# Making Jobs out of the Energy Transition: Evidence from the French Energy Efficiency Obligations Scheme

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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 1.5 jobs per million euros invested, mostly in a permanent position within micro enterprises. Hourly wages remained unaffected, suggesting that additional investments are captured by higher margins.

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# 1. Introduction

Public discourse frequently assumes that the energy transition will lead to significant job losses, contributing to the widespread reluctance to implement stringent carbon taxes in many countries[2]. Despite the scale of global investment in green initiatives, amounting to more than a trillion dollars in the post-Covid19 recovery packages[3], there remains limited evidence on their potential to create jobs. This gap in understanding is particularly relevant to the construction sector, which stands to gain substantially from green investments. In France, for example, over 70% of green investments should be directed toward energy retrofits, highlighting their potential to drive employment in this critical sector[4].

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[5]. We use a novel synthetic control method to look at the impact of an unprecedented and unexpected increase in energy efficiency investment subsidies granted to households in 2018 and 2019. We find that each million euro invested directly created 1.5 job-year, way below ex-ante forecasts and policy targets. Most policy-induced jobs are permanent, signaling sector consolidation, with micro-enterprises of less than 10 workers benefiting despite the administrative burden of the policy. Conversely, wages remain flat over the period of observation. This contradicts the assumption of a labor supply shortage and suggests that at

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[2] See for instance: The Guardian view on a carbon-free economy: no just transition in sight – yet (2025, January 3). *The Guardian*; Millard, R., & Rose, R. (2024, September 12). Global jobs market shaken by green transition. *Financial Times*; Ngo M. (2023, July 12). The Energy Transition Is Underway. Fossil Fuel Workers Could Be Left Behind. *The New York Time*.

[3] The OECD documents USD 1.29 trillion of public spending across 51 countries to support the development of low-carbon technologies (Aulie, et al. 2023). This includes NextGenerationEU (European Commission 2020) or the American Rescue Plan Act (Office of the Federal Register, National Archives and Records Administration 2021).

[4] Overall, 72% of all investments needed to meet the 2030 French decarbonation targets should be devoted to buildings energy renovation (Pisani-Ferry et Mahfouz 2023). This is in line with the European Commission's Renovation Wave objective of 35 million buildings renovated by 2035 (European Commission 2021).

[5] The French Energy Efficiency Obligations scheme is among the largest energy efficiency policies in Europe with over 6 billion euros invested annually (Broc, Stańczyk and Reidlinger 2020).

least part of the additional investments have been captured by higher margins.

Our findings hold important policy implications. Indeed, energy retrofits are often performed by Micro enterprises or SMEs (European Commission 2019) and involve manual workers who are most likely to be negatively impacted by other environmental policies. The political acceptability of the energy transition has been widely discussed, and a flourishing strand of literature fuels the job-killing vs. green jobs debate (Walker 2011, Hafstead and Williams 2018, Vona 2019, Hafstead and Williams 2020, Marin and Vona 2021, Haywood, Janser and Koch 2024, Rud, et al. 2024). More specifically, the energy transition appears to be biased against low-skilled workers, with low-carbon jobs having higher skill requirements across a broad range of activities, especially technical ones (Vona, Marin, et al. 2018, Yip 2018, Marin and Vona 2019, Saussay, et al. 2022, Curtis, O'Kane et Park 2024).

The contributions of this paper are twofold. First, to the best of our knowledge, this is the first causal estimation of employment impacts of home retrofits based on actual microdata. The ex-post evidence on green job creation from investment programs 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 limitation 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, two recent papers have focused on the labor market outcomes of the deployment of renewable energy generation. Focusing on job creations in Spain (Fabra, et al. 2024), find radically different impacts between solar versus wind energy investments, with the job content of solar energy being much higher than for wind. Similar results for solar have been found in Brazil by Scheifele et Popp (2024), with some positive effects appearing in the long run for wind. However, none of these papers address the specific case of energy conservation despite its central role in the decarbonation of our economies. The literature so far used ex-ante forecasting methods, such as input-output models (Mikulić, Rašić Bakarić and Slijepčević 2016, Markandya, et al. 2016, Dell'Anna 2021) or computed general equilibrium models

(Wei, Patadia and Kammen 2010, Sooriyaarachchi, et al. 2015)[6]. Accordingly, the multiplier effect used by the European Commission for energy retrofits has been set at 8.52 jobs per million euros, which has fueled ambitious policy targets[7]. On the other hand, we estimate a direct effect of 1.5 job-year in between that of Popp et al. (2021) and Fabra et al. (2024). Although the scope and context of these studies differ from ours[8], these ex-post estimates suggest that the job-creation multiplier of retrofits is in the range of other green occupations. Thus, ambitious expectations of job creation through retrofits should be updated downwards.

Second, we 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. The discontinuity is due to a set of policy changes between January 2018 and January 2019 (see **Figure 1**). We estimate that the monthly grant provided to households for insulation and heating retrofit operations through the scheme went from around EUR 60 million before January 2018 to more than EUR 200 million after this date, a 3-fold increase. This discontinuity reduces identification issues when looking at the impact of the policy on employment levels. We 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. We discuss our methods and hypotheses in detail, including robustness checks to ensure that our findings are not driven by potential biases or

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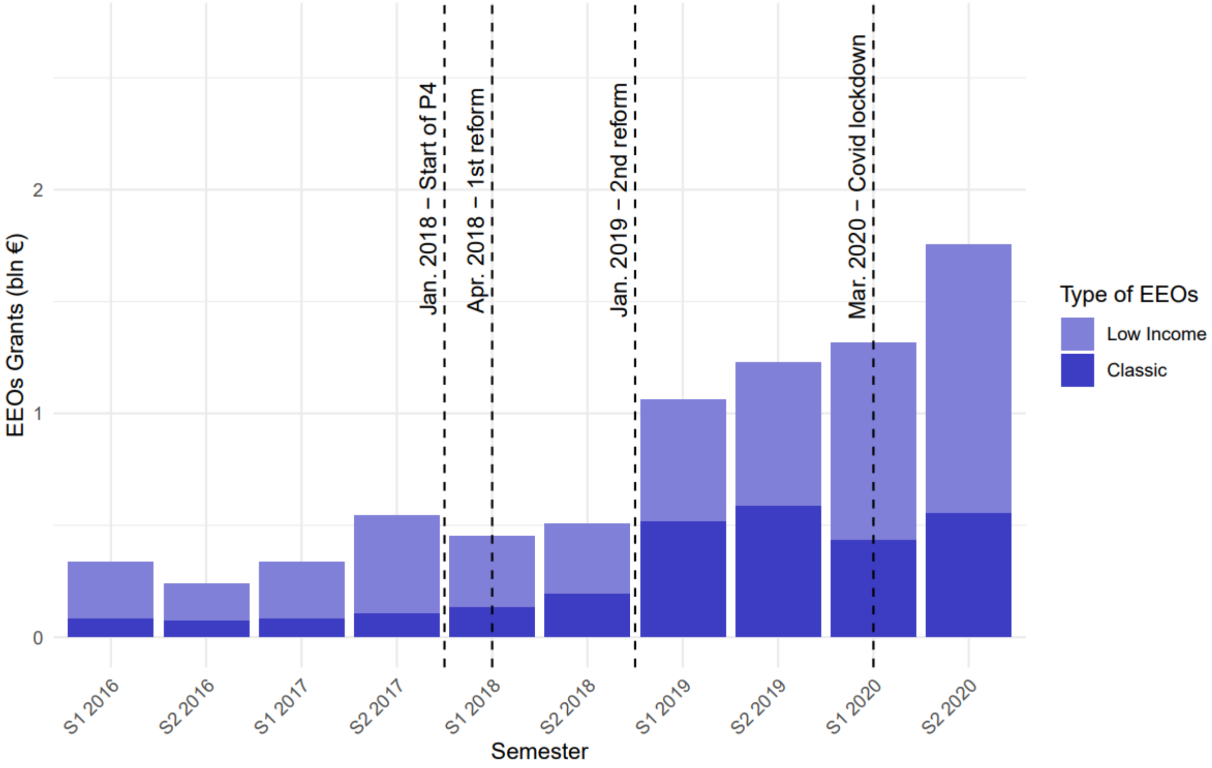
[6] According to a review by the Building Performance Institute Europe (BPIE 2020), ex-ante forecasts range from +12 to +29 direct and indirect jobs created per million dollars invested in energy retrofits, of which about one third would be direct hires in the energy retrofit sector.

[7] The achievement of the EU's 2030 climate and energy targets is expected to create almost one million new green jobs (European Commission 2020).

[8] We focus on direct jobs at national scale while Fabra et al. (2024) focus on direct jobs at a very local scale (finer than our regional resolution). Popp et al. (2021) and Scheifele et Popp (2024) use a method that should provide overall results for both indirect and direct job creations.

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.

**Figure 1: Subsidies granted to French households, in billion EUR**



Source: The figure displays the bi-yearly value of energy efficiency subsidies granted by energy suppliers. We used data from the French Ministry of Ecological Transition to compute the number of certificates generated by projects within the EEO scheme. We then multiply this number by the contemporaneous estimated value of grants associated to each certificate according to the report on the financial effort for energy renovation of buildings published by the French government in 2024.

Using this innovative estimation strategy, we address specific dimensions of green jobs creation. We take advantage of the sharp difference in job stability between permanent and fixed-term contracts in France to assess how permanent job creations from energy retrofit stimuli might be. Indeed, two opposing forces are at play. On the one hand, temporary subsidies could help structure value chains, stimulate innovation, and lead to long-term green job creation. Indeed, Popp et al. (2021) and Scheifele et Popp (2024) find stronger impacts in the long term compared to the short term, possibly because investments may allow structuring value chains. On the other hand, political uncertainty around the future of the policy could have led to a rise in short employment contracts, which can be terminated more easily. Such short-lived employment effects are supported by results in Fabra et al. (2024) suggesting 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. Moreover, short employment contracts could also affect the quality of the energy retrofits, with poor workmanship quality being one of the reasons for the energy performance gap (Giraudet, Houde and Maher 2018).

Finally, recent economic studies have questioned the effectiveness of energy retrofit programs, with realized energy savings being significantly lower than predicted savings (Davis, Fuchs and Gertler 2014, Liang, et al. 2018, Fowlie, Greenstone and Wolfram 2018, Lang and Lanz 2022). 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 2019, Hafstead and Williams 2020, Marin and Vona 2021, Haywood, Janser and Koch 2024, Rud, et al. 2024), 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.[9] Each period-specific 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.[10] 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

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[9] 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.

[10] 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.

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 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).[11] They correspond to the average price of all the certificates sold during a month.[12] 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.

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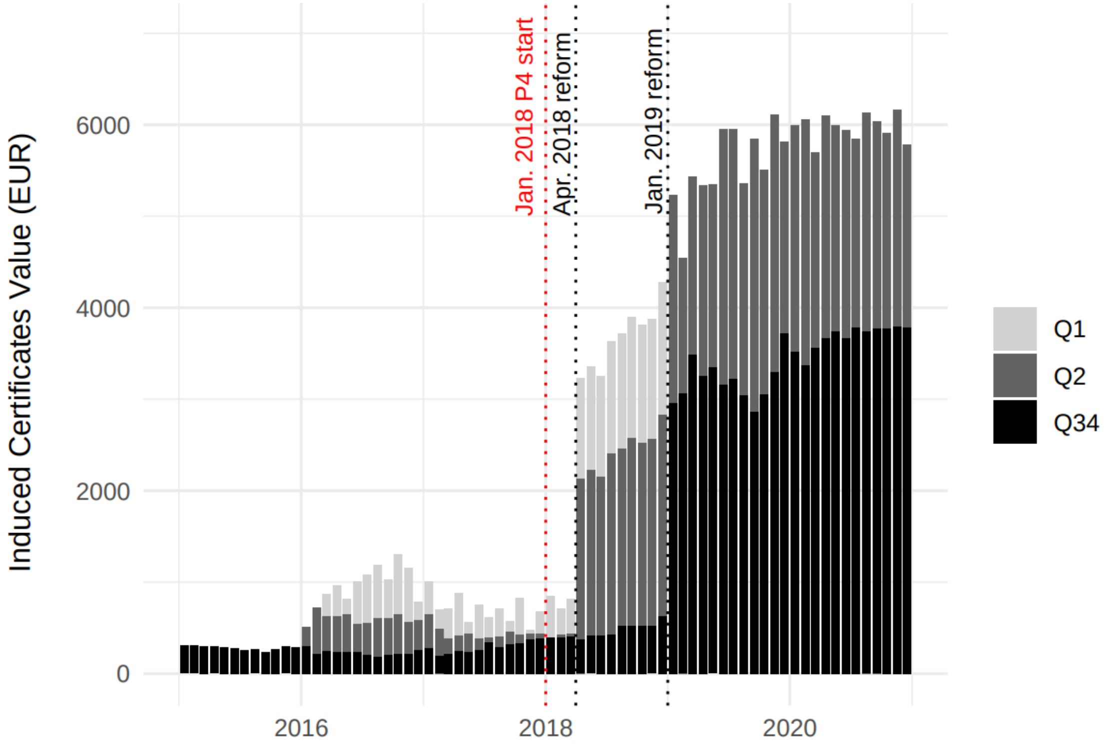
[11] For more information on the register, see: https://opera-energie.com/emmy-registre-national-cee/.

[12] 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.

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).

**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.

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.

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 April 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".[13]

The WMD collates all employment records from an official document that companies must fill every month, entitled the Nominative Social Declaration (NSD).[14] 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

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

[14] *Déclaration Sociale Nominative* in French

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 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.[15] 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.[16] 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

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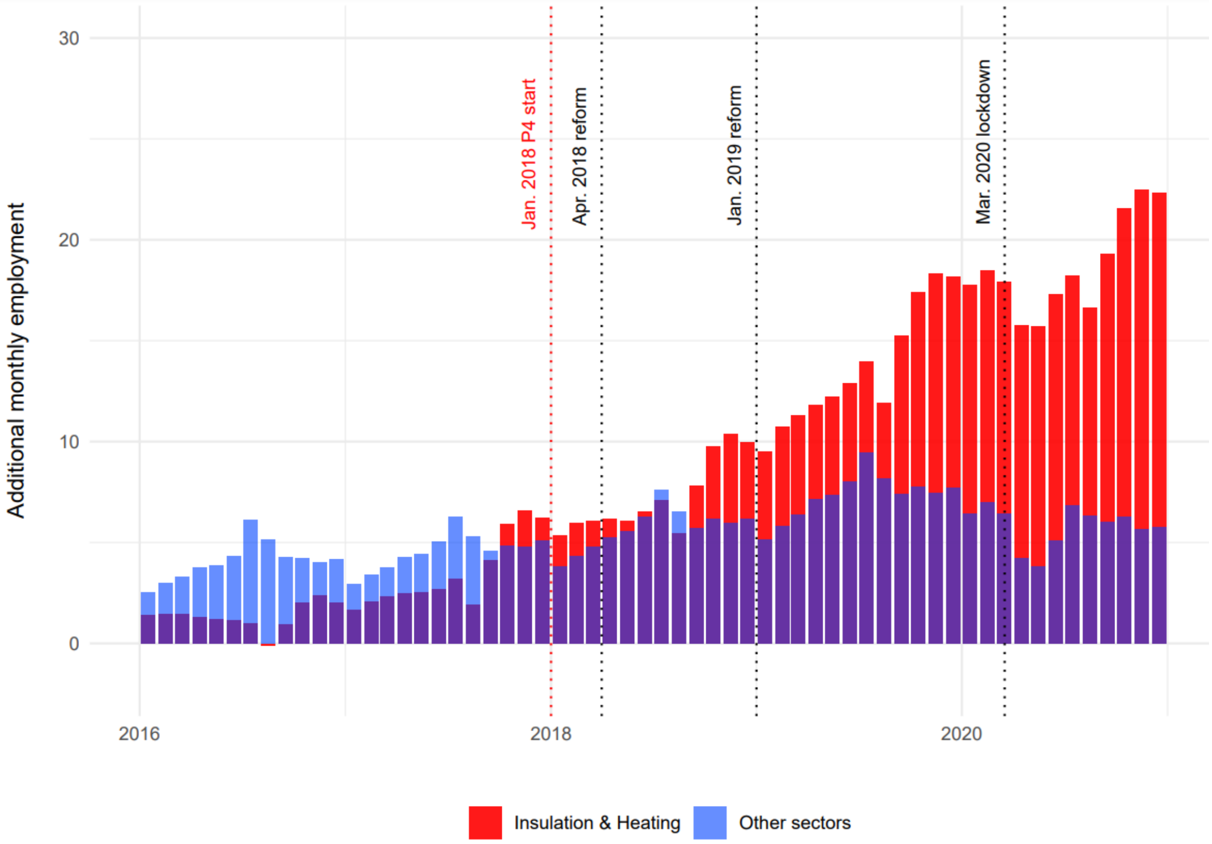
[15] The dataset starts in the second semester of 2015. However, the data collection quality at the beginning was substantially lower due to the progressive rollout of NSDs. For that reason, we do not use 2015 data in our baseline analyses.

[16] 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.

government-support schemes implemented to fight COVID-19.

**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 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,[17] 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

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[17] 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.

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.

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.[18] 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

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[18] 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**).

national aggregates.

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.[19] 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

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

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 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. Because the first implementation reform was enacted only in April 2018, we calibrate the PSC over a 27 month pre-treatment period, from January 2016 to March 2018. The main treatment effect on the treated which we report is the difference between cumulative employment growth and its synthetic counterfactual over the 23 months from April 2018 to 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 B**. 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 April 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**.

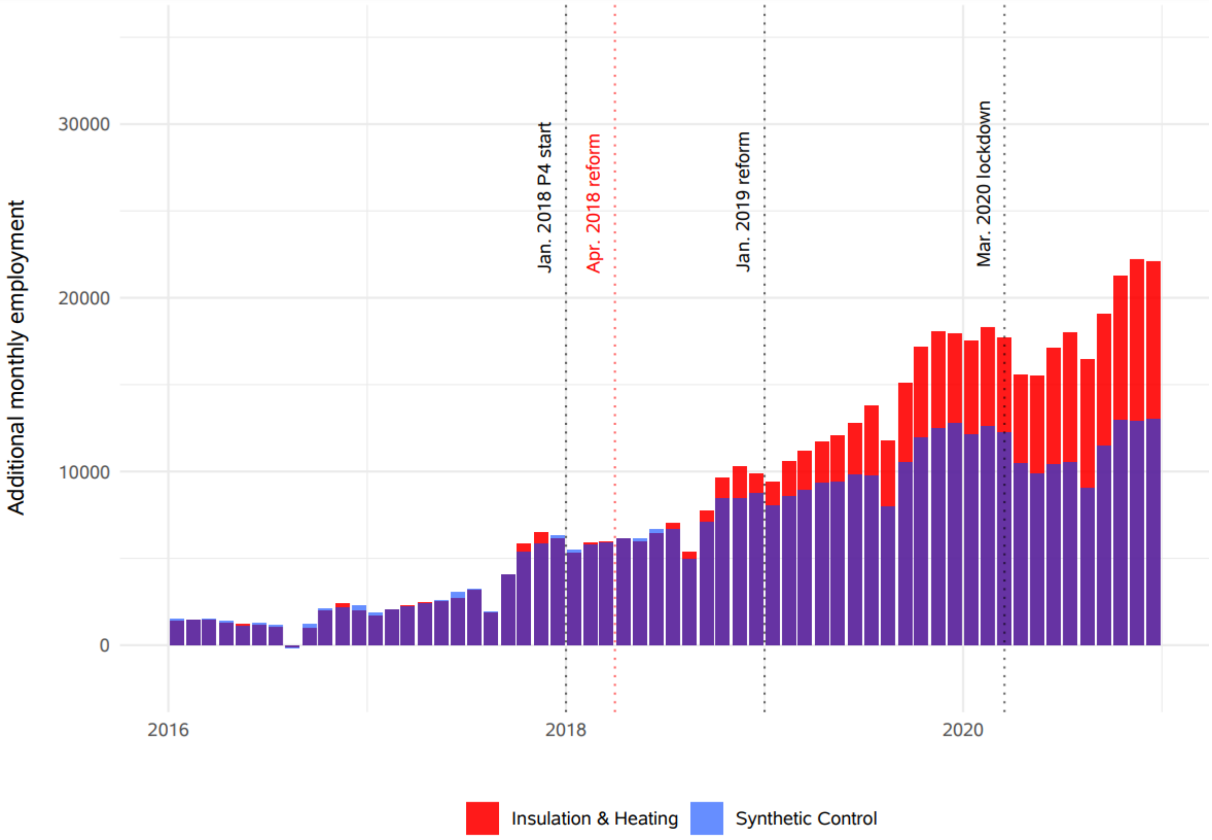
## 5. Results

### 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 first implementation reform in April 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 by 58,000 job-months, equivalent to the creation of about 4,900 additional job-years by February 2020 (before the first lockdown in France caused by the COVID-19 pandemic). Over the same period, cumulative employment growth increased by 12,276 jobs in the retrofit sectors (as presented in **Table A1** in **Appendix A**). Thus, our estimates attribute around 40% of the rise in sectoral employment between April 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 238 million, more than three times higher, between January 2019 and February 2020.

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

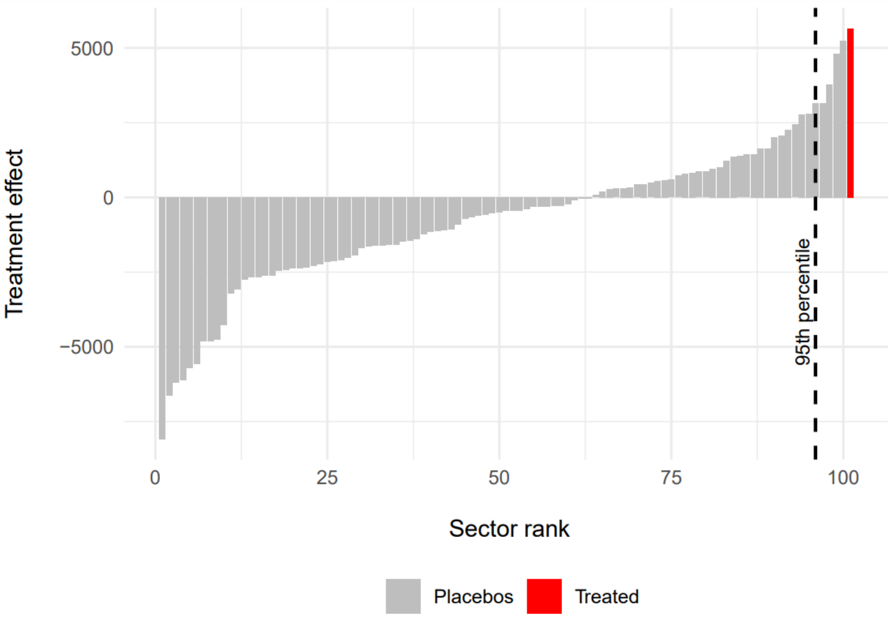


**Notes:** When the “synthetic control” (blue) and “insulation and heating” (red) sectors overlap, the color displayed on the graph becomes purple. The pre-treatment period includes all months from Jan. 2016 to Mar. 2018. The treatment period follows, from Apr. 2018 onwards. $\lambda^* = 0.01$ (almost no penalization of direct matches). $p$-value for the one-sided test is 0.01.

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, independent 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 ranks first against 100 other sectors. The corresponding $p$-value for the one-sided test is 0.01, hence we can confidently interpret the additional 4,900 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 $\lambda^* = 0.01$. 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 0.0099.

Several robustness checks confirm our findings. In **Appendix C.1**, we move the assumed starting time of the policy shift to January 2019 when the second implementation reform is implemented. This is considering that most of the changes in investment levels do not occur in April 2018, but later. The overall effect when the start is set in January 2019 significantly reduces our estimates, but the estimated increase still represents about 3,600 additional workers ($p$-value of 0.01). 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 occurred in January 2018, leading to anticipations by regulated companies, we run an anticipation test with a treatment date starting in January 2018, one trimester before the first implementation reform (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 previous obligation before the end of the year. However, this effect is small in magnitude (below 70 job-years). Effects after January 2018 remain clearly identified and comparable to our baseline figures with about 4,900 additional workers ($p$-value of 0.03).

We also run our econometric estimation (with the baseline date of April 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 4,850 additional workers ($p$-value of 0.02). It goes up to 4,950 additional workers ($p$-value of 0.01) when using the wider interval (659 donors). Thus, our results are robust to the selection of sectors included in the donor pool.

Finally, we perform two robustness checks to ensure the absence of violation of the stable unit of treatment value assumption (SUTVA). We first look at the relative importance of donor sectors in our penalized synthetic control (See **Appendix D.1**). Summing weights at the national level, we get a series of 10 sectors representing more than three quarters of the total. The first three sectors (about 44% of all weights) gather *Activities of holding companies*, *Maintenance and repair of light motor vehicles*, and *Chartering and transportation organization*. Other important sectors include very diverse productions of goods and services, which plays in favor of our identification strategy. Indeed, a greater variety of sectors and a limited weight for each of them decreases the risk of contamination by unobserved employment dynamics occurring in the donor pool.

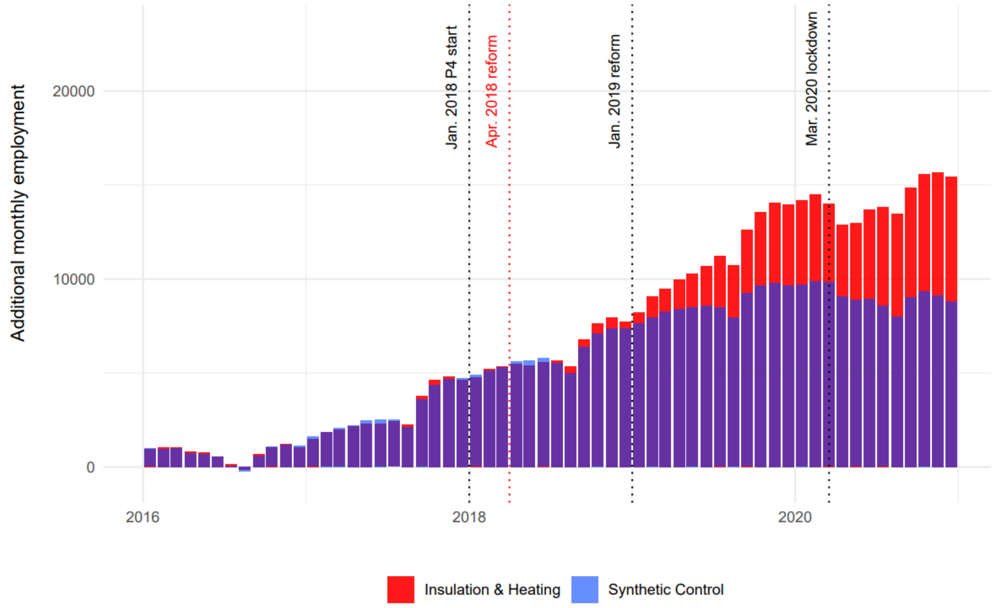
To further emphasize the robustness of our estimation to spillover effects, we perform the baseline synthetic control estimation on a modified donor pool (See **Appendix D.2**). Looking at all new hires in the treated sector between April 2018 and February 2020, we define each worker's sector of origin as her last sector of employment before joining the retrofitting industry. If such a sector stands out, we may fear contamination from unobserved employment dynamics fueling both an inflow of workers into the treated sector and an outflow from control sectors. To deal with this issue, we aggregate all sectors of origin at the section level (21 codes) and exclude the top 5 sections of our donor pool, from which originate 62.5% of all new hires (See **Figure 16**). The estimation on this restricted donor pool yields an estimated effect of about 4,900 additional workers ($p$-value of 0.01, see **Figure 17**). This confirms that our results are not driven by unobserved spillover effects from or to the sectors included in the donor pool.

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

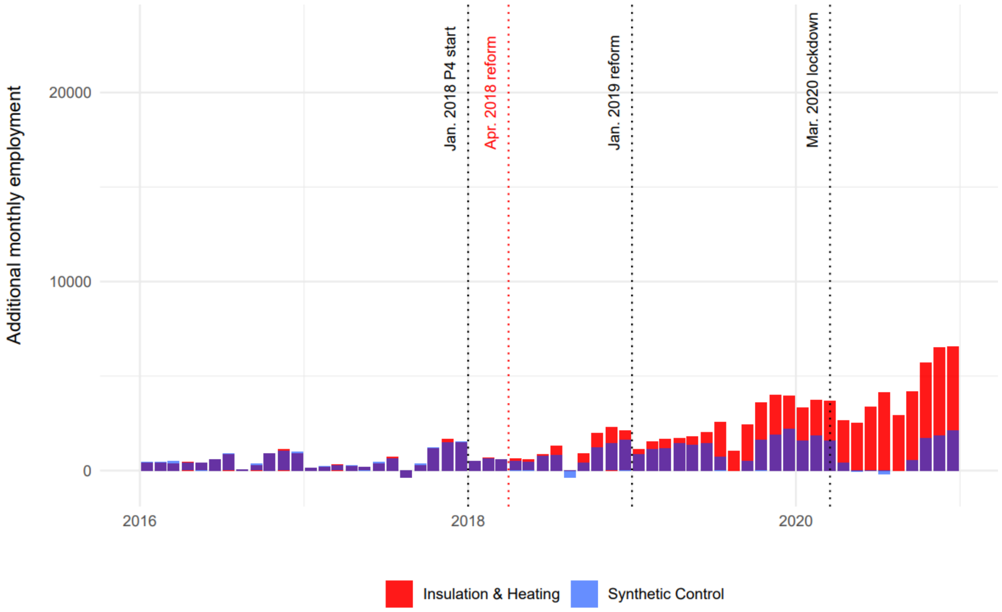
We look at the impact of the policy on permanent contracts vs. fixed-term contracts to assess whether the policy reform led to long-term job creation. In France, social protection laws imply that employers are often very reluctant to offer permanent positions because firing people can be very costly (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.

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 3,400 of the jobs created were permanent, amounting to around 70% of the estimated effect of the EEO reforms on total employment ($p$-value of inference test is 0.01). For fixed-term contracts, we find that nearly 1,750 jobs were created. However, the inference tests suggest that the effect is only weakly significant. The estimated effect ranks 5th out of 101 permutations, corresponding to a $p$-value of 0.05.

**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 Mar. 2018. The treatment period follows, from Apr. 2018 onwards. The penalization parameter is optimized at $\lambda^* = 0.01$ for both panel (a) and (b). 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 0.01 (a) and 0.05 (b).

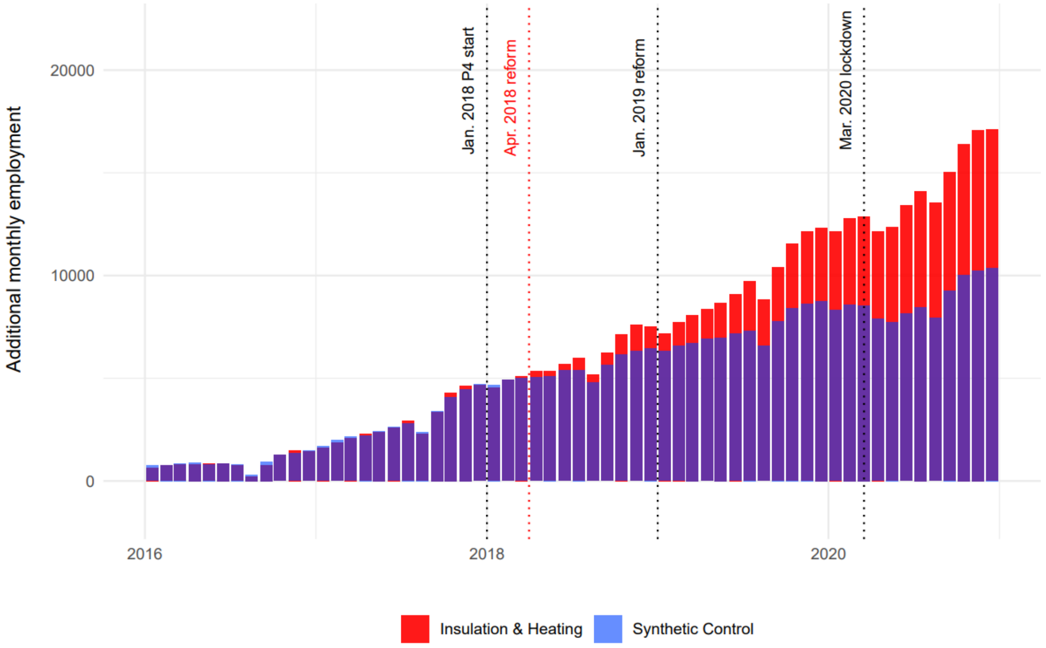
### 5.3. Effect by initial firm size (Micro vs. Small enterprises)

We investigate the heterogeneity of job creation across firms of different sizes. Indeed, 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 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. The distribution of job creation is also central in the political debate, because massification (hence, internal growth) is often seen as a first step towards the industrialization of retrofitting activities. On the other hand, smaller firms may have a stronger connection with their customer base and be more attentive to reputation effects, which are key in a credence goods market such as energy efficiency services.

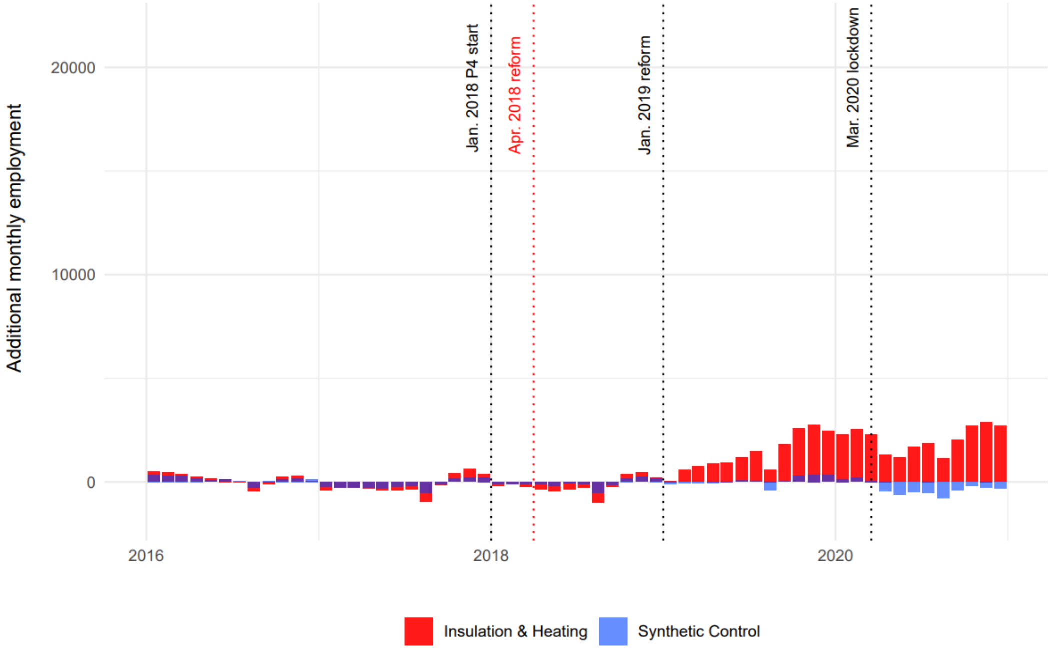
In **Figure 7**, we report the aggregate results after running the synthetic control model by firm size (see panel (a) for micro-enterprises below 10 workers, and (b) for small enterprises between 10 and 49 workers). We find that about 3,300 job-years were created within micro-enterprises, amounting to around 70% of the estimated effect of the EEO reforms on total employment ($p$-value of inference test is 0.01). Within small enterprises, we find that nearly 2,000 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.

As a result, one could claim that the rise in EEOs funding benefited primarily micro firms with a strong local anchor. This may be linked to the credence good nature of energy efficiency works, which requires confidence from customers and liability from installers. However, it is also useful to put these results in regards with the job creation dynamics in the synthetic control. In panel (b), we see that non-policy induced job creation is almost inexistant in small firms. Thus, the policy also fueled the growth of bigger firms active at a larger geographical scale.

## Figure 7: Employment growth in energy renovation vs its penalized synthetic control by initial firm size



(a) Micro enterprises



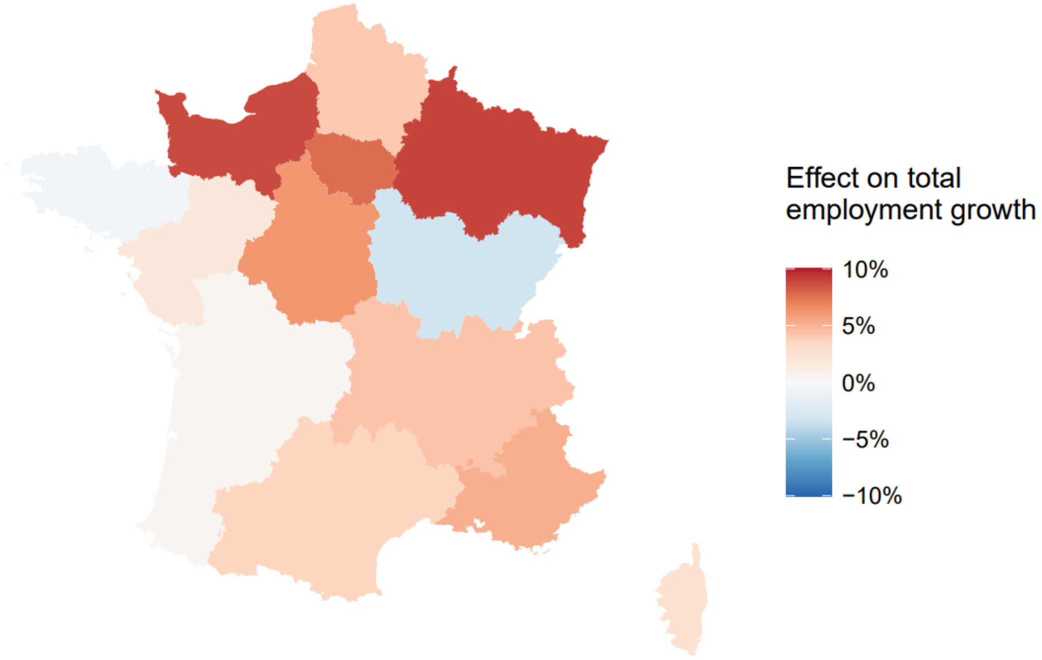
(b) Small enterprises

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 Mar. 2018. The treatment period follows, from Apr. 2018 onwards. The penalization parameter is optimized at $\lambda^* = 0.01$ for both panel (a) and (b). $p$-value for the one-sided test is 0.01 (a) and 0.07 (b).

### 5.4. Regional estimates

An innovation of the synthetic control method used in this paper is to allow estimating separate effects at regional level. **Figure 8** provides our regional estimates for total employment. Our estimates indicate that Northern regions particularly benefitted from the rise in investment through the EEOs. Indeed, nationwide job creations induced by the policy between April 2018 and January 2020 amount to 5% of the 2016 employment level in the energy retrofit sectors. This share is as high as 8.8% in Normandy, and 9% in Grand Est. On the contrary, policy-induced positions in Western regions, where the climate is milder, represent at most 2% of the industry's total employment at the start of the fourth period of the EEOs. Finally, the Eastern region of Bourgogne-Franche-Comté, which combines both a lower population density and a lower GDP per capita, stands as an outlier with a -3% effect of the policy on total employment.

**Figure 8: Effect of the policy on sectoral growth (% of Jan. 2016 total employment)**

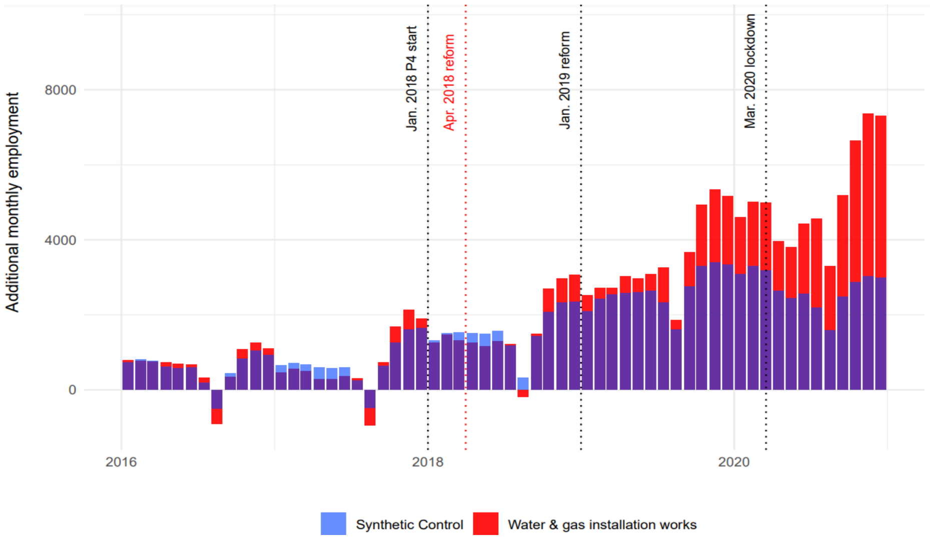


Notes: Baseline estimation ($\lambda^* = 0.01$, $p$-value 0.01). Percent changes are reported in relative terms with respect to the average employment level in the retrofitting industry in January 2016.

### 5.5. Effect on related sectors

We defined our treated group as the sum of the “insulation” (4329A) and “installation of heating equipment” (4322B) sectors. However, these “main activity” codes are defined by the French Statistical Office for administrative purposes and do not provide a comprehensive description of a firm activity. As such, firms in related construction sectors could have been affected by the policy change, either positively (because they also operate on the market for energy efficiency), or negatively through a drain of their employees in retrofitting activities. To investigate this issue, we leverage a key feature of the French energy efficiency regulation called the *RGE label*. This quality certification is granted to firms whose employees have followed a short training session (between 3 to 5 days) on building energy performance. As shown in **Figure 18**, we identified 10 sectors with a monthly headcount of RGE labelled firms above 10,000. Using our baseline estimation strategy, we find a significant effect for only one sector, “Water and gas installation works”. Indeed, firms operating under this code registered an increase in employment by +1,150 (0.08) job-years.

**Figure 9: Employment growth in water & gas installation vs its penalized synthetic control**

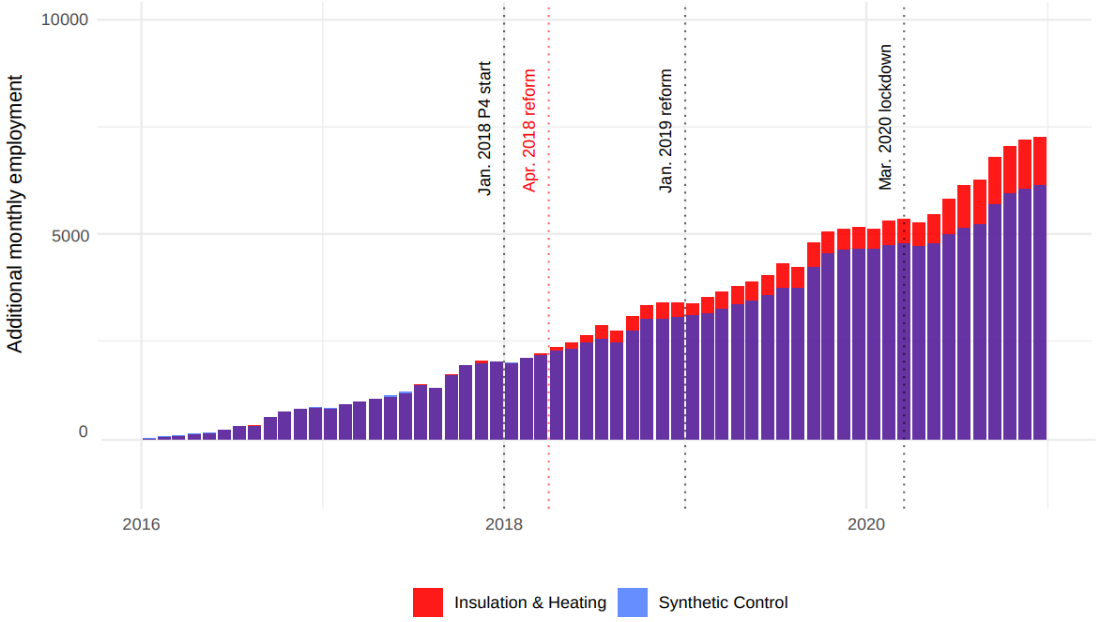


**Notes:** The donor pool is based on the ±33% interval around the national headcount of the retrofitting industry in Dec. 2017. The pre-treatment period includes all months from Jan. 2016 to Mar. 2018. The treatment period follows, from Apr. 2018 onwards. $\lambda^* = 0.01$; $p$-value for the one-sided test is 0.01.

### 5.6. Additionality of employment growth

Our approach leverages variations at the sector level, and documents a causal effect of investment on employment in the energy efficiency industry. However, one might ask if this employment growth is additional, or if it consists in transitions from other, non-related sectors. To investigate this question, we create an indicator variable that takes value 1 if a contract follows a period of at least 1 month of unemployment, and 0 otherwise. We then replicate our analysis, keeping only those contracts with a value of 1. We find a significant effect on exits from unemployment, with +750 (0.02) additional job-years induced by the policy. This is more than 15% of our total employment effect, which bears in favor of additionality at the economy-wide level. Moreover, new hires usually occur under a fixed-term contract, which represents one third (1,750 out of 4,900) of all policy-induced additional job-years (see **Section 5.2**). Thus, the policy-induced inflow of workers exiting unemployment equals more than 40% of fixed-term employment.

**Figure 10: Exits from unemployment vs its penalized synthetic control**

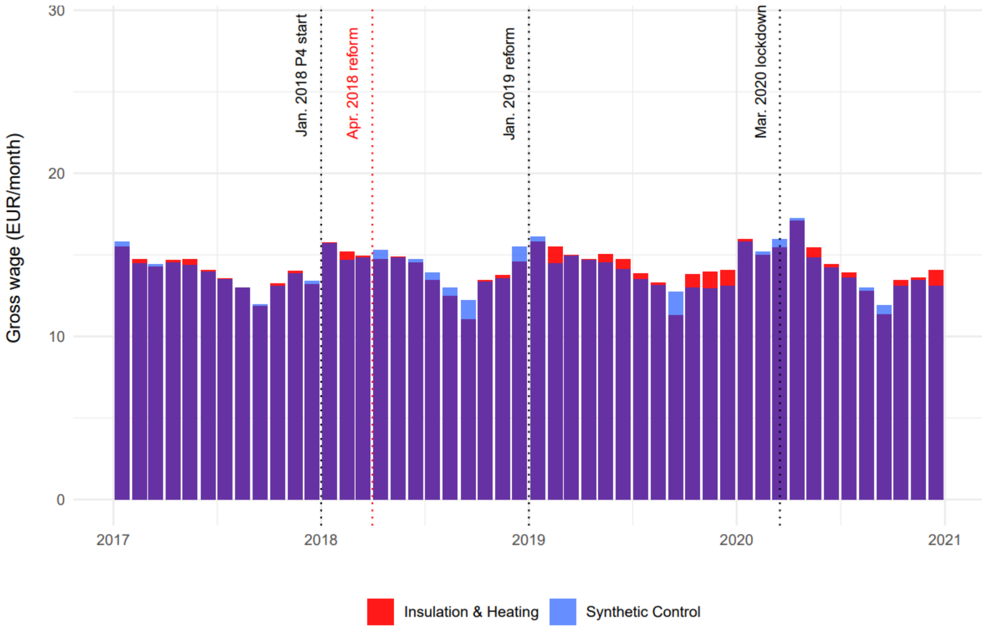


**Notes:** The donor pool is based on the ±33% interval around the national cumulative employment growth of unemployment-exiters in the retrofitting industry as of Dec. 2017. The pre-treatment period includes all months from Jan. 2016 to Mar. 2018. The treatment period follows, from Apr. 2018 onwards. $\lambda^* = 0.01$; $p$-value for the one-sided test is 0.02.

## 5.7. Hourly wages

One possible explanation for the moderate employment effect of the policy could be the lack of suitable candidates active on the labor market. In such a case, the compensation of newly hired workers should adjust to reflect the scarcity of the labor force. We investigate this question using another dataset from the Ministry of Labor (DADS – Postes), which records hourly wages at the worker-establishment level on an annual basis. To exploit this information, we average hourly wage for newly hired workers in each sector and region across all months from January 2017 to December 2020. We then follow our estimation strategy to analyze the effect of the policy on hourly wages. The pre-treatment period, although shorter than in our baseline estimation, still leverages 15 months of variation across the 12 Metropolitan French regions. Using the exact same donor pool as in the baseline estimation, we find no significant effect on hourly wages.

**Figure 11: Hourly wage in energy renovation vs its penalized synthetic control**



**Notes:** The pre-treatment period includes all months from Jan. 2017 to Mar. 2018. The treatment period follows, from Apr. 2018 onwards. $\lambda^* = 1.05$. The red bars represent the evolution of the hourly wage 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 0.33.

## 6. Discussion

If we compare our main job-creation estimate with total spending in the EEO scheme, we obtain that the policy sustained 1.5 direct jobs for each additional million euro invested.[20] 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. (2024)'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. (2024), 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. (2024) 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

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[20] 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 150 million. This is equivalent to a monthly increase by EUR 116 million on average. According to our estimations, the additional monthly employment growth caused by the policy was +181 workers a month (4,900 / 27). It follows that the policy led to an estimated increase in employment by 1.5 workers for each million euros invested in the EEO scheme: 181 / 130 = 1.56.

performed by SMEs locally, then our estimates and those of Fabra et al. (2024) 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. 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.

The rise in EEOs funding also had distributive effects across firms of different sizes. It primarily benefited micro-enterprises with a strong local anchor favored by the credence good nature of energy efficiency works. However, it also fueled the growth of bigger players, in line with the political objective of massification in the energy efficiency industry.

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.[21] 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 1.5 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. 2024), 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 slightly higher magnitude than other green investments in the energy transition.

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[21] 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.

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, to complement our results on fixed-term employment. We also want to evaluate the policy's effect on the overall number of firms, to disentangle internal growth and creation of new businesses. Finally, we wonder if firms passed through energy efficiency grants to their profits, a legitimate question given the low effect on both the quantity and price of labor in the industry. Notice, however, that such a pass-through would not necessarily be detrimental to the development of the industry, because it would increase margins, hence increasing the profitability of offering such services. In that regard, we plan to leverage firm-level accounting data to better understand their investment behavior. 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 | +957 | +430 |
| Dec. 2017 | +4,651 | +1,505 |
| Mar. 2018 | +5,363 | +622 |
| Feb. 2020 | +14,525 | +3,736 |
| **Other sectors** | | |
| Jan. 2016 | +131,433 | +275,265 |
| Dec. 2017 | +354,258 | +466,988 |
| Mar. 2018 | +400,576 | +373,371 |
| Feb. 2020 | +739,822 | +391,657 |

## 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 $T_0 = 27$ is the duration, in month, of our pre-treatment period from January 2016 to March 2018. Each column in $X_i$ therefore gives cumulative employment growth in month $t = 1, \dots, T_0$. We thus have 27 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 April 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 $\lambda$. 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 program. 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 $\lambda$ weights both minimizing functions, and the optimal set of weights $W_i^*(\lambda)$ is a function of $\lambda$. 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 $\lambda$ 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 $\lambda^*$ 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 $\lambda$ 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 $\lambda^*$ 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 one 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 $\lambda^*$. 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 $\hat{T}\left(\boldsymbol{D}^{(\boldsymbol{b})}, \lambda^*\right)$ such that:

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

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:

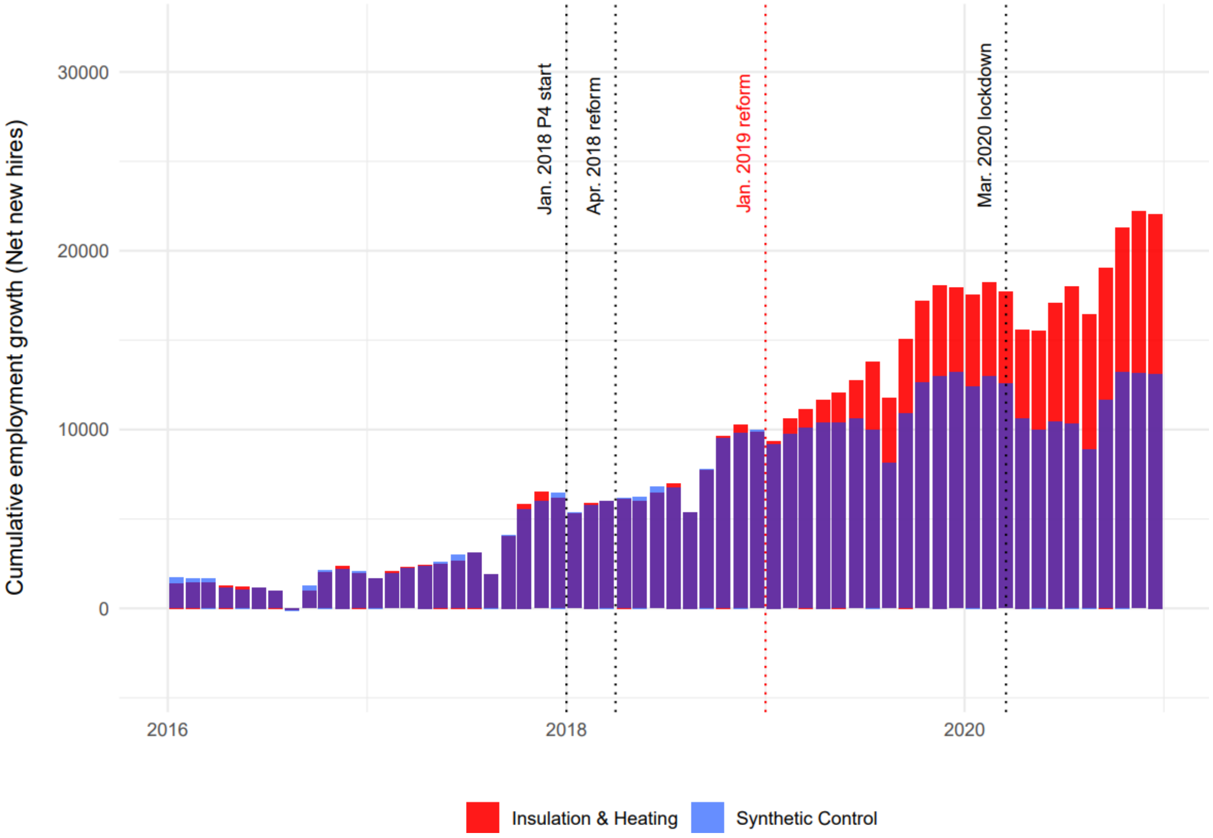
$$\hat{p} = \frac{1}{100+1}\left(1 + \sum_{b=1}^{B=100} \mathbf{1}\left\{\hat{T}\left(\boldsymbol{D}^{(\boldsymbol{b})}, \lambda^*\right) \geq \hat{T}\left(\boldsymbol{D^{obs}}, \lambda^*\right)\right\}\right) \tag{6}$$

# C. Sensitiveness analysis

## C.1. Later starting date

Below, we use January 2019 as the starting date of the policy, assuming no policy effect on employment before. This date was chosen to match the reform that occurred after the fourth phase of the scheme came into force. With a starting date in January 2019, results are below our baseline estimation, with 3,600 additional workers annually ($p$-value of 0.01).

**Figure 12: Trend in employment growth in the energy renovation vs its synthetic control, assuming a policy start in January 2019**

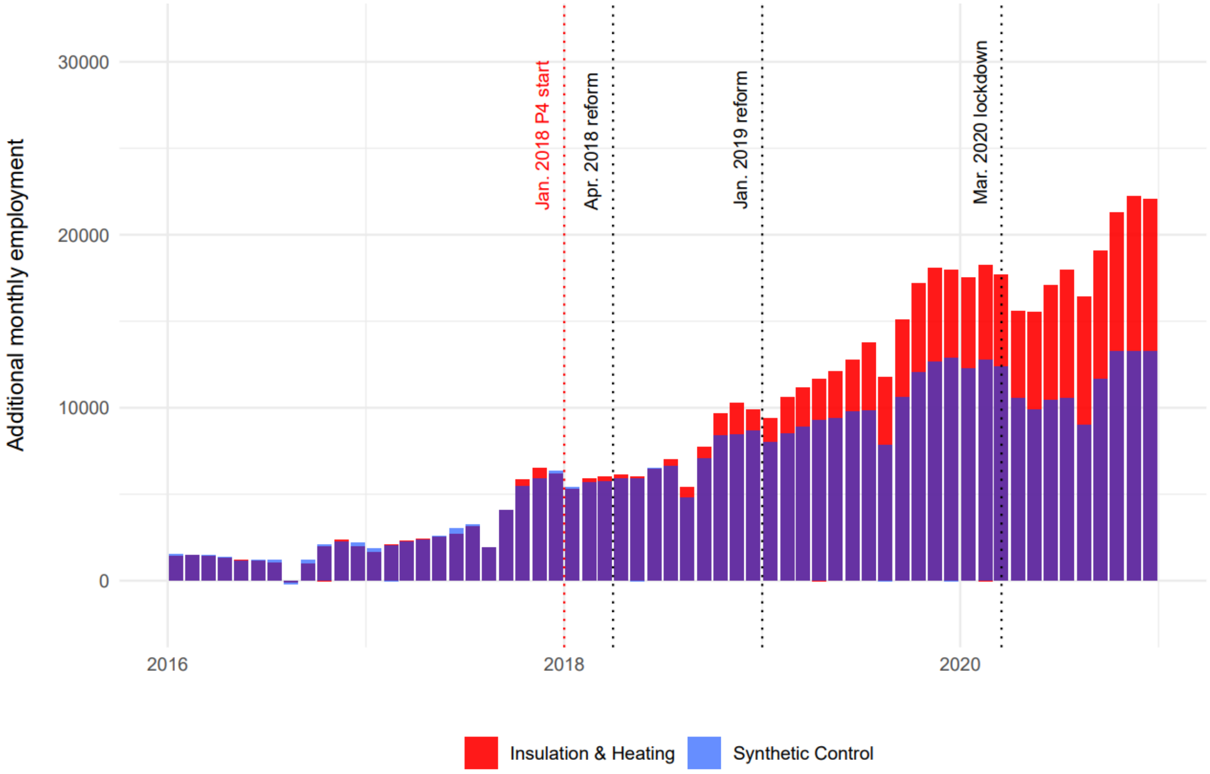


**Notes:** The pre-treatment period includes all months from Jan. 2016 to Dec. 2018. The treatment period follows, from Jan. 2019 onwards. $\lambda^* = 0$. The red bars represent the evolution of the workforce in the energy renovation sector, the blue ones display the evolution for its synthetic control 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 0.01.

### 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 first implementation reform started with the start of the fourth period of the EEO scheme, in January 2018.

**Figure 13: Trend in employment growth in the energy renovation vs its synthetic control, assuming a policy start in January 2018**



**Notes:** The pre-treatment period includes all months from Jan. 2016 to Dec. 2017. The treatment period follows, from Jan. 2018 onwards. $\lambda^* = 0.01$. 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.

As suggested in **Figure 3**, employment dynamics may have started to diverge slightly before January 2018 since energy providers had to rush to comply with their obligation under the third phase, closing in December 2017. In **Figure 13**, 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 68 job-years) and may stem from the end of the third phase rather than the changes introduced during the fourth phase, we keep April 2018 as our baseline starting date.

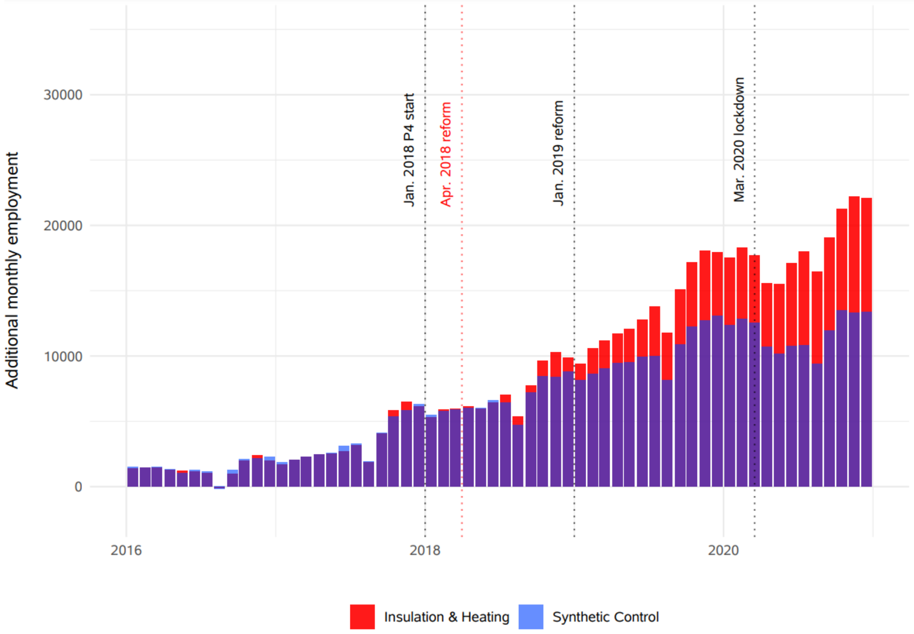
### C.3. Alternative pools of control sectors

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).

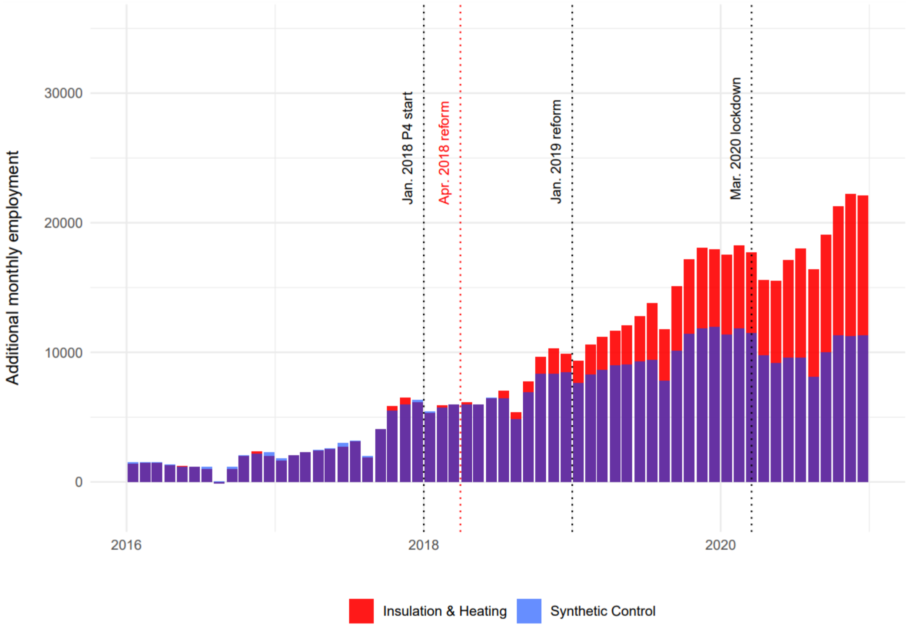
Our baseline donor pool included all regional sectors which national headcount was comprised within a ±33% interval around this threshold. This yielded 427 different control sectors. Below, we perform two robustness checks for this selection rule, with a ±25% and ±50% intervals, leaving us with 336 and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

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.

**Figure 14: Estimation with different intervals to restrict the pool of control sectors**



(a) ±25% interval



(b) ±50% interval

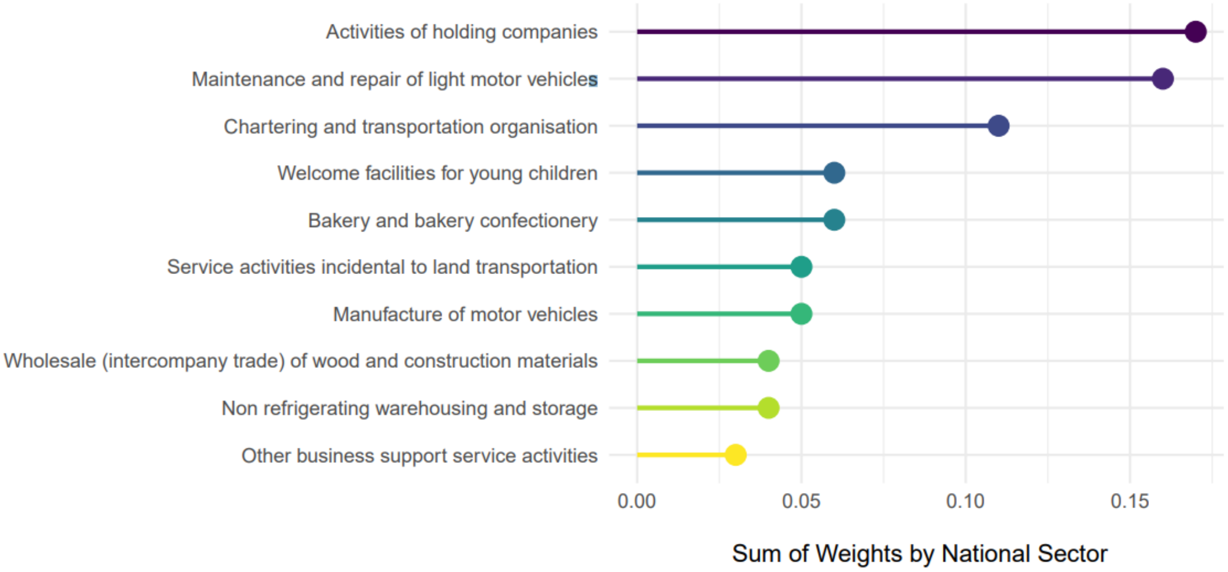
**Notes:** Estimation is similar to the baseline, except for the rule used to restrict the pool of control sectors.

# D. Stable Unit of Treatment Value Assumption

## D.1. Weights of the Penalized Synthetic Control

We aggregate the weights of each regional sector at the national level and rank the top 10 national sectors. They represent 77% of all weights used in the synthetic control, with the top 3 gathering 44%.

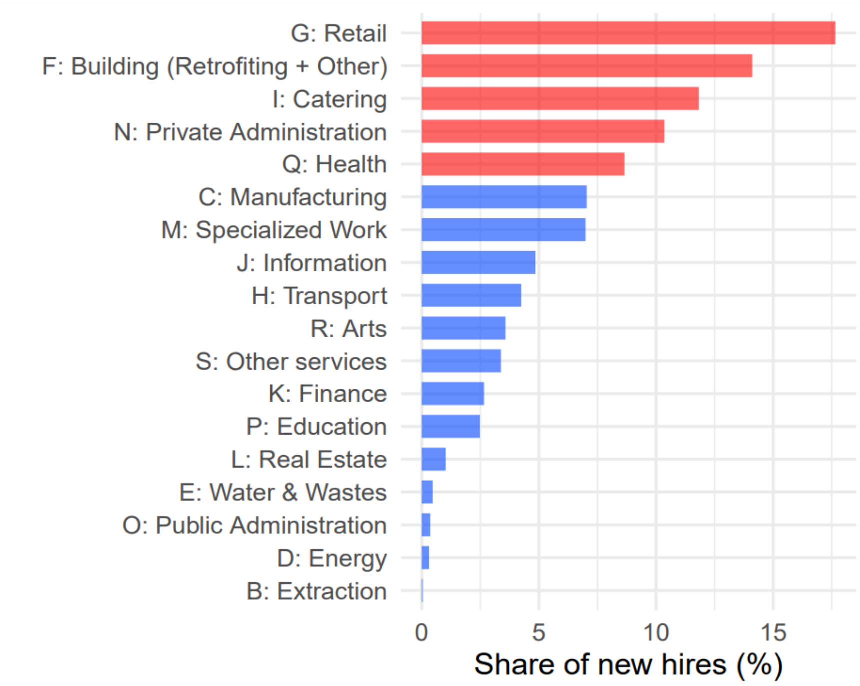
**Figure 15: Top 10 National Sectors by their Sum of Weights**



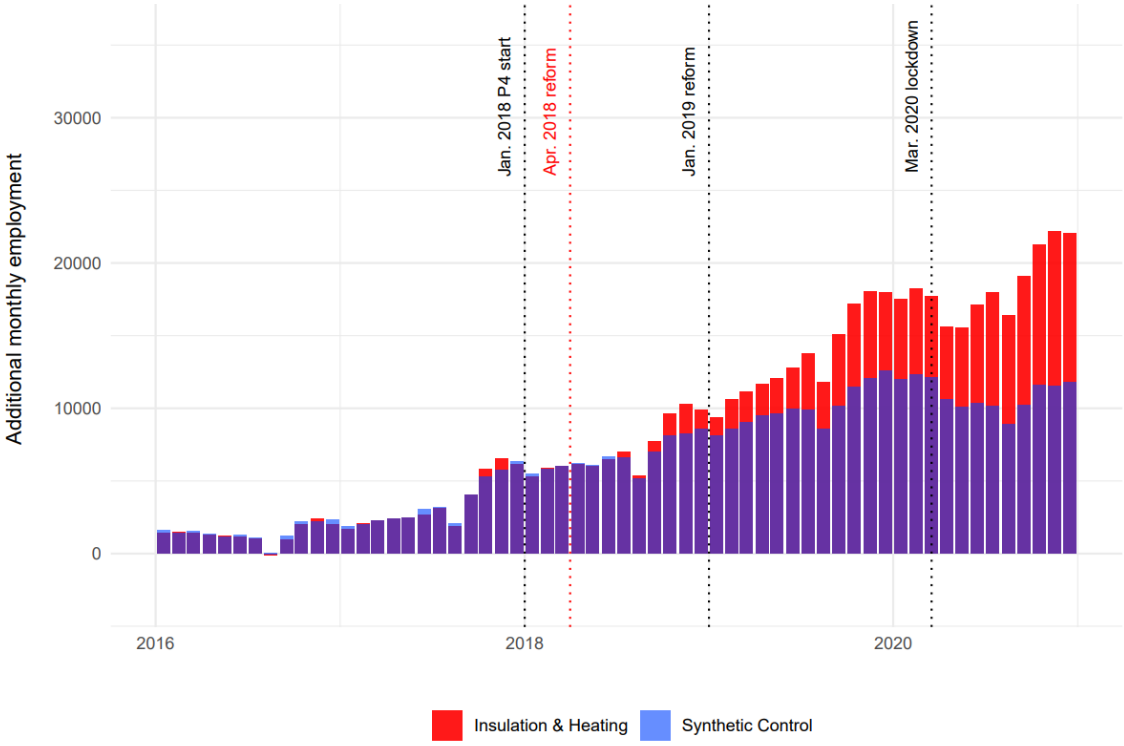
## D.2. Restricted Donor Pool

We aggregate the sectors of origin of new hires over the treatment period (April 2018-February 2020) at the section level (21 codes). The top 5 gathers 62.5% of all new hires (see below **Figure 16**). We then restrict our donor pool by excluding these 5 sectors, which heavily reduces the risk of contamination from other, unobserved employment dynamics occurring at the sectoral level. The estimation on this restricted donor pool yields an estimated effect of about 4,900 additional workers ($p$-value of 0.01). This confirms that our results are not driven by unobserved spillover effects from or to the sectors included in the donor pool.

## Figure 16: Top 5 origin sectors



## Figure 17: Estimation on a restricted donor pool (SUTVA)



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

### D.3. Related sectors

The RGE label is a quality certification for energy renovation firms. We use this label to identify other sectors involved in the renovation industry over our period of interest. Among the ten top sectors by monthly headcount, the insulation and heating sector ranks first with almost 60,000 employees. Then comes Wood and PVC joinery, and General masonry, both above 40,000 employees. General masonry ranks fourth with roughly 33,000 employees.

**Figure 18: Average monthly headcount of RGE labelled firms, by sector**