Overpromising Green Jobs? Ex-Post Evidence from French Energy Efficiency Obligations

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Abstract: Concerns over job losses are eroding support for climate action. The EU Green Deal promises one million jobs by 2030, with energy efficiency as key driver. However, projections rely on unverified ex-ante estimates. This paper provides the first ex-post estimate of employment impacts from a large-scale energy efficiency programme. Using a policy discontinuity in France and a state-of-the-art synthetic control method on disaggregated data, we find 1.5 job-years created per million euros invested, far below the 8.52 jobs assumed in EU assessments. This challenges widely used labour market projections, underscoring the need for ex-post validation to avoid misallocating public funds.

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1. Introduction

Can large-scale climate investments deliver meaningful employment gains, particularly for low-skilled workers?

Over a trillion dollars have been committed globally to green recovery programmes in the wake of the COVID-19 pandemic,[2] accelerating a much-needed energy transition. Yet achieving the pace of decarbonization required to meet climate goals will demand sustained public investment and policy support throughout the 21st century.

A key barrier to such investment is the persistent concern over job destruction. Climate policies, particularly carbon taxation and regulation, are often feared for raising costs and eliminating jobs in carbon-intensive industries. Meanwhile, the rapid progress of generative AI has amplified anxiety about automation, further clouding job prospects. The United States' recent decision to withdraw from the Paris Agreement, citing concerns over industrial competitiveness and economic growth, underscores the ongoing tension between climate action and employment.

In Europe, these concerns are central to the debate over financing the next phase of the green transition. The EU Green Deal is built on the promise of creating one million new jobs by 2030 (European Commission 2020), with energy efficiency investments, especially building retrofits, expected to drive employment for low-skilled workers. In France, for instance, such retrofits account for over 70 percent of planned green investment.[3]

However, these optimistic job creation estimates have yet to be validated ex post. Most projections rely exclusively on ex-ante macroeconomic models. For energy efficiency policies, no causal estimate of employment impact has been produced to date, despite their central role in green transition strategies. Without empirical validation, job promises from

[2] 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).

[3] 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).

energy efficiency investments risk losing credibility and may overstate the labor market gains from climate spending.

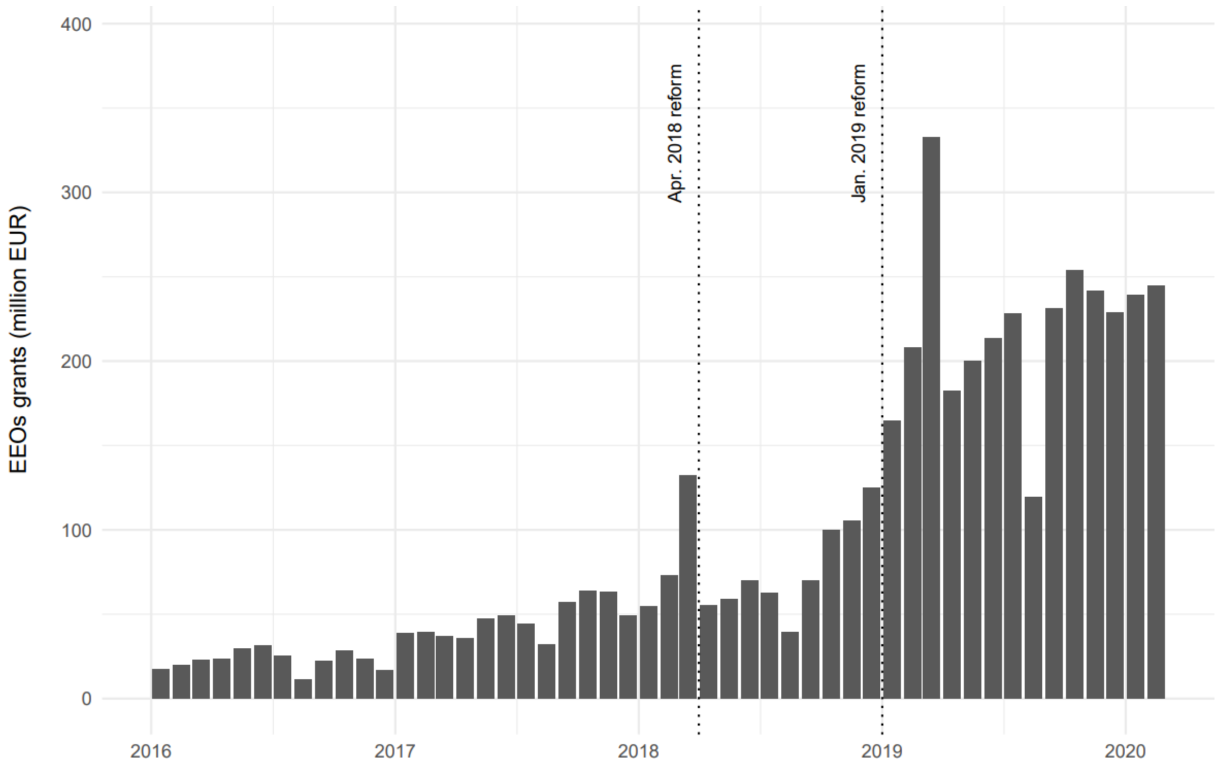
This paper provides the first ex-post causal estimate of job creation from a large-scale energy efficiency programme, offering new, policy-relevant evidence on an issue central to both climate policy and labour markets. Energy efficiency policies could boost low-skilled employment, as they require on-site construction work that cannot be easily automated or offshored. Unlike renewable energy investments, which often rely on capital-intensive manufacturing, energy efficiency retrofits generate labour-intensive demand in installation, maintenance, and renovation.

We examine the French Energy Efficiency Obligations (EEO) scheme, one of Europe's largest green investment policies, with over €6 billion invested annually (Broc, Stańczyk and Reidlinger 2020). France provides a particularly relevant case study due to its labour market, where low-skilled workers face persistent structural barriers to employment. Understanding how energy efficiency policies affect job creation in this context provides valuable insights for other economies where low-skilled employment opportunities are becoming increasingly scarce.

To estimate the employment impact of the policy, we exploit a sharp discontinuity in policy design (caused by reforms in 2018 and 2019), when subsidies for insulation and heating retrofits increased fivefold (from €35 million to over €175 million per month) (see **Figure 1**). Using a state-of-the-art synthetic control estimator with regional disaggregation (Abadie and L'Hour 2021), we construct synthetic employment trends for each regional retrofitting industry. Unlike traditional synthetic control models (Abadie and Gardeazabal 2003, Abadie, Diamond and Hainmueller 2010), our approach leverages disaggregated data at regional level, enhancing robustness and reducing vulnerability to spurious correlations. 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.

Our results indicate that the scheme created 1.5 job-years per million euros invested, far below the 8.52 jobs per million euros currently assumed for energy efficiency in EU policy assessments (European Commission 2019). These findings suggest that policymakers may be overestimating the job creation potential of energy efficiency investments, and possibly all green investments, reinforcing the need for rigorous ex-post evaluation to inform labour market policies in the green transition.

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



Source: The figure displays the monthly 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. We stop the analysis in February 2020, just before the start of the COVID-19 lockdown in France.

By quantifying job creation in a sector critical to blue-collar employment, this study contributes to a growing body of literature on the inclusivity of the green transition. Emerging evidence suggests that the green transition disproportionately benefits high-skilled workers, potentially limiting net job creation (Vona, Marin, et al. 2018, Yip 2018, Marin and Vona

2019, Saussay, et al. 2022, Curtis, O'Kane et Park 2024). Our findings offer a complementary perspective by showing that specific sectors, such as energy retrofits, can create stable employment opportunities for low-skilled workers, albeit at a lower scale than policymakers often assume.

Our results also reinforce the need for sector-specific job creation estimates. Prior research shows that green sectors differ in job intensity. In Spain, Fabra et al. (2024) find that solar energy generates significantly more jobs per unit of investment than wind energy, while Scheifele and Popp (2024) report that solar energy in Brazil creates strong short-term employment effects, whereas wind energy delivers only modest long-term benefits. Our study extends this literature by demonstrating that energy efficiency policies, while capable of creating local jobs, do so at a lower scale than commonly assumed.

Sectoral heterogeneity may therefore be crucial to understanding the labour market implications of large-scale investment packages. Popp et al. (2021) were the first to provide an ex-post estimate relevant to green job creation. They found that the 2009 American Recovery and Reinvestment Act (ARRA) created 2 to 4 jobs per million dollars in the construction sector. However, because ARRA combined many activities, such as energy retrofits, green infrastructure, and renewable energy investments, its employment effects cannot be disaggregated by sector. This lack of granularity makes it difficult for policymakers to identify which programmes are most effective at generating green employment, particularly in high-unemployment areas.

Overall, when looking at impacts for a wider spectrum of investments, our estimate of 1.5 job-years per million euros invested sits between the other ex-post estimates of Popp et al. (2021), Scheifele and Popp (2024), and Fabra et al. (2024). Because energy efficiency is often assumed to have a higher job creation content than renewable energy due to the need to operate on a fragmented housing sector, our estimate provides critical evidence to suggest that ex-ante forecasting methods may overestimate green job creation, including input-output models (Mikulić, Rašić Bakarić and Slijepčević 2016, Markandya, et al. 2016, Dell'Anna 2021) and computed general equilibrium models (Wei, Patadia and Kammen 2010, Sooriyaarachchi, et al. 2015).[4]

An additional contribution of this paper consists in providing new evidence on employment durability. Because France has a dual employment contract system, we can break out effects between temporary and permanent contracts. Temporary subsidies may help structure value chains and stimulate long-term job creation, as suggested by Popp et al. (2021) and Scheifele et Popp (2024). However, subsidies may also lead to short-lived employment, particularly in sectors reliant on installation rather than maintenance. For instance, Fabra et al. (2024) find that employment gains in installation-heavy sectors may diminish over time, as maintenance requires less labour. [5]

In our case, most policy-induced jobs were permanent hirings, suggesting sectoral consolidation, with micro-enterprises (fewer than 10 workers) benefiting despite the policy's administrative burden. However, wages remained flat, contradicting the assumption of labour shortages. This suggests that part of the additional investment was captured by firms as higher margins rather than increased labour demand.

Finally, our findings contribute to a debate on the overall effectiveness of energy retrofit policies. While some studies highlight lower-than-expected energy savings from retrofits (Davis, Fuchs and Gertler 2014, Liang, et al. 2018, Fowlie, Greenstone and Wolfram 2018, Lang and Lanz 2022), others emphasize co-benefits, including comfort gains (Aydin, Kok and Brounen 2017), public health improvements (Howden-Chapman 2007), and economic redistribution from high-income to low-income households (Darmais, Glachant and Kahn 2022). Our study adds to this literature by demonstrating that energy retrofit programmes also have employment benefits, though more modest than often assumed.

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 and their robustness. We provide a heterogeneity analysis (e.g. by contract type, firm size, and region)

[4] 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.

[5] 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).

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.[6] Each period-specific national energy savings target breaks 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 one cumulative kWh of saved energy, corresponding to a 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.[7] In addition, since 2016, a share of certificates must be obtained from subsidizing renovation efforts from 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

[6] 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.

[7] 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.

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 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).[8] They correspond to the average price of all the certificates sold during a month.[9] These indices are used as a signal by businesses, who may monitor their activities and make decisions under the scheme based on

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

[9] 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.

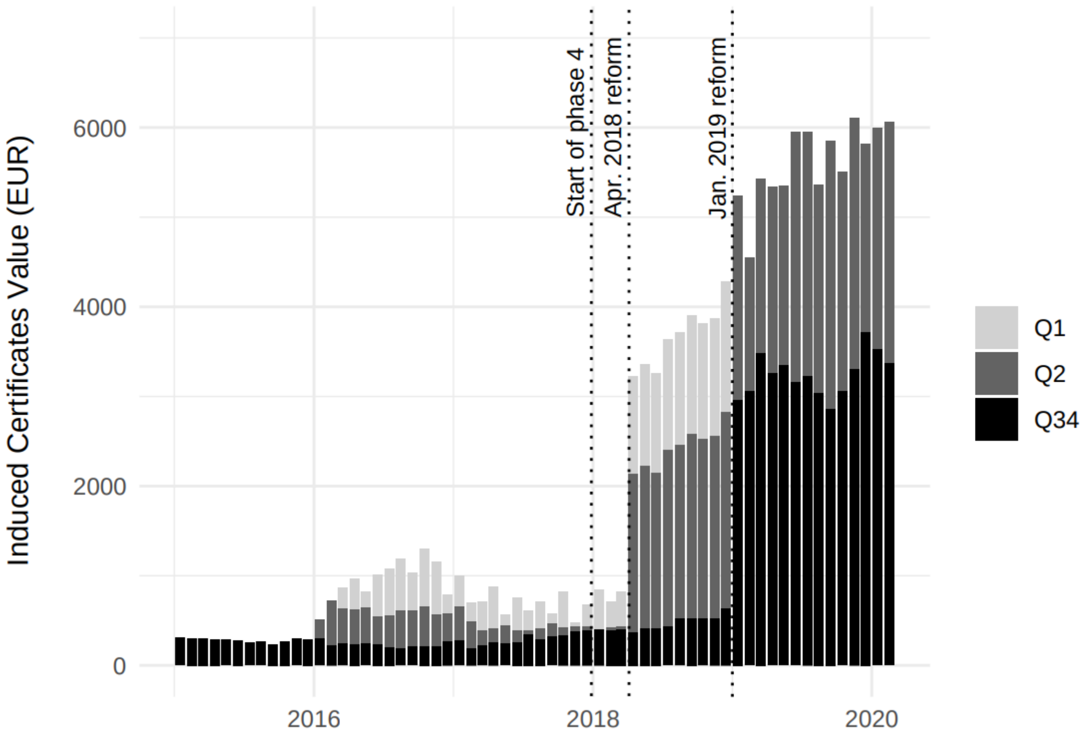
the evolution of these indices. Even though obligated and delegated parties freely set the financial conditions for the home improvements that they subsidize, energy efficiency grants to households ultimately depend on the number of certificates associated with each energy retrofit operation, and the 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.

In 2018, the value of the certificates delivered for many operations started to rise 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).

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, first in April 2018 and then 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, was nearly twice as ambitious as the total obligation of 1,166 cumulative TWh for the previous period (2014-2017). In addition, the government was concerned that obligated actors primarily targeted households that were looking to implement energy retrofits anyway, offering them only limited financial support.

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.

To ensure higher commitment from obligated parties, and higher policy additionality, the French government inflated the number of certificates that it would grant for specific operations if company support to households exceeded set values. The 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 increased 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, overall, the new phase as well as the minimum company support required to obtain more certificates 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".[10]

The WMD collates all employment records from an official document that companies must fill every month, entitled the Nominative Social Declaration (NSD).[11] 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

[10] In the dataset, these are codes 4329A and 4322B respectively.
[11] *Déclaration Sociale Nominative* in French

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

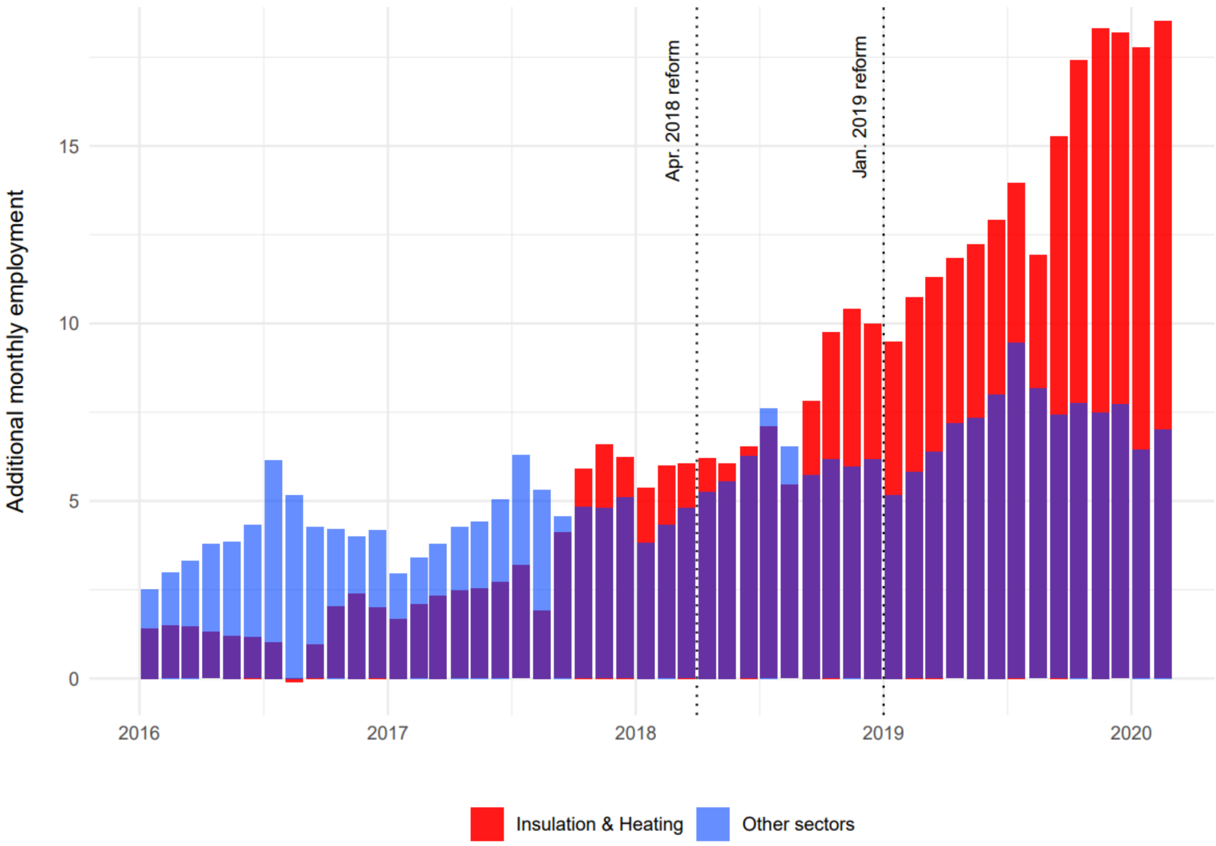
The weighted data can be swiftly used to estimate sectoral employment levels. For instance, in 2016, the retrofitting industry gathered around 100,000 workers, or about 1% of total employment in France.

For this analysis, we are above all interested in the evolution of employment levels over time. Hence, we focus on the part of 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.[12] We then calculate *cumulative employment growth* in each

[12] 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.

sector and region since January 2016 as the sum of all new contracts since January 2016 minus all contract terminations.[13] This variable corresponds to the stock of net job creations since the start of our observation period (January 2016).

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 Labour (2023). National aggregates are computed monthly and rely on the weights developed by the French Ministry of Labour (2018) to account for missing NSD files.

[13] 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.

Cumulative employment growth is displayed in **Figure 3,** jointly for the "insulation" and "installation of heating equipment" sectors, as well as for all other sectors in France. 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 bar is February 2020 since we stop the analysis just before 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 government-support schemes implemented to fight COVID-19.

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" (the treated) with cumulative employment growth in synthetic control groups. We do so at regional level for 13 French regions,[14] 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, for instance 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.[15] In these cases, synthetic controls have several appealing properties compared with other econometric tools commonly used for quasi-experimental policy evaluation

[14] 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.
[15] 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**).

(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.

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.[16] 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 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

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

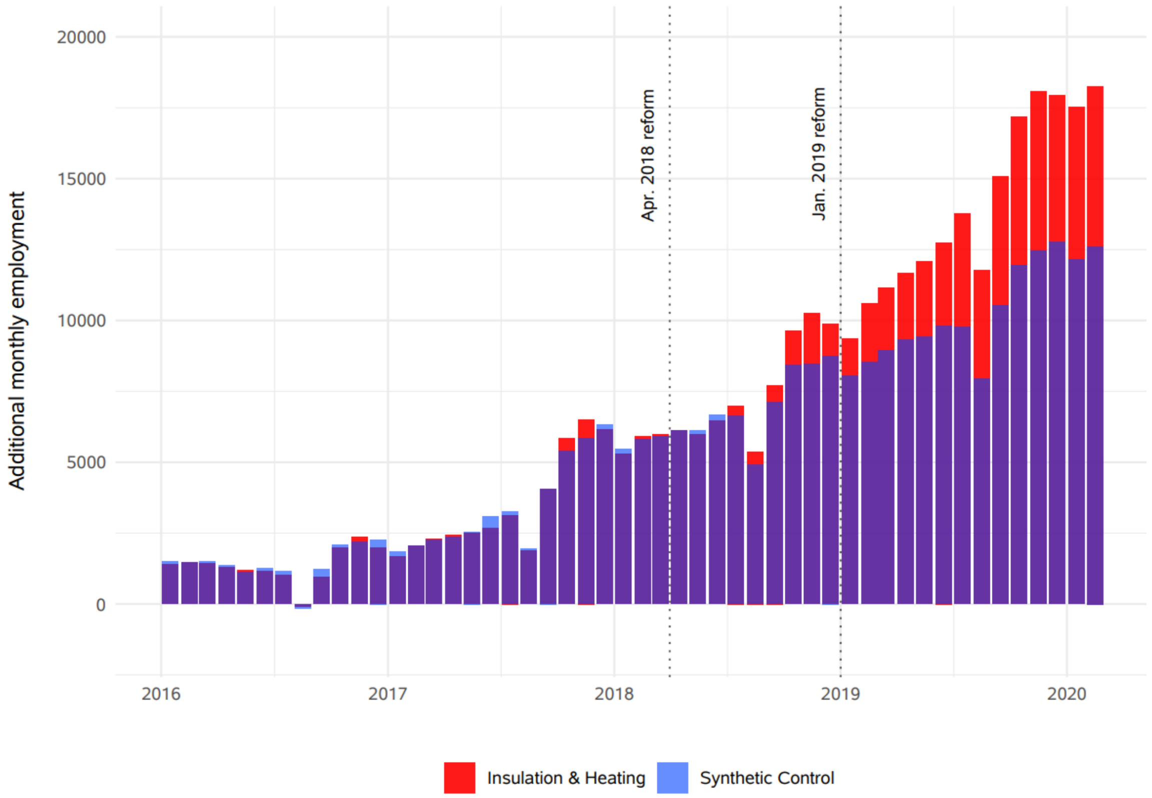
February 2020. Since the first reform was enacted in April 2018, we calibrate the PSC over a 27 month pre-treatment period, from January 2016 to March 2018. The main treatment effect on the treated which we report is the difference between cumulative employment growth and its synthetic counterfactual 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**. Note that 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 an indication about the distribution of the policy's effects on employment across French regions.

Finally, 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 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 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.

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.

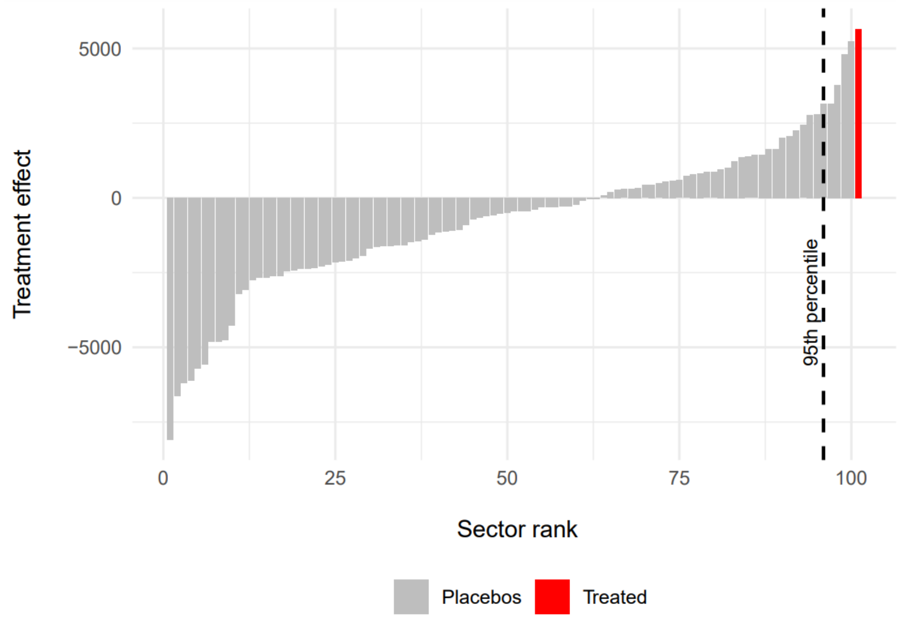
In **Figure 4**, most of the effect of the reforms on employment are recorded after the second reform in January 2019. During the period that follows the first reform (April 2018 to December 2018), we observe barely any effect on cumulative employment growth, suggesting that the first policy changes 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 market value of all the works performed under the EEO scheme was of EUR 78 million, whereas it reached EUR 238 million, more than three times higher, between January 2019 and February 2020.

There is possibly 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**.

Statistical inference. We follow the inferential framework briefly presented in **Section 4**, and detailed in **Appendix B**, 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 first highest against 100 alternative random permutations (each represented by a gray bar and ordered according to the estimated treatment effect). 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.

Robustness. Our main results are robust to considering slightly different dates for the start of the policy. In **Appendix C.1**, we shift the assumed policy start date to January 2019, aligning with the main reform's implementation, as most investment changes occurred after this point rather than in April 2018. Under this specification, we estimate an increase of 3,600 additional jobs (p = 0.01), accounting for 70% of the total effect of 4,900 jobs. This result reinforces our earlier discussion on the relative impact of the two reforms and confirms that most of the employment effect materialized in 2019. In contrast, since the fourth phase of the EEO scheme started in January 2018, it may be useful to advance the start date to January rather than April 2018 (see **Appendix C.2**). Effects remain clearly identified and comparable to our baseline figures with about 4,900 additional workers (p-value of 0.03).

Furthermore, the results are robust to modifying the size of the donor pool for the control sectors (**Appendix C.3**). Specifically, we re-run our baseline estimation (treatment start: April 2018) using two alternative donor pools: sectors where the national headcount in December

2017 was within ±25% (336 donors) and ±50% (659 donors) of the retrofitting industry's headcount. Under the narrower donor pool, we estimate an increase of 4,850 additional workers ($p = 0.02$), while the wider donor pool yields 4,950 additional workers ($p = 0.01$). These results confirm that our findings are robust to donor pool selection.

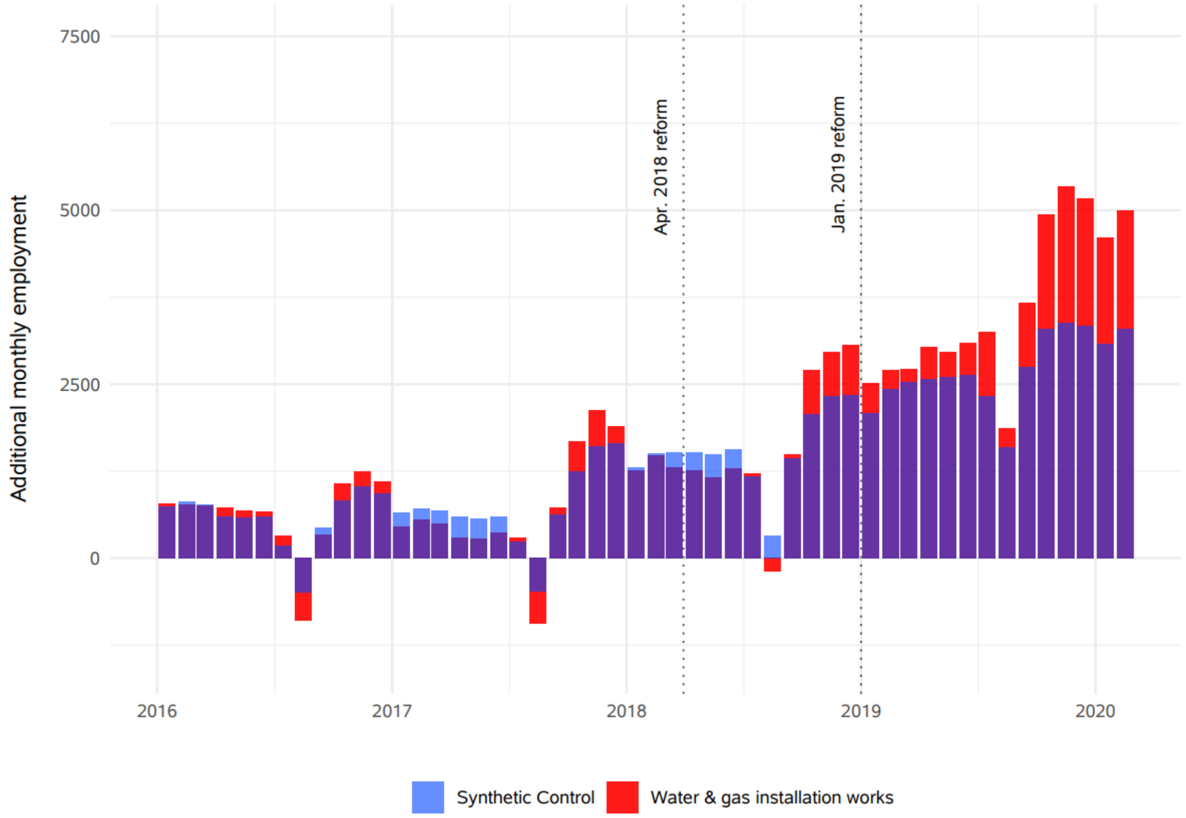
Most importantly, we assess whether the stable unit treatment value assumption (SUTVA) is likely to hold. This assumption requires that the treatment does not affect the outcomes of control units. To examine this, we first analyze the composition of the donor pool used in our penalized synthetic control estimator (see **Appendix D.1**). Aggregating weights at the national level, we find that ten control sectors account for more than 75 percent of the synthetic control. The top three contributors ("Activities of holding companies," "Maintenance and repair of light motor vehicles," and "Chartering and transportation organization") together make up 44 percent of the total weight. The remaining weights are distributed across a diverse set of sectors. Importantly, none of these sectors were likely to be directly or indirectly affected by the policy, which further supports the credibility of the control group. This diversity, combined with relatively low individual sector weights, reduces the risk that our results are driven by spillovers or unobserved shocks in any single donor sector. In short, the dispersion of weights across unaffected and unrelated sectors supports the plausibility of SUTVA in this setting.

In addition, we check that our estimation is robust to potential spillover effects from sectors supplying workers to the retrofitting industry (see **Appendix D.2**). SUTVA could be violated if job creation in the treated sector leads to job loss in the sectors of origin. To address this concern, we identify the sectors of origin for all workers newly hired in the treated sector between April 2018 and February 2020, defining origin as each worker's last sector of employment prior to joining the retrofitting industry. We aggregate these sectors of origin at the broader section level (21 sections) and exclude from our donor pool the top 5 sections, which together account for 62.5% of new hires in the treated sector (see **Figure 16** in Appendix). Re-estimating our baseline model on this restricted donor pool yields an effect of approximately 4,900 additional workers (p-value = 0.01; see **Figure 17** in Appendix). This finding reinforces our conclusion that SUTVA is not violated and that our main results are not driven by unobserved spillover effects originating from sectors included in the donor pool.

Finally, our baseline estimation only considers “insulation” (4329A) and “installation of heating equipment” (4322B) as treated sectors. However, firms in related construction sectors could also 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 toward retrofitting activities.

So far, we have simply excluded construction sectors from the donor pool. We therefore look at job creation from those sectors. We focus on the sectors with the largest numbers of firms obtaining the “Recognized Environmental Guarantor” 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. This would be a necessary step to benefit from the policy. In our dataset, there are 10 sectors for which at least 10,000 firms were obtaining the RGE label every month (see **Figure 18** in Appendix). We ran our synthetic control model on each of these sectors. We find no impact of the policy on employment, apart possibly for companies involved in “Water and gas installation works”. We report those results on **Figure 6**. With a p-value of 0.08, we find a positive effect of 1,150 job-years for this sector.

Figure 6: 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 0.08.

6. Heterogeneity analysis

To better understand how the policy led to job creation, we explore the heterogeneity of employment effects across key dimensions such as contract types, firm sizes, and regions.

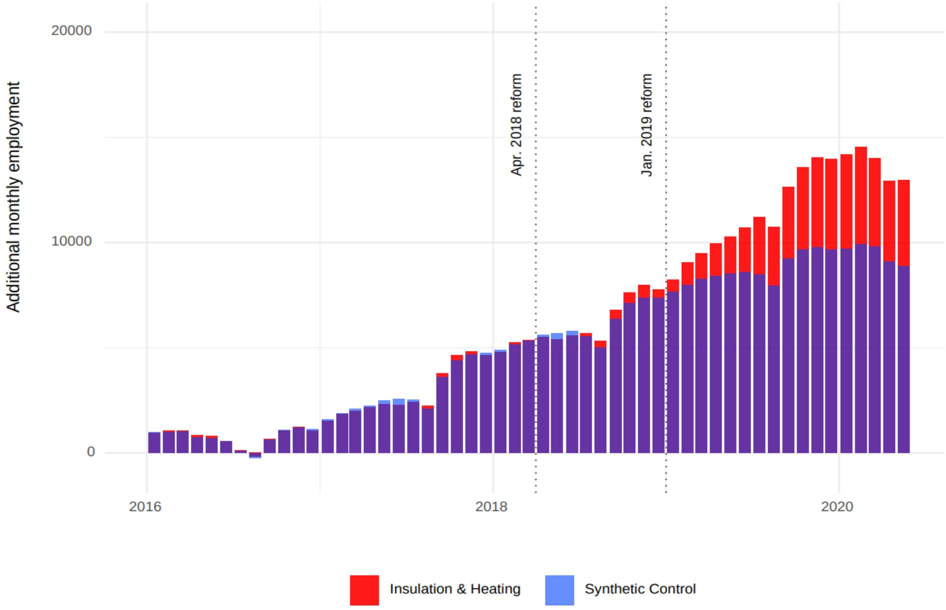
Contract duration. Using information on the contract offered to new recruits, we can further assess whether the reforms led to long-term job creation. In France, social protection laws imply that employers are often very reluctant to offer permanent positions because firing people can be very costly. 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) (Article R1234-2, French Labor Code). French employers only offer permanent contracts when they think that the activity will be sustained for several years.

In **Figure 7**, we provide the national-level results after running the synthetic control model by contract type. 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 0.01). For fixed-term contracts, we find that 1,750 jobs were created (p-value of 0.05).

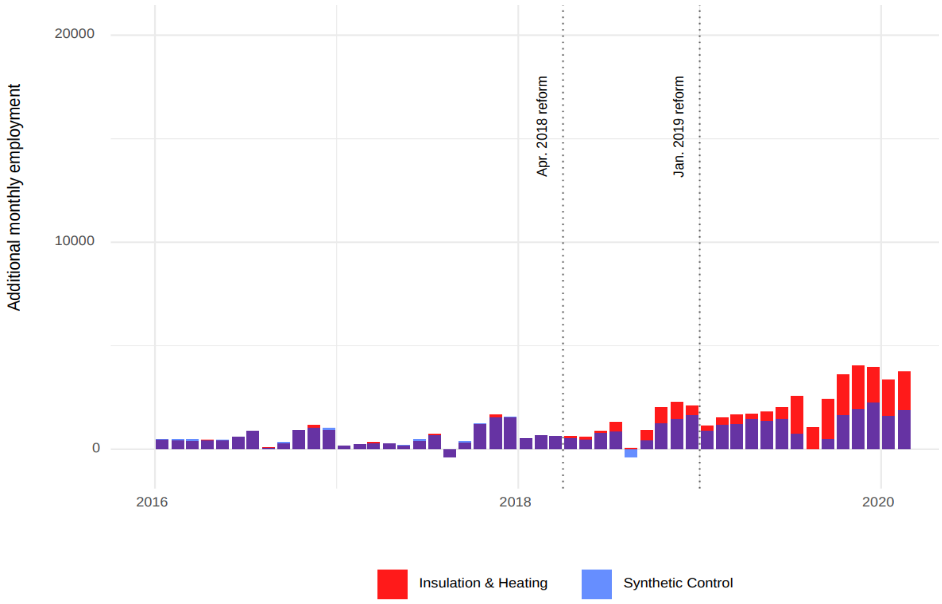
Impact on the unemployed. Our analysis leverages sector-level variation to document a causal relationship between investment and employment growth in the energy efficiency industry. However, an important question is whether these newly created positions primarily benefited workers with low employability or instead improved opportunities mainly for those who were already employed or who had experienced only short unemployment spells. To investigate this, we distinguished new employment contracts that followed a period of at least one month of unemployment (indicating lower employability) from those that did not. Replicating our analysis using only contracts signed after sustained unemployment periods yielded an estimated effect of about 750 additional job-years for previously unemployed individuals (p-value = 0.02) (see **Figure 8**). Considering that the unemployed are very unlikely to be given permanent contracts firsthand, it is useful to compare this figure with the total number of fixed-term contracts created (at 1,750). Therefore, the unemployed may have filled around 40% of all temporary positions created. These findings suggest that while the policy provided employment opportunities for a non-negligible share of workers with lower employability, most newly created positions benefited individuals who were already employed or had only very short unemployment durations.[17]

[17] This does not indicate displacement or job destruction elsewhere. It is entirely possible (and indeed common) that these workers transitioned voluntarily from other sectors to pursue improved wages, working conditions, or career opportunities in the expanding energy efficiency industry.

Figure 7: Employment growth in energy renovation vs its penalized synthetic control by contract type



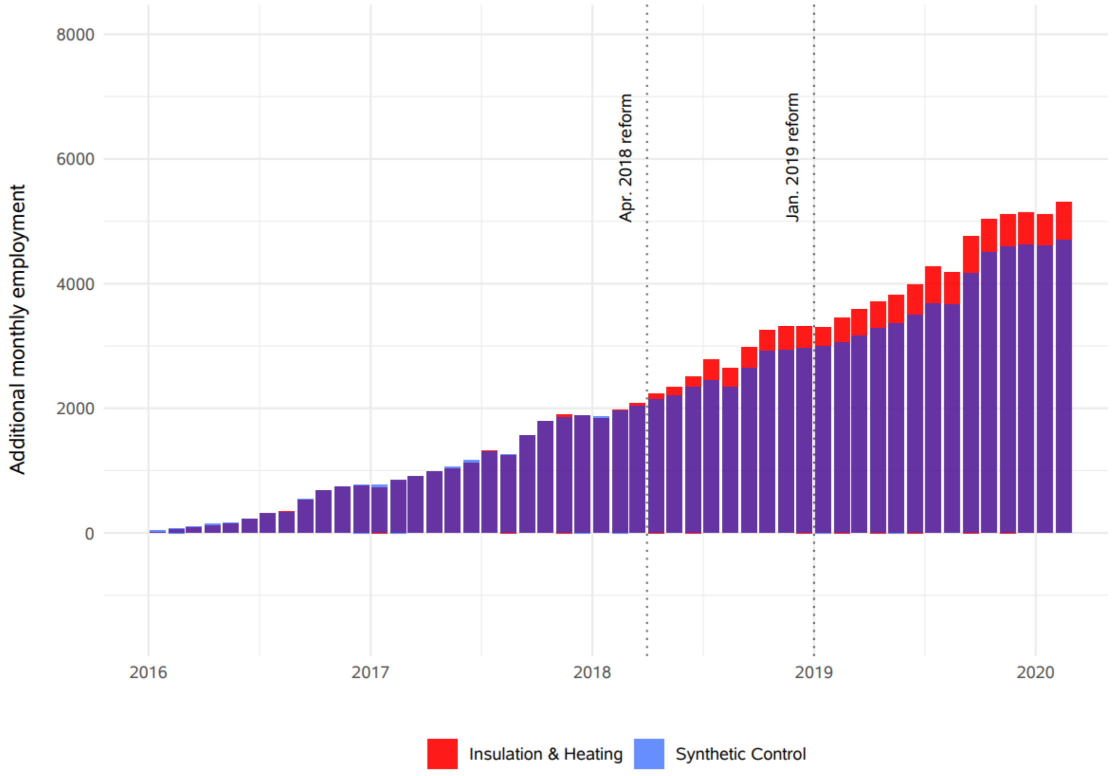
(a) Permanent contracts



(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).

Figure 8: Exits from unemployment vs its penalized synthetic control



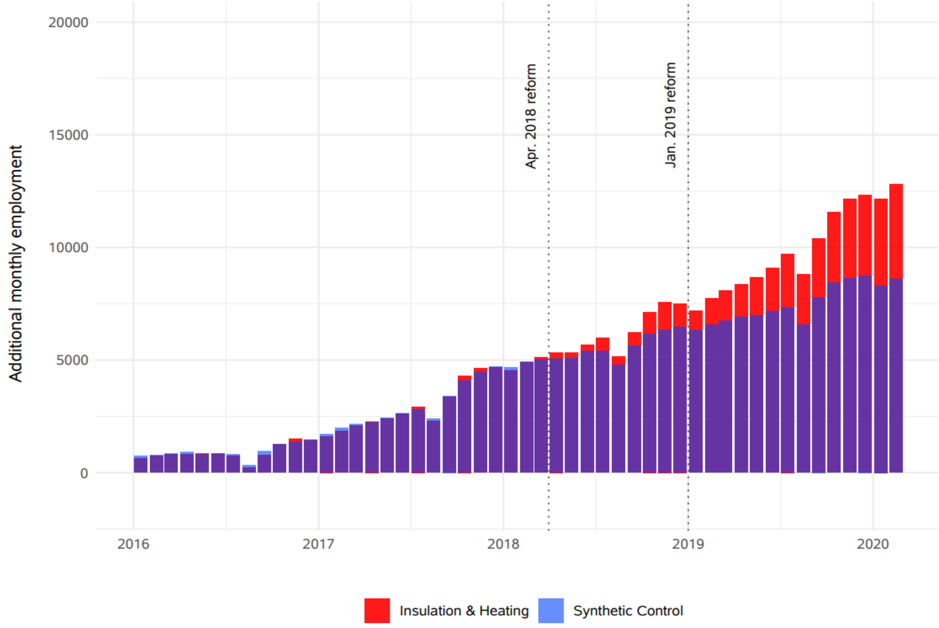
Notes: The donor pool is based on the ±33% interval around the national cumulative employment growth of unemployment-exiters in the retrofitting industry as of Dec. 2017. The pre-treatment period includes all months from Jan. 2016 to Mar. 2018. The treatment period follows, from Apr. 2018 onwards. $\lambda^*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

Impacts by firm size. We investigate how job creation varies across firms of different initial sizes. Firm size could influence a company's ability to respond effectively to increased demand and its capacity to manage the administrative requirements associated with Energy Efficiency Obligations (EEOs). Since the paperwork required for compliance with EEOs can be considerable, larger firms with specialized administrative staff may have been better positioned to benefit from the policy. This question is central to policy discussions, as internal growth and scaling up retrofitting activities within large firms are often viewed as necessary initial steps towards industrializing the sector. On the other hand, smaller firms may hold an advantage due to closer connections with their customer base and greater sensitivity to reputational factors, which are particularly important in credence goods markets such as energy efficiency services, where consumer trust is essential.

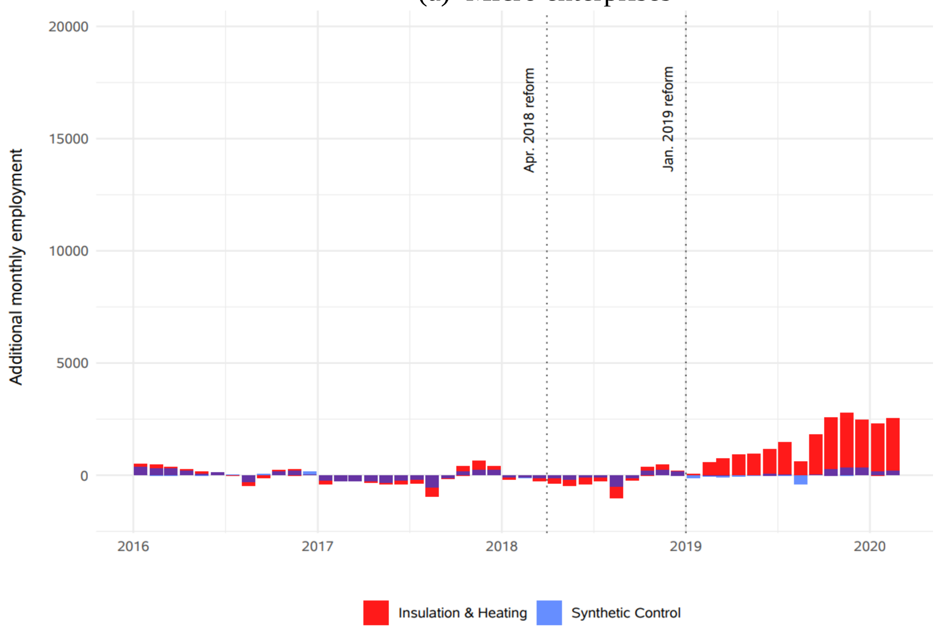
In **Figure 9**, we report the aggregate results after running the synthetic control model by firm size (see panel (a) for micro-enterprises below 10 workers, and (b) for small enterprises between 10 and 49 workers). We find that about 3,300 job-years were created within micro-enterprises, amounting to around 70% of the estimated effect of the EEO reforms on total employment (p-value of inference test is 0.01). For 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 might conclude that the increase in EEO funding primarily benefited micro-firms strongly anchored in local markets. This outcome may reflect the nature of energy efficiency services as credence goods, for which customer trust and installer accountability are critical. However, it is also important to contextualize these results by comparing them to job creation trends observed in the synthetic control group. Panel (b) shows that, absent the policy, job creation among smaller firms is nearly nonexistent. Consequently, the policy appears to have also supported the expansion of larger firms operating on a broader geographical scale.

Figure 9: Employment growth in energy renovation vs its penalized synthetic control by initial firm size



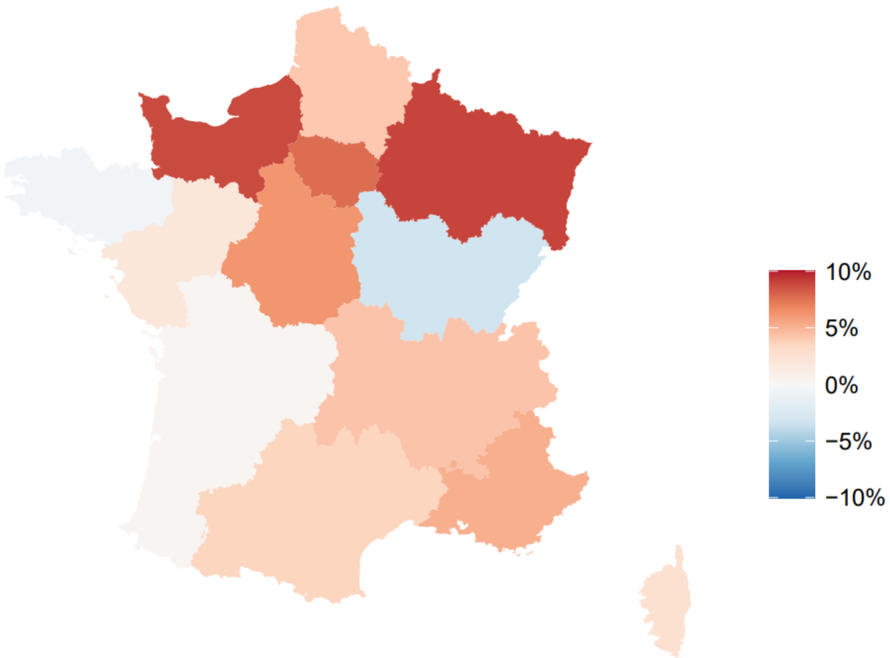
(a) Micro enterprises



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).

Regional estimates. An innovative feature of the synthetic control method used in this paper is that it allows us to estimate separate effects at the regional level. **Figure 10** presents our regional estimates of the policy's impact on total employment in the energy retrofitting sector. Our results indicate that northern regions particularly benefited from increased investment driven by the EEOs. Nationally, policy-induced employment between April 2018 and January 2020 corresponds to roughly 5% of the sector's total employment level in 2016. Regionally, this share reaches as high as 8.8% in Normandy and 9% in Grand Est. For other regions, estimated effects (both positive and negative) remain modest, below 3%, reflecting limited impacts.

Figure 10: Effect of the policy on regional employment (% of Jan. 2016 total employment)

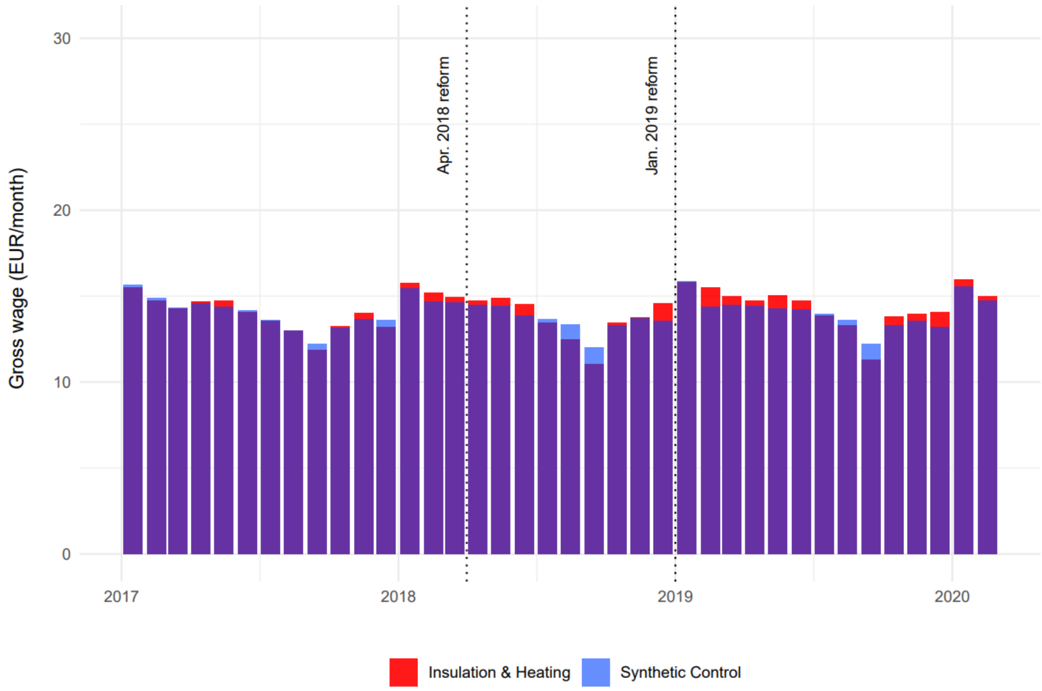


Notes: Baseline estimation ($\lambda^* = 0.01$, p-value 0.01). Percent changes are reported in relative terms with respect to the average employment level in the retrofitting industry in January 2016.

Hourly wages. A possible explanation for the modest employment effect observed could be a shortage of suitable candidates available in the labour market. If this were the case, we would expect the wages of newly hired workers to rise, reflecting increased competition for scarce labour. To test this hypothesis, we make use of an additional dataset from the French

Ministry of Labour (DARES, Ministère du Travail 2025) which records hourly wages annually at the worker-establishment level. We calculate average hourly wages for newly hired workers for each sector and region on a monthly basis from January 2017 to December 2020. We then follow our baseline estimation strategy to analyse the effect of the policy on hourly wages. Although the pre-treatment period for this analysis is shorter than in our baseline specification, it still includes 15 months of variation. Using the same donor pool as in the baseline estimation, we find no significant effect of the policy on hourly wages (See **Figure 11**). This result suggests that labour shortages did not constitute a major constraint on employment growth.

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.

7. Conclusion

In this paper, we leverage a discontinuity in the French Energy Efficiency Obligations (EEOs) scheme to provide the first ex-post causal evidence on the employment impacts of large-scale energy retrofit policies. Using a penalised synthetic control method, we estimate a significant but modest employment effect, finding that approximately 1.5 direct jobs were sustained for each additional million euros in subsidies granted to beneficiary households.

Our estimates stand in stark contrast with existing ex-ante assessments commonly referenced in European policymaking. Ex-ante studies reviewed by BPIE (2020) suggest that energy renovations could create between 4.3 and 9.2 direct jobs per million euros invested. Similarly, the European Commission has previously employed a multiplier of 8.5 full-time-equivalent jobs per million euros based on earlier literature (Janssen and Staniaszek, 2012; Cuq et al., 2011). The substantial discrepancy we document indicates that ex-ante models may significantly overestimate job creation, and thus, calls for a downward revision of these figures in policy impact assessments.

Our findings align with recent ex-post analyses from other contexts. Popp et al. (2021) estimated job multipliers between 2 and 4 per million dollars in the US construction sector, while Fabra et al. (2024) found a multiplier of approximately 0.65 jobs per million euros in Spain’s solar industry. These similar outcomes further support the conclusion that widely used ex-ante job creation multipliers are overly optimistic.

Our heterogeneity analysis sheds additional light on employment dynamics. The policy primarily benefited micro-enterprises with strong local connections, which is consistent with the credence-good nature of energy efficiency services requiring consumer trust and installer reliability. Nonetheless, larger firms also benefited, aligning with broader policy objectives aimed at scaling up the sector. Geographically, colder, wealthier, and more populous regions saw proportionally larger employment gains, suggesting that spatial factors could influence the effectiveness of similar energy retrofit programmes.

The modest employment effects could partially be explained by labour market constraints or changes in employers’ expectations due to sustained policy commitments. However, we find no evidence of significant upward pressure on hourly wages, suggesting limited labour scarcity. The stability provided by increased funding and reduced regulatory uncertainty may have supported employment growth, especially in more stable, open-ended contracts, consistent with evidence from broader employment literature (Schaal, 2017).

Several questions remain open for future research. Investigating interim employment effects, distinguishing between the internal growth of firms and new business creation, and examining how firms' profits and investment behaviours respond to the subsidies would yield deeper insights into the sector's economic dynamics. Additionally, exploring the pass-through of policy costs to residential energy prices could clarify the role consumer price sensitivity plays in driving energy efficiency investments.

Overall, our findings suggest a cautious reassessment of the employment co-benefits associated with energy retrofit policies. While these initiatives do support job creation, the scale appears significantly smaller than currently anticipated by policymakers. A realistic understanding of these employment effects is crucial for accurately evaluating the costs and benefits of green investments.

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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 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 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 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 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 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:

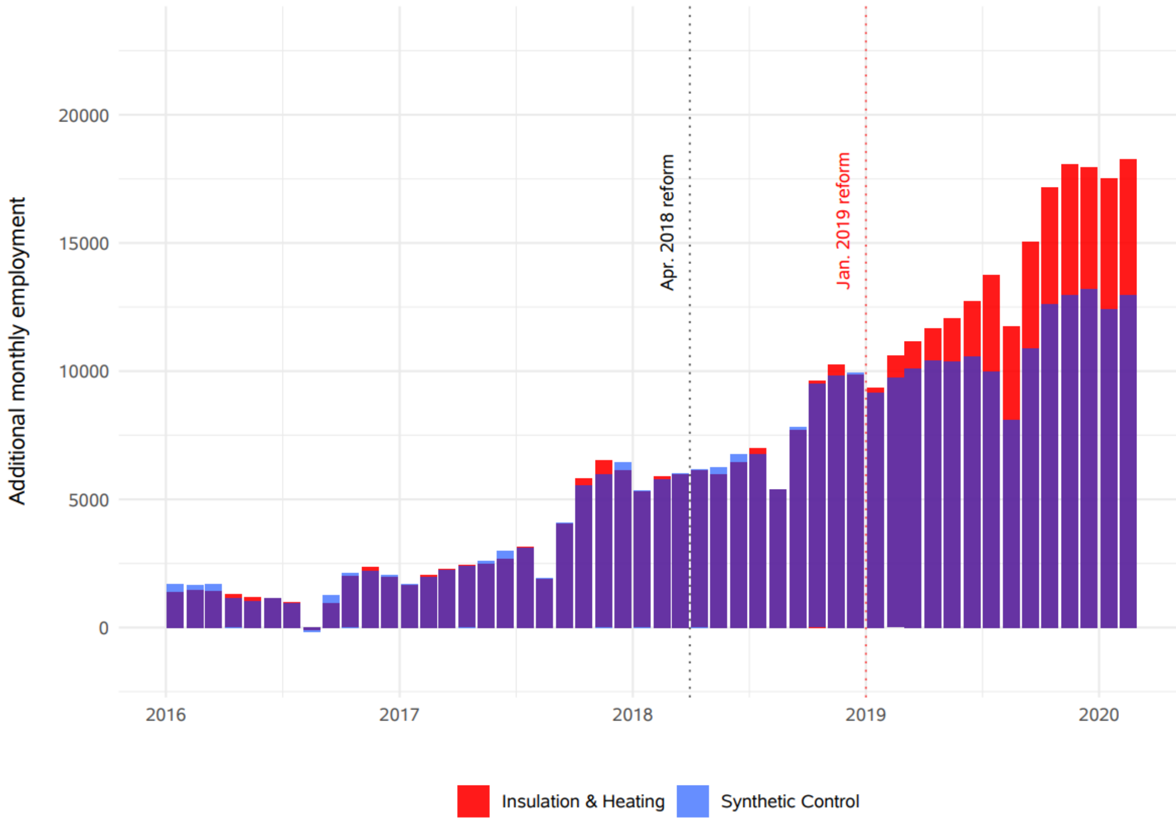
$$\hat{p} = \frac{1}{100+1}\left(1 + \sum_{b=1}^{B=100} \mathbf{1}\left\{\hat{T}\left(\boldsymbol{D^{(b)}}, \lambda^*\right) \geq \hat{T}\left(\boldsymbol{D^{obs}}, \lambda^*\right)\right\}\right) \tag{6}$$

C. Sensitiveness analysis

C.1. Later starting 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

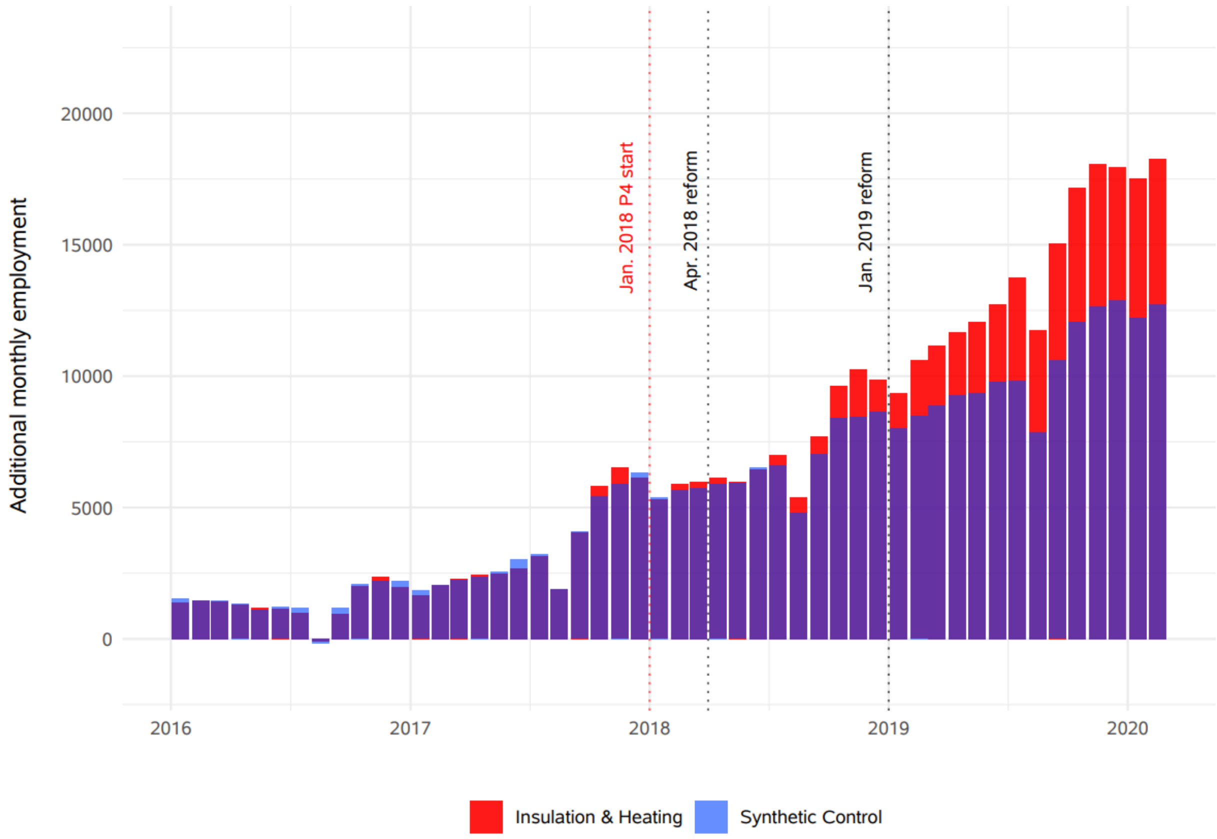


Notes: The pre-treatment period includes all months from Jan. 2016 to Dec. 2018. The treatment period follows, from Jan. 2019 onwards. $\lambda^* = 0$. The red bars represent the evolution of the workforce in the energy renovation sector, the blue ones display the evolution for its synthetic control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

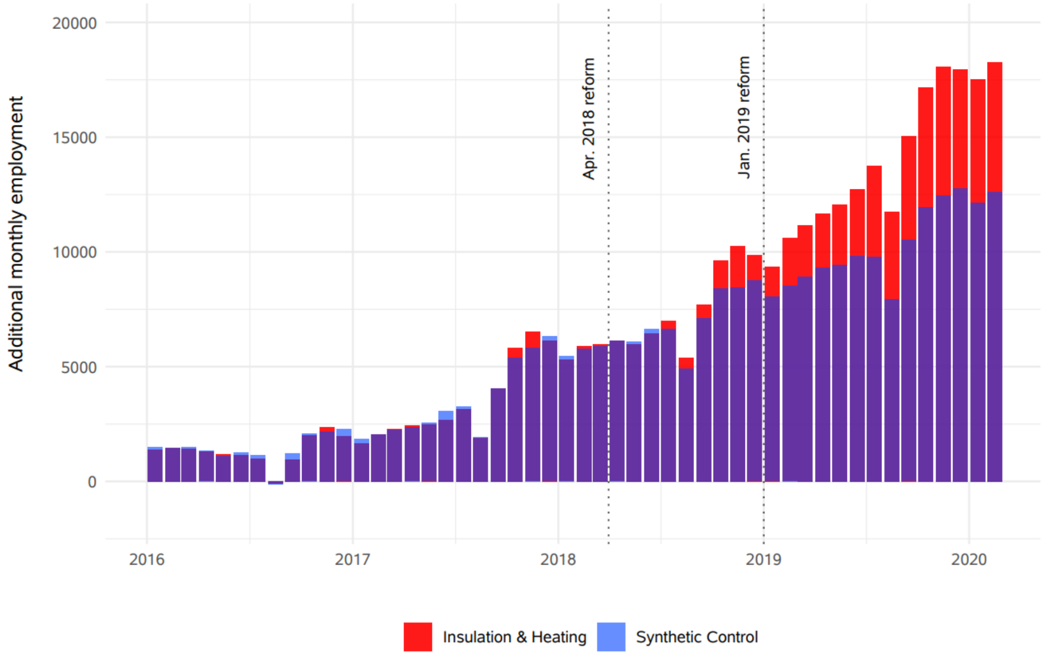
C.3. Alternative pools of control sectors

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).

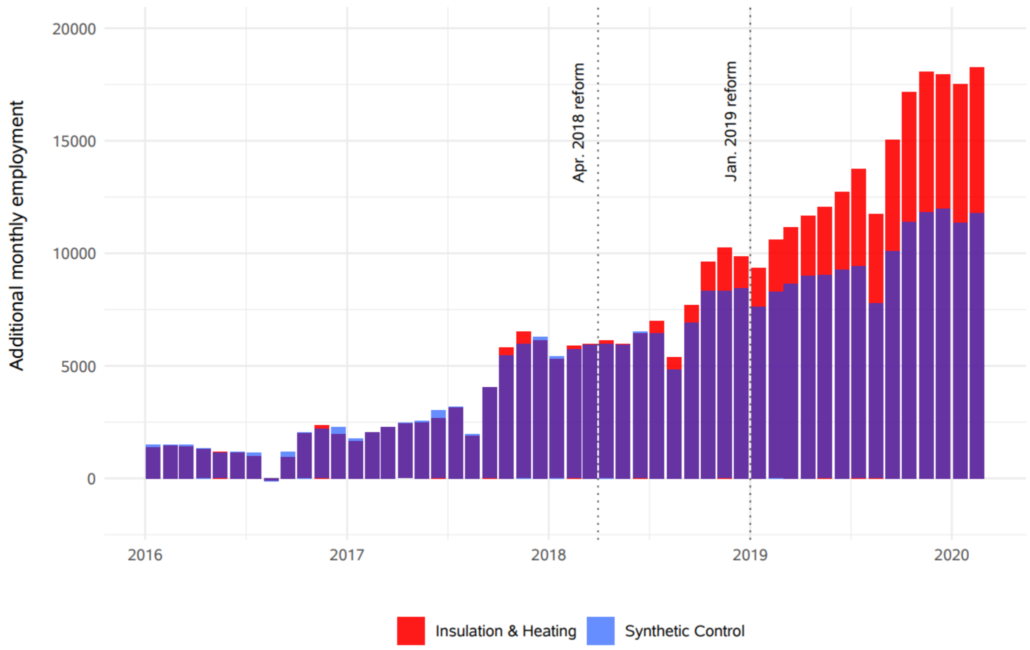
Our baseline donor pool included all regional sectors which national headcount was comprised within a ±33% interval around this threshold. This yielded 427 different control sectors. Below, we perform two robustness checks for this selection rule, with a ±25% and ±50% intervals, leaving us with 336 and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

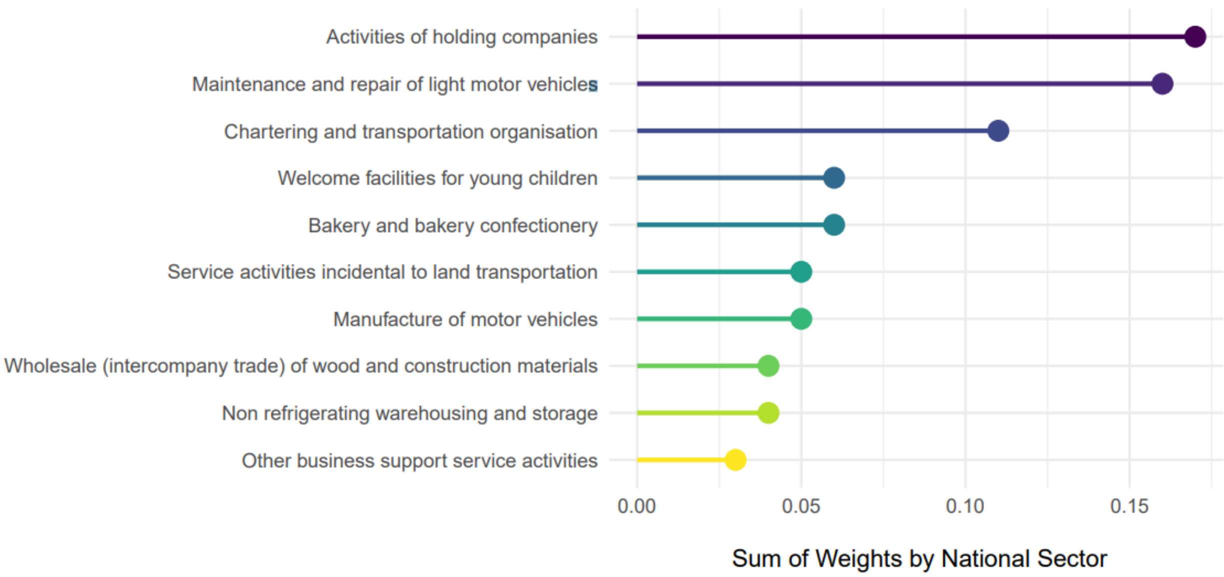
Notes: Estimation is similar to the baseline, except for the rule used to restrict the pool of control sectors.

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

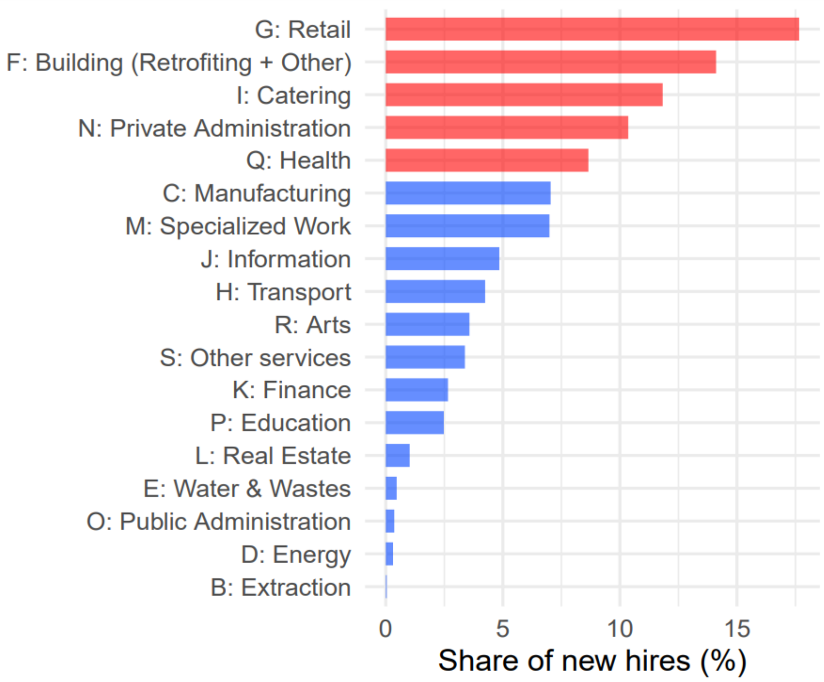
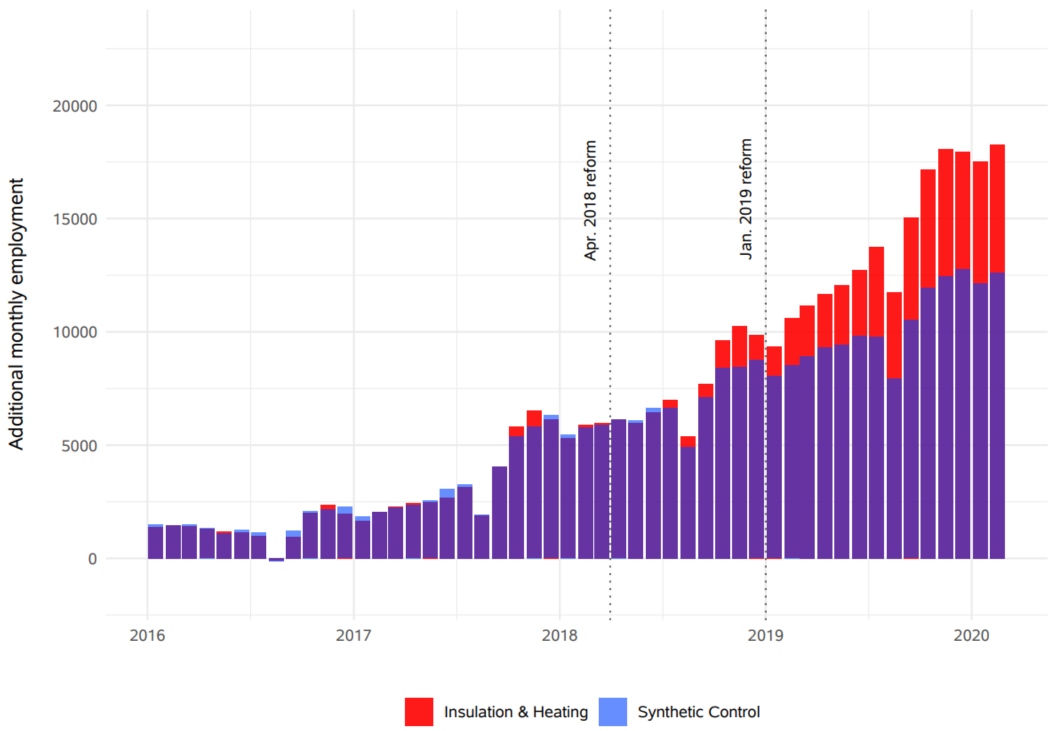


Figure 17: Estimation on a restricted donor pool (SUTVA)



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