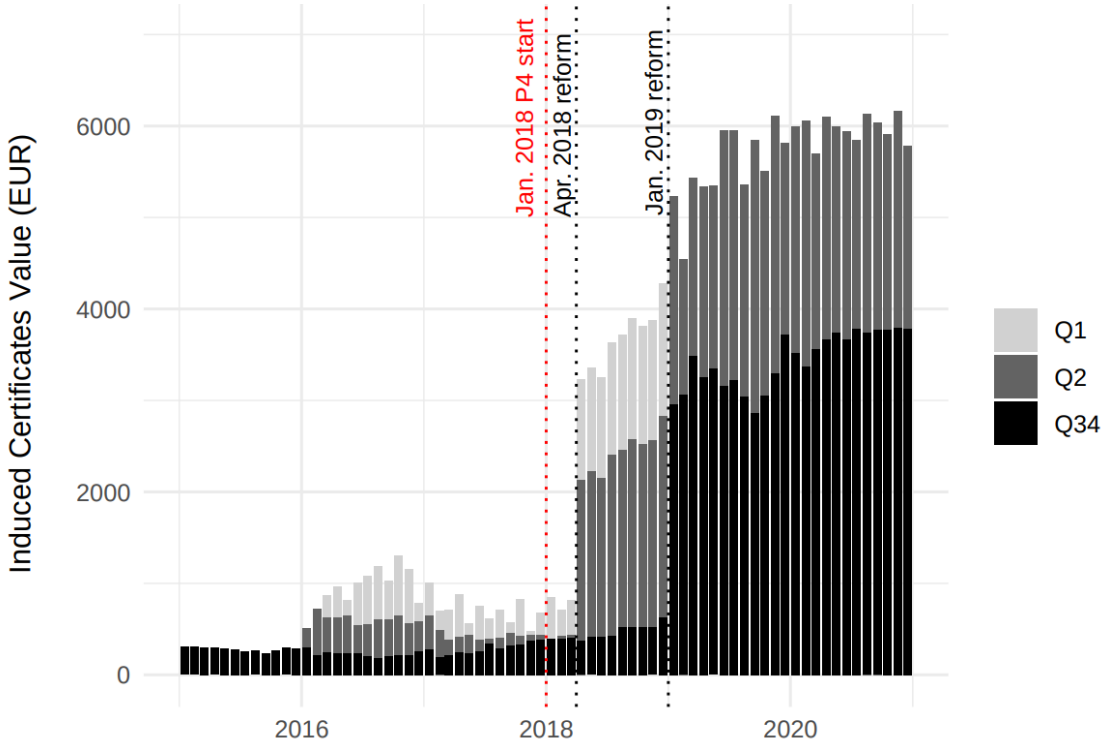
From January 2018 onwards, the value of the certificates delivered for many operations rose sharply, explaining the sudden change in the market value of the works performed (displayed in **Figure 1)**. Across all retrofit types, the value of subsidies delivered to households through the French EEO scheme increased substantially, from less than EUR 1 billion in 2017 to EUR 2.5 billion in 2019 (Darmais, Glachant and Kahn 2022).

[7] For more information on the register, see: https://opera-energie.com/emmy-registre-national-cee/.

[8] This price index is sometimes difficult to interpret because it includes certificates sold in very different conditions, not only certificates traded with contracts “on the spot” happening during month m, but also certificates from forward contracts that came to maturity during month m. Moreover, the price index also includes price information from trades happening between subsidiary companies belonging to the same mother company.

The case of heat pumps is especially telling. **Figure 2** displays the evolution of the market value of the certificates that obligated and delegated parties obtained after installing a heat pump. This value has been computed by multiplying the price of certificates with the number of certificates associated with a heat pump. We provide this information separately for different income quartiles of households. As explained before, the obligated and delegated parties can claim low-income certificates for home improvements performed in the 1st and 2nd quartiles of income, and twice as many of these certificates for improvements benefiting the 1st quartile. **Figure 2** shows that, for all household types, the market value of the certificates delivered for the installation of a heat pump increased sharply in the second haft of 2018 and after the January 2019 reform.

Figure 2: Evolution of the market value of the certificates for heat pumps



Source: French Ministry of Ecological Transition (SDES, Ministère de l'Environnement 2023). The bars represent the average market value of certificates associated with heat pumps that fulfil the energy efficiency eligibility conditions of the scheme. The figures break down the market value by type of residential household (Q1 for those in the first quartile of income, Q2 for those in the second quartile, and Q34 for those in either the 3rd or 4th quartile). The value of certificates is calculated by multiplying the number of certificates associated with each energy operation by the relevant price index (for either general or low-income certificates). Units on the y-axis are in current euros.

Several key changes explain the sharp increase in the value of individual operations from 2018 onwards. First and foremost, the scheme entered its fourth period of implementation in January 2018. The total obligation, set at 2,133 cumulative TWh for 2018-2021, became nearly twice as ambitious as the total obligation of 1,166 cumulative TWh of the previous period (2014-2017). This drastic change in ambition could have created wrong incentives for obliged actors to primarily target households that were looking to implement energy retrofits anyway and offer them only limited financial support. This would have allowed obligated parties to reach their legal obligations by spreading costs between more works, but at the expense of overall policy additionality. To avoid this scenario, the French government inflated the number of certificates that it would grant for specific operations if the subsidy exceeded a set value. A first reform occurred in April 2018, when the number of certificates for heat pumps benefiting low-income households was multiplied by more than 4. The regulator also increased by 15 percent the number of certificates obtained for attic, roof and floor insulation benefiting households belonging to the 2nd quartile of income. In January 2019, another reform substantially inflated the number of certificates delivered for all heating-system related operations. The regulator also raised the number of certificates granted for insulation operations benefiting households in the second income quartile to the same level as for the first quartile, leading to a 65-percent increase.

The April 2018 and January 2019 reforms explain the sudden jumps in the values displayed in **Figure 2**. They mitigated the stringency of the increase in the individual obligation of each energy provider during phase 4, hence the overall objective during this period. However, they encouraged much stronger support for each investment. Altogether, the quantity and value of investments through the EEO scheme became substantially higher after January 2018.

3. Data

To estimate the impact of the EEO scheme on employment, we obtained monthly data on all hires and terminations of employment contracts for each business in Metropolitan France. The data comes from the Worker Movement Database (WMD) of the French Ministry of Labour (DARES 2023). It is available from 2015 to 2022. In the WMD, employers are classified with 732 codes corresponding to different sectors. Later, this will allow us to focus on the two sectors most effected by the policy: those of "insulation works" and the "installation of heating equipment".[9]

The WMD collates all employment records from an official document that companies must fill every month, entitled the Nominative Social Declaration (NSD).[10] The NSDs contain information about employee activity periods including, among other things, the start and end dates of each employment contract, the type of contract (e.g., permanent, or fixed term), sick leaves, maternity, and paternity leaves. However, due to missing data, the WMD does not allow to directly compare the total numbers of hires and terminations at sector level over time. This is because the NSDs started as a voluntary scheme in 2013, became compulsory for large companies in 2015 and finally for all businesses in 2017. Despite being compulsory since 2017, several small companies did not fill any NSD before 2019, when automation ensured that all companies were registered into the system and filling their NSD every month. At the beginning of 2016, only 33% of businesses filed an NSD. They were 60% in 2017 and 80% in 2018. Compliance rates strongly depended on business size. More than 90% of companies with more than 50 employees were already filing their NSD by mid-2016, against only half of businesses with less than 10 employees.

To account for missing data and create homogeneous time series, the Ministry of Labour (DARES 2018) has developed a method of weights that extrapolates entries and exits in businesses with missing declarations. In a nutshell, the method consists in associating a weight to each observation (a business in month *m* and year *t*), each weight being inversely proportional to the probability that an observation would have filled the NSD. This is very close to what would be done in a survey, where weights are given to each respondent according to their inverse probability of response. Inverse probabilities were estimated for different classes of respondents according to the number of employees in the business, the number of subsidiary businesses the mother company has, the region of the business, and its

[9] In the dataset, these are codes 4329A and 4322B respectively.

[10] *Déclaration Sociale Nominative* in French

activity sector (tertiary, industry, or construction), the age and the revenue of the business.

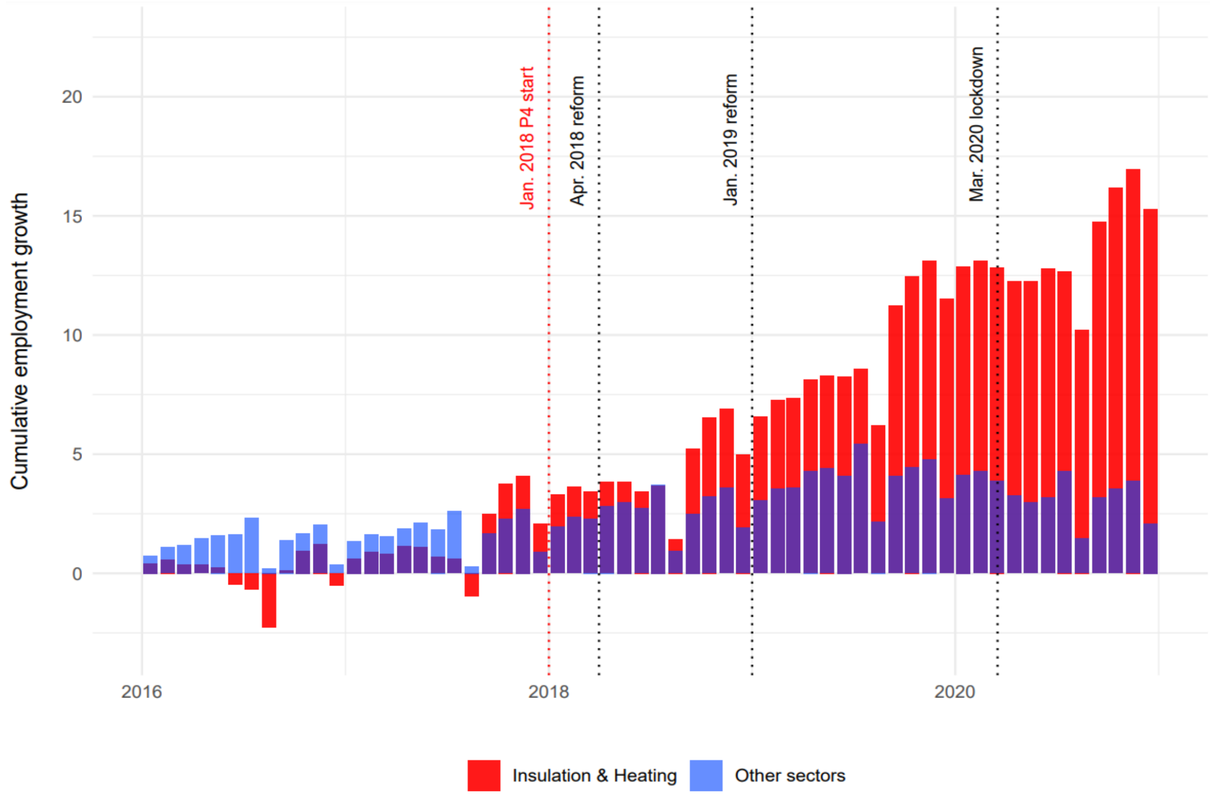
The weighted data can be swiftly used to recalculate total employment levels. For instance, in 2016, the retrofitting industry gathered around 100,000 workers, or about 1% of total employment in France. However, for this analysis, we are above all interested in the evolution of employment levels over time. For this, we focus on the data recording entries and exits rather than total employment. This is because the number of employees recorded in the WDM was smoothed by the data provider with a 3-month moving average. In contrast, monthly entries and exits truly represent shifts in employment from one month to another. We compute the weighted numbers of entries and exits in each month and in each sector, and by region in Metropolitan France between 2016 and 2020.[11] We then calculate *cumulative employment growth* in each sector and region since January 2016 as the sum of all new contracts since January 2016 minus all contract terminations.[12] This variable corresponds to the stock of net job creations since the start of our observation period (January 2016).

Cumulative employment growth is displayed for the "insulation" and "installation of heating equipment" sectors, versus all other sectors in France, in **Figure 3**. The three first vertical lines correspond to the start of the fourth period of the scheme and the two subsequent reforms in the delivery of certificates in April 2018 and January 2019. The last line corresponds to the beginning of the COVID-19 lockdown in France, date after which differences may become less comparable as different sectors were affected differently by the pandemic and the government-support schemes implemented to fight COVID-19.

[11] The dataset starts in the second semester of 2015. However, the data collection quality at the beginning was substantially lower due to the progressive rollout of NSDs. For that reason, we do not use 2015 data in our baseline analyses.

[12] To obtain this measure, we use the movement (entry and exits) data for each month and sector. New hires increase employment in each sector, while terminations decrease it. We therefore weight each movement (either an entry or exit) by the time span between the movement date and the end of the month. We then aggregate this weighted measure of employment growth at the sector level for each month, and compute its cumulative sum.

Figure 3: Comparison of cumulative employment growth for insulation and heating, versus all other sectors (% of Jan. 2016 total employment)



Notes: when "other sectors" (blue) and "insulation and heating" (red) overlap, the color displayed on the graph becomes purple. The energy renovation sectors in red are those corresponding to "insulation" and the "installation of heating equipment". They correspond to codes 4329A and 4322B respectively in the data from the French Ministry of Labor (2023). National aggregates are computed monthly and rely on the weights developed by the French Ministry of Labor (2018) to account for missing NSD files.

To evaluate the effect of the EEOs policy changes on employment in the insulation and heating sectors, we ultimately compare the evolution of employment in these two sectors and in other sectors that are unaffected by the reforms of the EEO scheme. **Figure 3** shows that the employment in the two energy renovation sectors experienced a much faster growth after the start of the fourth implementation period, as compared to employment in other sectors. More precisely, cumulative employment growth in the sectors of "insulation" and "installation of heating equipment" was 6.3 times higher in February 2020, as compared to December 2017. In contrast, cumulative employment growth for all sectors apart from "insulation" and "installation of heating equipment" was only 4.8 times higher in February 2020, as compared to December 2017. **Figure 3** also suggests that the policy changes might have been anticipated by a few months, something we analyze later in one of our robustness checks (in **Appendix C.2**).

Besides, France has a dual employment contract system. Employers can provide fixed-term contracts or permanent contracts. In general, it is not possible to use fixed-term contracts beyond 18 months of contract duration, with some rare cases allowing fixed-term contracts to be of 24 months. The WMD distinguishes between both types of contracts, allowing us to compute cumulative employment growth for permanent and fixed-term contracts (descriptive statistics in **Appendix A**). We use this piece of information to gauge whether the EEO reforms led to a temporary increase in jobs, or to a more permanent strengthening of the energy retrofit sectors.

4. Methodology

To assess the impact of the EEO reforms on employment in the treated sectors, we use a state-of-the-art synthetic control method on disaggregated data (Abadie and L'Hour 2021). With this method, this paper compares cumulative employment growth in "insulation" and the "installation of heating equipment" with cumulative employment growth in synthetic control groups. We do so at regional level for 13 French regions,[13] making pairwise comparisons between the treated sectors and their synthetic controls in each region separately, and then aggregating regional impacts at national level. We build those synthetic control groups with some of the other sectors available in the WMD data, which we are sure were not impacted by the EEO reforms. This is different from comparing treated and control regions, which is the most common type of applications of synthetic control methods. However, comparing treated and control sectors is an equally valid method, followed by Falkenhall, Månssonn and Tano (2020) in their analysis of the impact of a VAT reform in Sweden.

[13] This regional divide has been in place since 2016, when some of the 22 former metropolitan regions (corresponding to the NUTS 2 level) were merged to reduce administrative costs.

Synthetic control methods on aggregated data (Abadie and Gardeazabal 2003, Abadie, Diamond and Hainmueller 2010) have been widely used in labour economics (Bohn, Lofstrom and Raphael 2014, Allegretto, et al. 2017, Reich, Allegretto and Godoey 2017, Peri and Yasenov 2019, Wiltshire 2023, Jardim, et al. 2022). They are appropriate for policies that are implemented at aggregate level and affecting a small number of units (Abadie 2021). The reform of the French EEOs, which affected all Metropolitan France at the same time but would only have had an impact on job creation for a small set of sectors, would fit this description.[14] In these cases, synthetic controls have several appealing properties compared with other econometric tools commonly used for quasi-experimental policy evaluation (Abadie and L'Hour 2021). As opposed to regression-based estimators, synthetic control weights are explicitly reported after the estimation procedure. Like with matching estimators, weights are sparse, non-negative and sum to one, thus avoiding extrapolation outside the support of the data (Abadie, Diamond and Hainmueller 2010). Synthetic controls are, furthermore, more flexible than matching estimators as they allow weights to be different for each donor and do not require an arbitrarily fixed number of matches.

The main drawback of synthetic control methods with aggregated data, though, is that they can end up exploiting relatively little variation. In this case, we would only use aggregate time series by sector. Recent developments allow using synthetic controls on disaggregated data (Abadie and L'Hour 2021), increasing the total amount of information used in the model. For instance, a policy shock may well affect a single unit from a macroeconomic point of view, such as the French retrofitting industry with the EEO reforms. However, it would be preferrable to exploit variations in employment at sub-national level to reduce the risk of large, worst-case interpolation biases. Using the model by Abadie et L'Hour (2021), we can disaggregate impacts at regional level and exploit substantially more variation than with national aggregates.

[14] Besides, the method requires that the policy analyzed be of sufficient magnitude to be detectable. We believe this is likely to be the case because investment levels through the EEO scheme increased drastically, from EUR 600 million in 2017 to more than EUR 2.75 billion in 2019 after the policy change (as shown in **Figure 1**).

The challenge in a disaggregated setting lies in the management of a larger pool of potential donors to create synthetic control groups. In the application below, since we have 13 regions and 730 nationwide sectors that could serve as potential control sectors, our donor pool could include nearly 10,000 potential control sectors.

There are two problems with this. The first one is an increased risk of overfitting. With a very large pool of control sectors, one sector could provide a very close match to a treated sector at regional level. This could be because both sectors behave the same way, but also because, with such a large pool of control sectors, it is quite likely that a single untreated sector would resemble a treated sector for completely spurious reasons. Thus, when computing each synthetic control group, the statistician may want to avoid relying on a single control sector to create the synthetic control group, even when this control sector is a very good match. There is a tradeoff between using a very small number of sectors that are very good matches (what Abadie and L'Hour (2021) call the "matching case") and using a larger number of control sectors that, individually, offer less perfect matches but, as a whole, may constitute a good synthetic control (the "synthetic control case"). The *penalized* synthetic control (PSC hereafter) framework proposed by Abadie and L'Hour (2021) precisely deals with the existence of this tradeoff and on how to calibrate the model accordingly.

The second problem is a problem of computational intensity. Pairwise comparisons and inferences can take a very long time, requiring that the number of sectors is reduced. In this paper, we reduce the number of sectors as follows. Firstly, we exclude all other construction-related sectors. This is because they might have indirectly been affected by the policy, even if they were not the main recipients of the policy.[15] For instance, households could insulate their home and decide to perform other improvements at the same time, such that other professionals could indirectly benefit from the policy. In the U.S., Cohen, Glachant and Söderberg (2017) show that households tend to perform several house improvements at the same time. This has also been noted by Peñasco and Anadón (2023) in the UK, where loft or cavity walls insulation is often performed at the same time as building extensions. By

[15] There are 36 construction-related sectors. In the WMD, those have sector codes starting by 41, 42 or 43.

extension, we also exclude building and real estate services. Secondly, we narrow down the number of sectors based on their national headcount just before the start of the fourth period of the EEO scheme (2018-2021). We take as a benchmark the average employment level in December 2017 in the retrofitting industry (the combined “insulation” and “installation of heating equipment” sectors). Our baseline donor pool includes all regional sectors which national headcount is comprised within a ±33% interval around this threshold. This yields 427 different control sectors. We also perform two robustness checks for this selection rule, with a ±25% and ±50% intervals, leaving us with 336 and 659 control sectors, respectively. This method of selection of control sectors ensures that their size is close enough to the energy retrofit sectors.

We use the sectors in the donor pool to define a Penalized Synthetic Control for cumulative employment growth in the retrofitting industry within each region from January 2016 to February 2020. We calibrate the PSC over a 24 months pre-treatment period, from January 2016 to December 2017. The policy shock occurs in January 2018. The main treatment effect on the treated which we report is the difference between cumulative employment growth and its synthetic counterfactual in February 2020. Each treated regional sector is matched to a weighted average of untreated sectors. Weights are defined for each of the control sectors in the donor pool according to the minimization program detailed in **Appendix 0**. We depart from Abadie and L’Hour (2021) as we do not look for an average, but rather an aggregate effect on the treated. We are nevertheless also interested in the individual treatment effects estimated for each regional retrofitting industry, as they give us a precise estimate of the distribution of the policy’s effects on employment across French regions.

Conversely to standard linear regression models, there is no classical inference test to estimate whether the estimated treatment effect is statistically significant or not. We follow Abadie et L’Hour (2021) and define a placebo test to analyze whether the difference between the control and treatment groups can be attributed to the policy. The placebo test consists of the creation of a PSC for 100 sets of regional sectors, randomly selected from the donor pool. In theory, since none of the control sectors were affected by the policy, there should be no tangible difference in cumulative employment growth before and after January 2018 between the control sectors and their synthetic controls. If the placebo test shows that the gap estimated for the energy retrofit sectors is sensibly larger than the post-reform placebo differences in employment obtained with the sectors from the donor pool, then we can infer that the reform had a noticeable impact on employment in energy retrofit industries. Otherwise, results should be considered as not statistically different from zero. We detail this inferential framework and the implementation of the permutation tests in **Appendix B**.

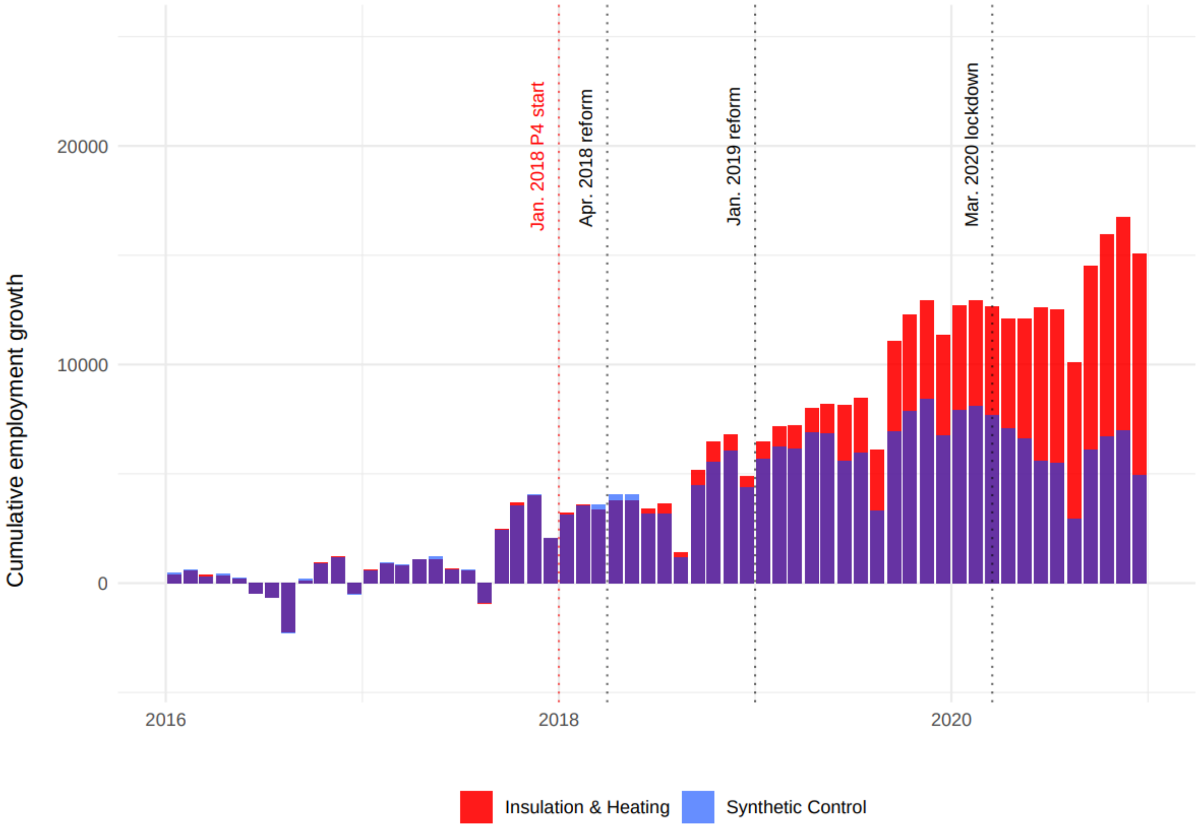
5. Results

5.1. Effect on total employment

Our main results are provided in **Figure 4**, where we have aggregated the 13 regional estimates at national level. The calibration of the synthetic control model is done on all months before the start of the fourth implementation period of the EEOs in January 2018. Before that month, the evolution of the workforce in the energy retrofit sectors is, by construction, very similar to the evolution in the synthetic control group. Policy effects are then obtained by comparing post-treatment trends. Taken together, we find that the reforms led to an increase in employment equivalent to the creation of about 4,800 additional jobs by February 2020 (before the first lockdown in France caused by the COVID-19 pandemic). Over the same period, cumulative employment growth increased by 10,889 jobs in the retrofit sectors (as presented in **Table A1** in **Appendix A**). Thus, our estimates attribute around 44% of the rise in sectoral employment between January 2018 and February 2020 to the policy reforms.

In **Figure 4**, most of the effect of the reforms on employment are recorded after the second reform in January 2019. During the period that follows the first reform (April 2018 to December 2018), we observe barely any effect on cumulative employment growth, suggesting that the first policy changes did not have the strongest impacts on employment. This is consistent with the fact that most of the policy changes were introduced with the second reform: from April 2018 to December 2018, the average monthly value of all the works performed under the EEO scheme was of EUR 78 million, whereas it was of EUR 220 million, almost three times higher, between January 2019 and February 2020.

Figure 4: Employment growth in energy renovation vs its penalized synthetic control

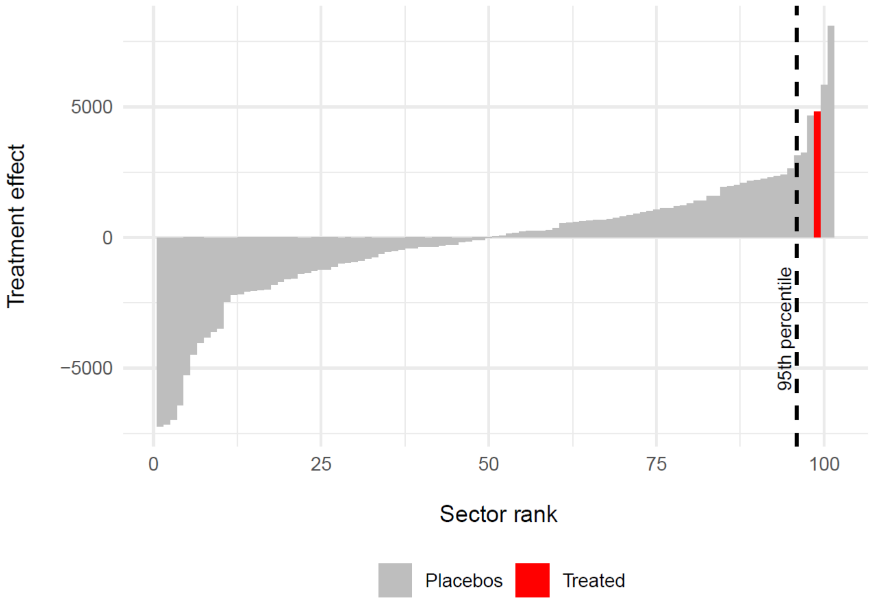


Notes: When the "synthetic control" (blue) and "insulation and heating" (red) sectors overlap, the color displayed on the graph becomes purple. The pre-treatment period includes all months from Jan. 2016 to Dec. 2017. The treatment period follows, from Jan. 2018 onwards. $\lambda^* = 0$ (no direct matches to penalize). The red bars represent the evolution of the workforce in the energy renovation sector, the blue ones display the evolution for its synthetic control group. p-value for the one-sided test is 0.03.

There is a complementary explanation for the small effect of the first reform. Our model assesses employment based on the number of days that workers were under contract, independently of how many hours they did. When workers that are already employed are doing overtime work, this is not captured in **Figure 4**. Hence, for small increases in the demand for energy efficiency services, part of the activity surplus could have been borne by workers already employed in the industry and this would therefore not be observable in **Figure 4**.

We follow the inferential framework (discussed in **Section 4**) to ensure that the results of **Figure 4** can be attributed to the policy. **Figure 5** displays the results of the permutation tests for our baseline model. The treatment effect ranks third highest against 100 alternative random permutations (each represented by a gray bar and ordered according to the estimated treatment effect). In **Figure 5**, the effect for the truly treated sector is among the top 5% of all sectors. The corresponding p-value for the one-sided test is 0.03, hence we can confidently interpret the additional 4,800 jobs as stemming from the effect of the policy changes.

Figure 5: Permutation test for the effect of the policy



Notes: Results are obtained for 100 permutations, using the optimal value $\lambda^* = 0$. Vertical bars correspond to the aggregate treatment effect for any placebo sector (in grey) and the retrofitting industry (in red). The dotted line represents the 95th percentile. The p-value for the one-sided test is 0.0297.

Several robustness checks confirm our findings. In **Appendix C.1**, we move the assumed starting time of the policy shift to either April 2018 or January 2019, when the first and second reforms are implemented. This is considering that most of the changes in investment levels do not occur in January 2018, but later. The overall effect when the start is set in April 2018 is very similar with about 4,700 additional workers (p-value of 0.04). Consistently, starting the treatment period in January 2019 has barely any effect on our estimates, with an estimated increase by about 4,500 additional workers (p-value of 0.04). This last result echoes the above discussion on the relative magnitude of the two implementation reforms, and the fact that most of the employment effect occurred in 2019.

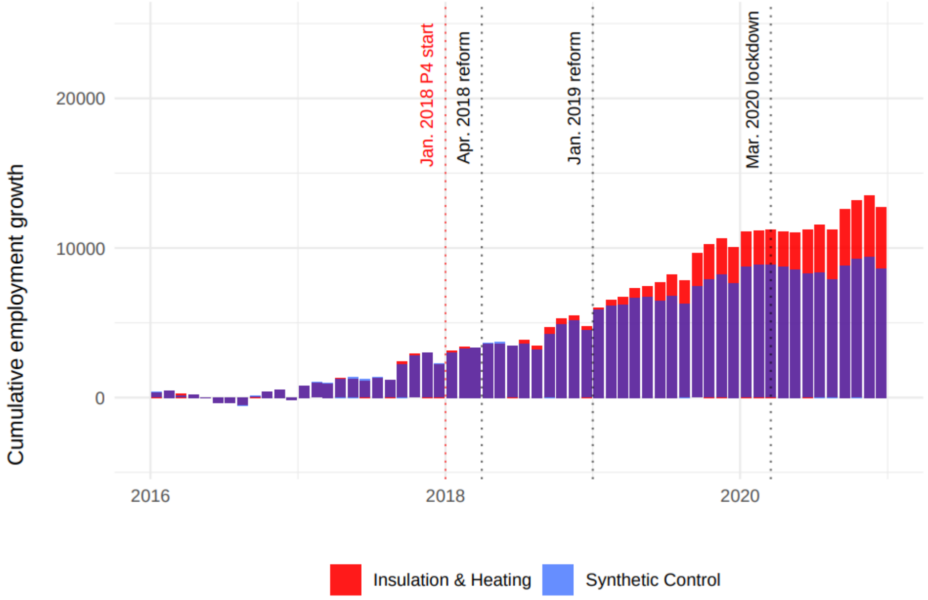
In contrast, considering that the change of implementation period in January 2018 could have been anticipated by regulated companies, we run an anticipation test with a treatment date starting in October 2017, one trimester before the end of the fourth period (see **Appendix C.2**). In this setting, we find that cumulative employment growth may have started to diverge before January 2018 since energy providers had to rush to comply with their obligation before the end of the year. However, this effect is small in magnitude (below 1,300 jobs). Effects after January 2018 remain clearly identified and comparable to our baseline figures with about 6,100 additional workers (p-value of 0.03).

Finally, we perform our econometric estimation (with the baseline date of January 2018 for the start of the treatment) using two alternative donor pools for the control sectors (See **Appendix C.3**). They include all regional sectors which national headcount is comprised within a ±25% (±50%, respectively) interval around the national headcount of the retrofitting industry in December 2017 (±33%, respectively). Using the narrower interval (336 donors), we estimate about 4,650 additional workers (p-value of 0.04). It goes up to 5,200 additional workers (p-value of 0.02) when using the wider interval (659 donors). Thus, our results are robust to the selection of sectors included in the donor pool.

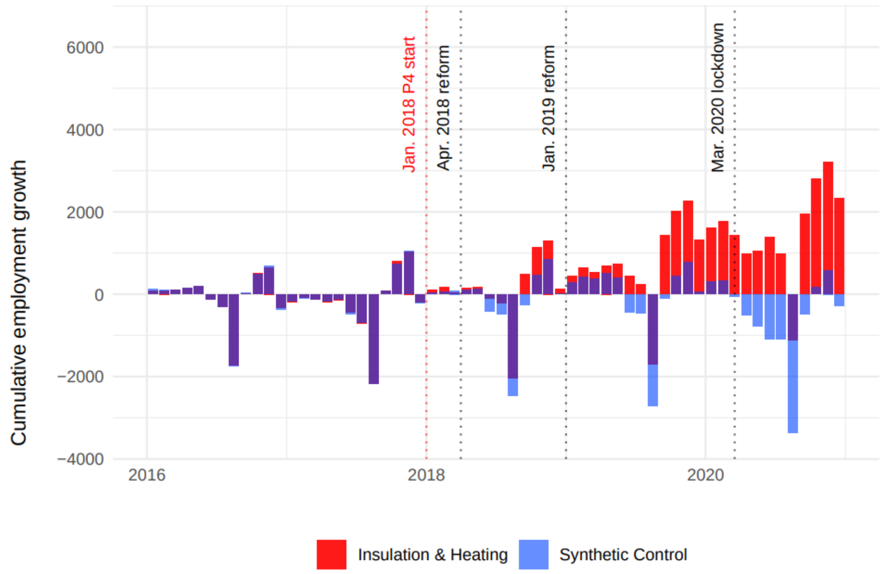
5.2. Effect by contract type (permanent vs. fixed term)

We look at the impact of the policy on permanent contracts vs. fixed-term contracts to assess whether the policy reform led to long-term job creation. In France, social protection laws imply that employers are often very reluctant to offer permanent positions because firing people can be very costly (Article R1234-2, French Labor Code). On average in 2023, severance was equal to 6.6 months of salary (Dalmasso et Signoretto 2023). For short term increases in activity, employers can use fixed-term contracts with a maximal duration of 18 months (in the general case, some exceptions allow for 24 months). French employers only offer permanent contracts when they think that the activity will be sustained for several years.

Figure 6: Employment growth in energy renovation vs its penalized synthetic control by contract type



(a) Permanent contracts



(b) Fixed term contracts

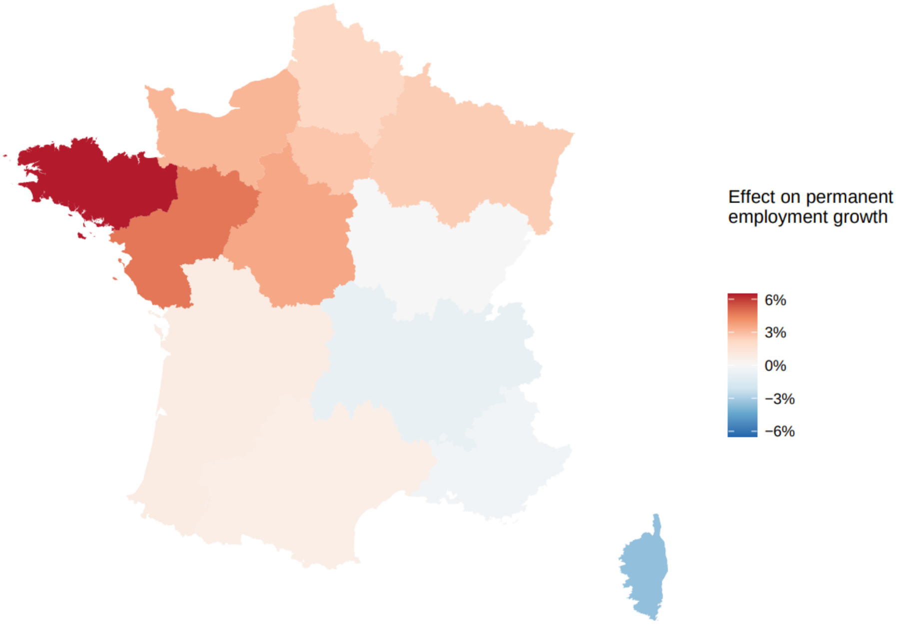
Notes: The donor pool is based on the ±33% interval around the national headcount of the retrofitting industry in Dec. 2017. The pre-treatment period includes all months from Jan. 2016 to Dec. 2017. The treatment period follows, from Jan. 2018 onwards. The penalization parameter is optimized at $\lambda^* = 0.00001$ for both panel (a) and (b). The red bars represent the evolution of the workforce in the energy renovation sector, the blue ones display the evolution for its synthetic control group. When they overlap, the color displayed on the graph becomes purple. In panel (b), contract terminations seem to follow a seasonal pattern, with terminations being more frequent in July-August as well as in December. p-value for the one-sided test is 0.02 (a) and 0.07 (b).

In **Figure 6**, we report the aggregate results after running the synthetic control model by contract type (see panel (a) for permanent contracts, and (b) for temporary ones). We find that about 2,300 of the jobs created were permanent, amounting to around 50% of the estimated effect of the EEO reforms on total employment (p-value of inference test is 0.02). For fixed-term contracts, we find that nearly 1,400 jobs were created. However, the inference tests suggest that the effect is only weakly significant. The estimated effect ranks 7th out of 101 permutations, corresponding to a p-value of 0.07.

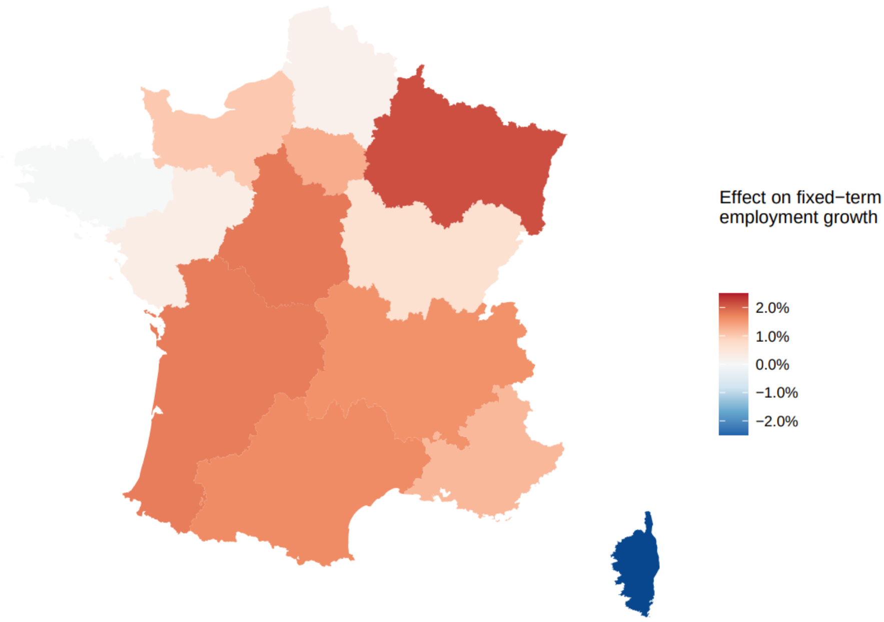
5.3. Regional estimates

An innovation of the synthetic control method used in this paper is to allow estimating separate effects at regional level. **Figure 7** provides our regional estimates for permanent contracts (a) and fixed-term contracts (b). For permanent contracts, estimates indicate that Western France particularly benefitted from the rise in investment through the EEOs. Indeed, permanent job creations induced by the policy exceed 3% of the 2017 employment level in the energy retrofit sectors. This share is as high as 6.5% in Britany. On the contrary, policy-induced permanent positions in Southern regions represent at most 1% of the industry's total employment at the start of the fourth period of the EEOs. The estimated effect is even negative for South-eastern regions as in Auvergne-Rhône-Alpes (-0.8%) or Corsica (-3.5%). Findings are interesting since they suggest that most of the more permanent employment effect was registered in the coldest regions of France. Conversely, the policy seemed to have led to a surge in temporary employment in the South of France.

Figure 7: Effect of the policy on sectoral permanent and fixed term contract growth (% of Jan. 2016 total employment)



(a) Permanent contracts



(b) Fixed term contracts

Notes: Baseline estimation ($\lambda^* = 0$, p-value 0.03). Percent changes are reported relatively to the average employment level in the retrofitting industry in January 2016.

6. Discussion

If we compare our main job-creation estimate with total spending in the EEO scheme, we obtain that the policy sustained 1.4 direct jobs for each additional million euro invested.[16] This estimate is well below existing ex-ante estimates. BPIE (2020) provides a literature review of 35 ex-ante studies on the impact of energy renovation on job creation in Europe. The review finds that energy renovation could be responsible for creating 13 to 28 direct and indirect jobs per million euros invested, of which one third would be direct jobs (this is 4.29–9.24). Relying on Janssen and Staniaszek (2012) and Cuq et al. (2011), the European Commission has used the value of 8.52 Full-Time-Equivalent (FTE) jobs per million euros invested as the reference job multiplier for energy renovation (European Commission 2019). Our results imply that this figure may largely overestimate job creation and should be revised downwards.

Revising estimates seem all the more necessary that the other two major ex-post studies seem to align with ours, with Popp et al. (2021)'s estimate being of 2 to 4 jobs per million dollars in the American construction sector, and Fabra et al. (2023)'s estimate of 0.65 jobs per million euros in the Spanish solar industry. Outside of the EU, the same problem of an overestimation of the number of jobs created may arise and mislead cost-benefit analyses of investment programs. For the U.S., (Garrett-Peltier 2017) uses an input-output model and finds that 4.55 direct jobs were created for each million dollars invested in energy retrofits.

If compared with the estimates in Fabra et al. (2023), ours suggest that energy retrofits may have a slightly higher impact on job creation than solar energy, and a higher impact than wind energy. Fabra et al. (2023) only consider direct local job creation at a sub-regional scale, so their scope is narrower than ours. However, if we consider that most energy retrofits are

[16] According to official records made available by the French Ministry of Ecological Transition, the monthly average market value of certified energy efficiency works was EUR 34 million between January 2016 and December 2017. After the first reform, investment steadily increased, reaching an average monthly market value of EUR 164 million. This is equivalent to a monthly increase by EUR 130 million on average. According to our estimations, the additional monthly employment growth caused by the policy was +185 workers a month. It follows that the policy led to an estimated increase in employment by 1.4 workers for each million euros invested in the EEO scheme: 185 / 130 = 1.4.

performed by SMEs locally, then our estimates and those of Fabra et al. (2023) can be closely compared. Our results also align with recent insights published in a note by the French Economic Analysis Council (CAE 2023) looking at the effect of the energy transition on employment. Authors acknowledge the existence of a clear development potential for retrofitting activities but believe it might only have had a limited effect on total employment.

With an increase in the amounts provided, the EEO reforms may have modified employer expectations. In particular, it may have reduced policy uncertainty, and/or suggested that the French government was going to invest in energy retrofits very durably, beyond the scope of the fourth phase (2018-2021) or fifth phase (2022-2025) of the EEO scheme. Changes in expectations and reductions in regulatory uncertainty could have contributed to the estimated job creations, especially to the effect on open-ended contracts. The impact on employment of changes in risk levels is studied by (Schaal 2017), who finds that changes in risk levels affect fluctuations in aggregate unemployment in the case of the U.S.

Our spatial heterogeneity analysis suggests that the geographical distribution of job creations may not be homogeneous. The number of creations could be proportionally higher in colder, richer and/or more populous regions. More research is however needed to understand the special distribution of the employment effects of a policy like the French EEOs.

Finally, the French EEO scheme has some features that make it particularly interesting to study. However, impacts may not be fully transferrable to other investment policies because of the specificities of this market-based instrument. Especially, the cost of the policy is put on energy providers, who are required to provide subsidies to households and businesses. There are very few government expenditures to support the scheme, and the policy is much more socially acceptable than a carbon tax. However, the financial burden of the EEOs is likely to have been passed on to domestic consumers through increases in residential energy prices. Darmais, Glachant, and Kahn (2022) estimated that a 4-percent increase in residential energy prices would be necessary to cover the cost of the EEO scheme. Therefore, the effect of the French EEOs on investments may not exclusively come from the subsidies, but also from the concomitant increase in energy costs for households, who may decide to invest in energy efficiency because of the increase in energy prices.[17] However, the evidence on the responsiveness of consumers to energy prices for energy-using products and home improvements is mixed. Long-term energy costs may be underestimated by consumers, even though energy price increases could still trigger improvements in energy efficiency (Cohen, Glachant and Söderberg 2017, Schwarz, et al. 2021, Houde and Myers 2021, Kiso, Chan and Arino 2022).

7. Conclusion

We exploit a discontinuity in the French EEOs to estimate the impact on employment of one of the largest energy retrofit policies in Europe. Our penalized synthetic control method detects a significant, but limited increase in employment in the energy renovation sector, with about 1.4 jobs sustained per million euros invested in the EEOs.

To the best of our knowledge, this study constitutes the first ex-post analysis of the employment co-benefit of energy retrofits. We find that job creations were substantially lower than those estimated in ex-ante studies, and well below reference values used in the European Union. Our estimates align with other ex-post studies for other investment categories and countries (Popp, et al. 2021, Fabra, et al. 2023), suggesting that the job creation estimates used in cost-benefit and impact assessments should be revised downwards. Our analysis however confirms the existence of social co-benefits with energy retrofit policies, possibly of a higher magnitude than other green investments in the energy transition.

We hope that further iterations of this working paper may allow us to explore a few additional unanswered questions. We plan to investigate the effect of the policy on the number of interim workers within the industry. We also wonder if the initial size of businesses may have played a role in their ability to respond to the increased demand as well as to the administrative

[17] In that regard, energy price increases due to the EEOs should only have concerned households and low-consuming businesses in the tertiary sector. This is because industrial energy consumption is exempted from the policy: obligations do not depend on the amount of energy sold in the industrial sector, and some providers, who exclusively sell energy to the industrial sector, were not covered by the EEO scheme. This was done to ensure that the EEOs would not lead to a contraction of economic activity in other sectors. We can therefore presume that there was no job loss in industrial sectors because of the introduction of the policy.

burden of the EEOs. Because filling EEO-related paperwork can be time-consuming, it is possible that only the largest businesses with the appropriate administrative staff benefited from the policy. This would have implications for the composition of the industry. While beyond the scope of this study, an analysis of the pass-through of the policy to residential energy prices could also be especially relevant.

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Online appendices

A. Summary statistics of hires in the retrofitting industry

Table A1 below shows that permanent contracts accounted for most new hires within the retrofitting industry after 2016. In contrast, employment growth in other sectors mostly stemmed from a growth in the number of fixed-term contracts.

Table A1: Monthly cumulative employment growth by contract type (from Jan. 2016 onwards)

	Permanent	Fixed-term
Retrofitting industry		
Jan. 2016	+337	+74
Dec. 2017	+2,248	-201
Feb. 2020	+11,168	+1,768
Other sectors		
Jan. 2016	+50,067	+69,867
Dec. 2017	+20,183	+124,214
Feb. 2020	+502,536	+188,046

B. Penalized Synthetic Control

Estimator

To establish our own PSC estimator, we rely on cumulative employment growth in n different sectors observed at regional level from January 2016 to February 2020. We observe n_1 treated sectors (one in each region, bundling together those of "insulation works" and the "installation of heating equipment") and n_0 control sectors representing our pool of donors, with n representing the total number of treated and control sectors. In our application, $n_1 = 13$ and $n_0 = 427$.

Y_i denotes the realized outcome, i.e., cumulative employment growth since January 2016 and until February 2020, i.e. before the start of the French lockdown caused by COVID-19. Following Rubin (1974)'s potential outcomes framework, Y_{1i} and Y_{0i} respectively refer to the potential outcomes under treatment ($D_i = 1$) and under no treatment ($D_i = 0$).

We rely on pre-treatment cumulative employment growth to estimate Y_{0i} for the treated sectors. We define X_i for $i \in n_0$ as the $1 \times T_0$ vector of pre-treatment predictors of Y_{0i}, where $T_0 = 24$ is the duration, in month, of our pre-treatment period from January 2016 to December 2017. Each column in X_i therefore gives cumulative employment growth in month $t = 1, \dots, T_0$. We thus have 24 predictors of Y_{0i} corresponding to cumulative employment growth since January 2016 and until each month of the pre-treatment period. We then set the policy shock to occur in January 2018, and observe our outcome in February 2020.

The data is pooled into a single dataset $\{(Y_i, D_i, X_i)\}_{i=1}^{n}$. We sort the data such that the n_1 treated sectors come first. The treatment effect on the treated $\tau_i = Y_{1i} - Y_{0i}$ (for $i = 1, \dots, n_1$) is estimated using a synthetic counterfactual Y_{0i}.

Each treated regional sector is matched to a weighted average of untreated sectors, where the n_0-vector of weights $W_i^*(\lambda) = (W_{i,n_1+1}^*, \dots, W_{i,n}^*)$ is solving:

$$\underset{W_i \in \mathbb{R}^{n_0}}{\text{Min}} ||X_i - \sum_{j=n_1+1}^{n} W_{i,j} X_j||^2 + \lambda \sum_{j=n_1+1}^{n} W_{i,j} ||X_i - X_j||^2 \qquad (1)$$

$$\text{s.t. } W_{i,n_1+1} \geq 0, \dots, W_{i,n} \geq 0,$$

$$\sum_{j=n_1+1}^{n} W_{i,j} = 1$$

$W_{i,j}^*$ is the j^{th} element of $W_i^*(\lambda)$. It is weighting control sector j in the synthetic control sector attached to the treated sector i.

Compared to a standard synthetic control model, Eq. (1) above includes two parts, which are weighted according to a tuning parameter λ. The first part minimizes the difference between the pre-sample cumulative employment growth in sector i (X_i), and a weighted sum of cumulative employment growth in the pool of control sectors ($\sum_{j=n_1+1}^{n} W_{i,j} X_j$). This is the standard minimization synthetic control programme. In addition, the equation also minimizes the weighted difference in cumulative employment growth between all control and treatment sectors separately ($W_{i,j}||X_i - X_j||^2$ for every $j \geq n_1 + 1$). The tuning parameter λ weights both minimizing functions, and the optimal set of weights $W_i^*(\lambda)$ is a function of λ. When weighting potential donors, the PSC estimator does not only rely on minimizing the difference between the treatment and the synthetic control in the pre-sample period. It also favors untreated sectors j that are individually closer to the treated one i, hence minimizing interpolation biases. The inverse interpretation is also true. If λ is small, then the programme focuses on the difference between the synthetic control and the treated control rather than its components.

The definition of the optimal λ^* relies on a data-driven process. We follow the protocol of Abadie and L'Hour (2021), which they called the "leave-one-out cross-validation of post-intervention outcomes for the untreated".

First, we select a set of k "placebo-treated" control sectors, with $k < n_0$. Those "placebo-tested" sectors comprise the four nearest neighbors of each treated sector within the donor pool. We therefore look at control sectors that are close to the treated ones.

Second, we compute the treatment effect $\hat{\tau}_i(\lambda)$ as the difference between Y_i and the prediction of Y_i obtained from a synthetic control with optimal weight vector $W_{i,j}^*(\lambda)$, computed with all other control sectors j in the donor pool $\{n_1 + 1, \dots, n\}\backslash\{i\}$:

$$\hat{\tau}_i(\lambda) = Y_i - \sum_{\substack{j=n_1+1 \\ j \neq i}}^{n} W_{i,j}^*(\lambda) Y_j$$

In theory, $\hat{\tau}_i(\lambda)$ should be close to zero because we have used placebo sectors, hence there should be no treatment effect. We choose the optimal λ to minimize the root mean squared prediction error across all "placebo-treated" sectors, such that:

$$\lambda^* = \min_{\lambda} \left(\sqrt{\frac{1}{k} \sum_{i=1}^{k} [\hat{\tau}_i(\lambda)]^2} \right)$$

Since this minimization programme is computationally intensive, we select λ^* within a list of discrete values. Following Abadie and L'Hour (2021), our list includes $\lambda = 0.00001$; 0.01; 0.1; 0.15; and all increments of 0.1 up to 4.95.

Inference test

For inference, we follow the procedure for "inference on aggregate effects" of Abadie and L'Hour (2021). The framework compares the treatment effect in the treated sectors with a hundred placebo effects, estimated for a hundred sectors that have been randomly selected within the pool of control sectors. Those placebo effects are calculated using the PSC estimator described above. In theory, these placebo effects should be null since the control sectors should not have been affected by the policy. Therefore, we will reject the null hypothesis at 5% if the treatment effect that is being recorded for the treated sectors is higher than the 95th percentile of all the effects estimated with the placebos.

Let's denote $\boldsymbol{D^{obs}}$ the actual vector of treated sectors. The process starts by estimating the average treatment effect for all those sectors, with the optimal penalization parameter λ^*. We denote this average $\hat{\tau}_i(\boldsymbol{D^{obs}},\ \lambda^*)$, such that:

$$\hat{T}\left(\boldsymbol{D^{obs}}, \lambda^*\right) = \frac{1}{n_1}\sum_{i=1}^{n_1} \hat{\tau}_i(\boldsymbol{D^{obs}}, \lambda^*) \tag{4}$$

We then randomly select a subset of a hundred control sectors within n_0, and for each of those sectors, which we denote b, we calculate a placebo treatment effect $\hat{T}\left(\boldsymbol{D^{(b)}}, \lambda^*\right)$ such that:

$$\hat{T}\left(\boldsymbol{D^{(b)}}, \lambda^*\right) = \frac{1}{n_1}\sum_{i=1}^{n_1} \hat{\tau}_i(\boldsymbol{D^{(b)}}, \lambda^*) \tag{5}$$

We then rank all placebo effects and look at the rank of the treatment effect to compute a p-value. The p-value for this one-sided test writes as follows:

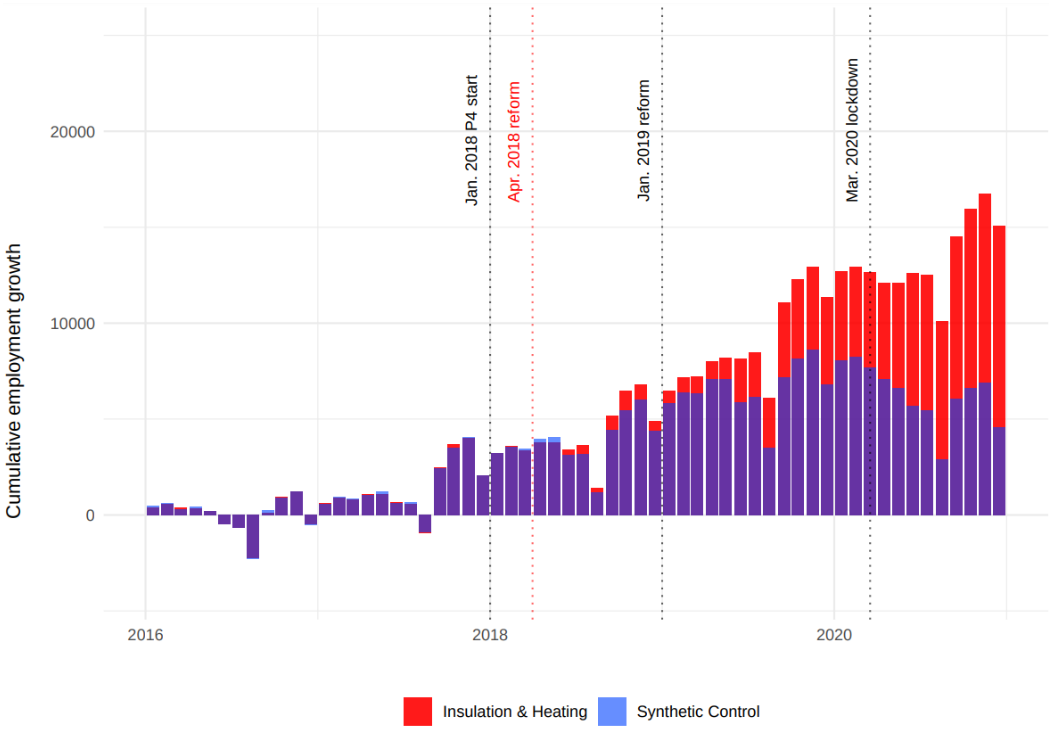
$$\hat{p} = \frac{1}{100+1}\left(1 + \sum_{b=1}^{B=100} \mathbf{1}\left\{\hat{T}\left(\boldsymbol{D^{(b)}}, \lambda^*\right) \geq \hat{T}\left(\boldsymbol{D^{obs}}, \lambda^*\right)\right\}\right) \tag{6}$$

C. Sensitiveness analysis

C.1. Later starting dates

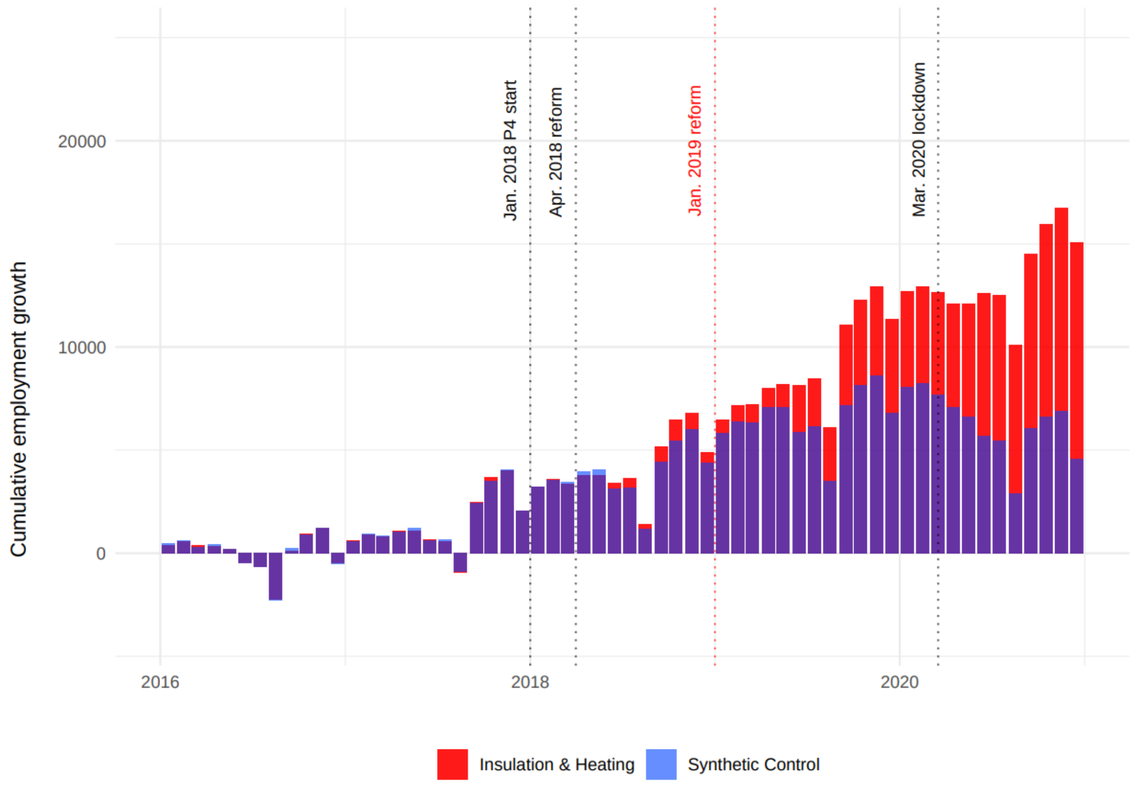
Below, we use April 2018 (**Figure 8**) and January 2019 (**Figure 9**) as the starting dates of the policy, assuming no policy effect on employment before. Those dates were chosen to match the reforms that occurred after the fourth phase of the scheme came into force. With a starting date in April 2018, results are very similar to our baseline estimation, with 4,700 additional workers (p-value of 0.04). Likewise, restricting the treatment period to the months after January 2019 yields similar results again (about 4,500 additional workers, p-value of 0.04).

Figure 8: Trend in employment growth in the energy renovation vs its synthetic control, assuming a policy start in April 2018



Notes: The pre-treatment period includes all months from January 2016 to March 2018. The treatment period follows, from April 2018 onwards. $\lambda^* = 0$ (no direct matches to penalize). The red bars represent the evolution of the workforce in the energy renovation sector, the blue ones display the evolution for its synthetic control group. When the "synthetic control" (blue) and "insulation and heating" (red) sectors overlap, the color displayed on the graph becomes purple. The p-value for the one-sided test is 0.04.

Figure 9: Trend in employment growth in the energy renovation vs its synthetic control, assuming a policy start in January 2019

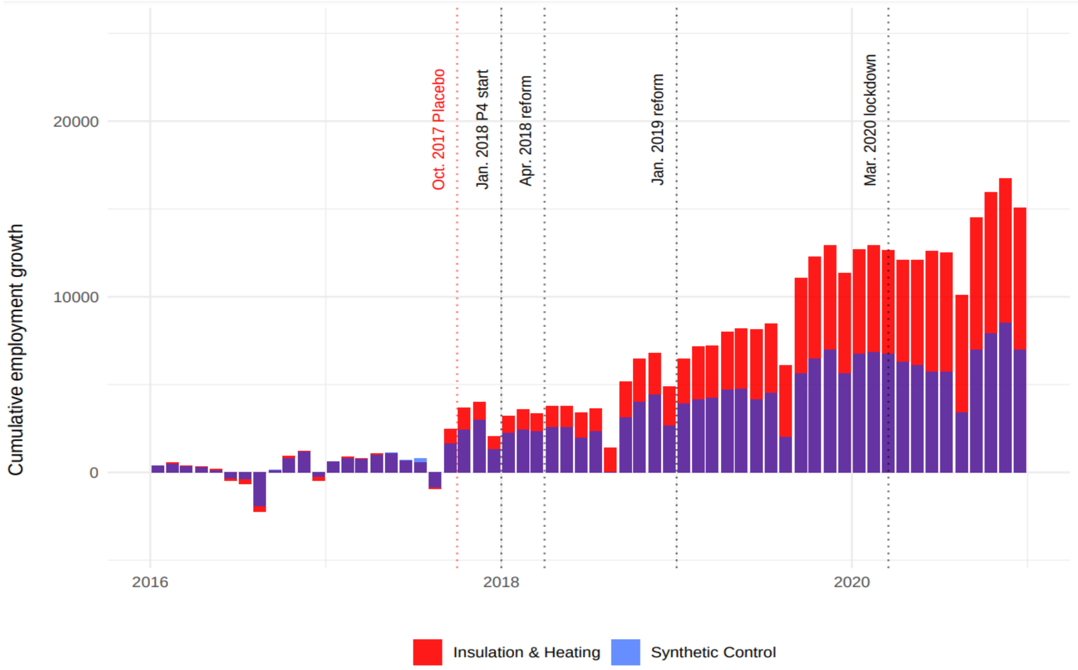


Notes: The pre-treatment period includes all months from Jan. 2016 to Dec. 2018. The treatment period follows, from Jan. 2019 onwards. $\lambda^{*} = 0.00005$. The red bars represent the evolution of the workforce in the energy renovation sector, the blue ones display the evolution for its synthetic control group. When the "synthetic control" (blue) and "insulation and heating" (red) sectors overlap, the color displayed on the graph becomes purple. The p-value for the one-sided test is 0.04.

C.2. Earlier starting date (anticipation test)

For the synthetic control method to be valid, there should be no anticipatory effect of the policy. Abadie (2015) proposes a placebo test to check this. It consists in running the same analysis, but as if the policy reform had occurred a bit earlier. If an effect can be observed during the placebo period, then the non-anticipation condition does not hold. We perform this anticipation test by assuming that the fourth period of the EEO scheme started one trimester earlier, in October 2017. As suggested in **Figure 3**, employment dynamics may have started to diverge slightly before January 2018 since energy providers had to rush in order to comply with their obligation under the third phase, closing in December 2017. In **Figure 10**, we observe an effect on employment during the last trimester of 2017, resembling an anticipation of the policy. However, since this effect is small (about 1,000 jobs) and may stem from the end of the third phase rather than the changes introduced during the fourth phase, we kept January 2018 as our baseline starting date.

Figure 10: Trend in employment growth in the energy renovation vs its synthetic control, assuming a policy start in October 2017



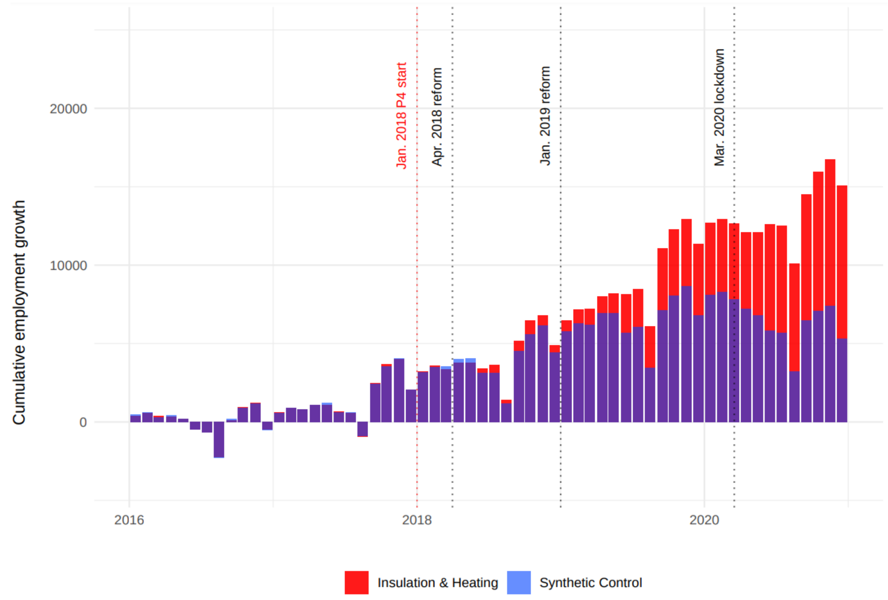
Notes: The pre-treatment period includes all months from Jan. 2016 to Sep. 2017. The treatment period follows, from Oct. 2017 onwards. $\lambda^* = 0$. The red bars represent the evolution of the workforce in the energy renovation sector, the blue ones display the evolution for its synthetic control group. When the "synthetic control" (blue) and "insulation and heating" (red) sectors overlap, the color displayed on the graph becomes purple. The p-value for the one-sided test is 0.03.

C.3. Alternative pools of control sectors

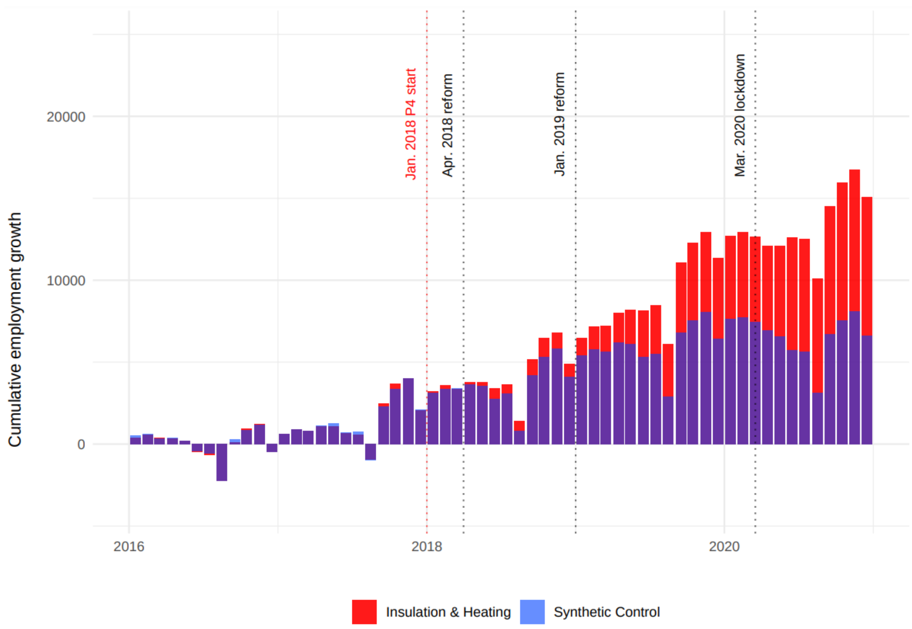
In our baseline estimation, we narrowed down the number of sectors based on their national headcount just before the start of the fourth period of the EEO scheme (2018-2021). We took as a benchmark the average employment level in December 2017 in the retrofitting industry (the combined "insulation" and "installation of heating equipment" sectors).

Our baseline donor pool included all regional sectors which national headcount was comprised within a ±33% interval around this threshold. This yielded 427 different control sectors. Below, we perform two robustness checks for this selection rule, with a ±25% and ±50% intervals, leaving us with 336 and 659 control sectors, respectively.

Figure 11: Estimation with different intervals to restrict the pool of control sectors



(a) ±25% interval



(b) ±50% interval

Notes: Estimation is similar to the baseline, except for the rule used to restrict the pool of control sectors.

The narrower interval (336 donors) yields an estimated effect of about 4,650 additional workers (p-value of 0.04). Using the wider interval (659 donors), the estimated effect is 5,200 additional workers (p-value of 0.02). Thus, our results are also robust to the selection of sectors included in the donor pool.

2020

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Institut d'Economia de Barcelona

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ieb@ub.edu
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