The effects of the 2021 energy crisis on medium-sized and large industrial firms: evidence from Italy

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Abstract

Using survey data from Italy, we study the effects of the 2021 energy crisis on the energy input choices of medium and large-sized industrial firms. Our instrumental variable (IV) strategy, based on the availability of fixed-price contracts subscribed before the crisis, reveals an average infra-annual price elasticity of demand very close to zero for both electricity and natural gas. Large energy consumers subject to the *European Emission Trading System* (EU ETS) have significantly larger natural gas elasticities and were able to partially substitute gas with other fossil fuels. Surprisingly however, their elasticity to electricity prices is similar to that of other firms. We finally show that in 2021 energy-intensive and EU ETS firms increased their final prices more than other firms, but this differential effect was mitigated by the presence of fixed-price contracts. Our evidence is valid for 2021 but does not necessarily extend to 2022.

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1 Introduction[1]

In 2021 world demand for natural gas rose rapidly due to a swift post-Covid-19 recovery. Yet, lower than expected nuclear, wind and hydro-electric production, prevented the supply side from completely meeting this rapid increase in demand, especially in the European Union (EU). Beginning in the second half of 2021, the matter worsened when the rise in geopolitical tensions caused a major decrease in Russian exports of gas to the EU. As a result of all of these factors, the wholesale price of gas in the EU rose from around 30 euros per megawatt hour in June 2021 to more than 100 in December 2021 (OECD, 2022), a level never seen before. Due to the heavy reliance on gas for electricity production, gas price surges were also transmitted to the wholesale price of electricity. After Russia invaded Ukraine in February 2022, the energy crisis significantly aggravated.

There is great concern that this unprecedented rise in natural gas and electricity prices could deeply hurt firms and EU economies more broadly. In a recent and widely discussed policy paper, Baqaee et al. (2022) argue that the economic losses coming from a complete and immediate halt of energy imports from Russia (which would trigger even higher price increases) would be small, around 0.3 percent of GDP, with some heterogeneity across EU countries.[2] The moderate size of these effects crucially relies (among other things) on calibrated price-elasticity of energy or gas demand.[3] Despite its centrality in the policy debate, there is little evidence to guide the choice of this crucial parameter in the current context. While there are many estimates, thoroughly reviewed in Labandeira et al. (2017) and used in macro studies, many of these precede year 2008 and most likely do not reflect technical substitution possibilities. Also, the fact that *some* substitution of natural gas and electricity is possible in normal times says nothing about substitution *in the limit*, when prices rise to unprecedented levels (Geerolf, 2022).[4] Furthermore, even in current meta-analyses, all available elasticity estimates rely on yearly variations in prices, at least. Not much is known about this parameter at shorter time horizons (below one year) where adjustment costs to

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[2]All EU countries display effects lower than 1 percent of GDP, with the exception of Slovakia, Bulgaria (both between 2 and 3 percent) and Lithuania (around 5 percent).

[3]This class of models employs CES technology, whereby the own-price elasticity of natural gas demand corresponds to the elasticity of substitution between gas and other inputs.

[4]There is some anecdotal evidence that German firms may be substituting gas with other fuels in response to the *current* energy crisis. Benjamin Moll has collected these in a Twitter thread that can be accessed at the top of his Twitter page: `https://twitter.com/ben_moll`.

an energy shock could be substantial. Getting this parameter right is of crucial importance, because small variations in plausible values for these elasticities can yield big changes in the GDP loss estimates, ranging from 2 to 15 percent (Geerolf, 2022).

We contribute to this academic and policy debate by estimating *infra annual* price elasticities of both electricity and natural gas among industrial firms during the 2021 energy crisis. The analysis relies on Bank of Italy survey data on medium-sized and large industrial firms (more than 50 employees). The survey data allow us to gather firm-level information on expenditures and physical quantity use of both electricity and natural gas, separately for the first and second semester of 2021. Expenditures in euros and physical quantities in KWh or scm[5] allow us to compute bi-annual average unit prices for both inputs at the firm-level, which give us firm-level idiosyncratic *changes* in energy prices around the onset of the energy crisis.

As usual, the identification of demand elasticities requires *supply* shifters that generate exogenous variation in prices. One could argue that the energy crisis is itself an aggregate supply shifter, and that ensuing price changes come from exogenous (to firms) *supply* conditions in wholesale markets. Nevertheless, simple OLS estimates from regressing firm-level changes in energy quantities on firm-level changes in energy prices could still be plagued by reverse causality. Indeed, fixed charges in energy bills (e.g. transportation fees) make it such that decreases in quantities demanded lead to higher per-unit energy prices.

In order to address these identification concerns, we exploit survey information on whether, *before* the crisis, firms already had in place fixed-price contracts or other types of financial instruments that shielded them, at least partially, from the unexpected price increases that occurred in the second part of the same year.

The ideal experiment envisages two firms, each subscribing an identical 12-month fixed-price contract, the only difference being the subscription date. The first firm subscribes in September 2020, while the second in March 2021. These firms would be exposed to the shock to very different degrees because they subscribed their previous contracts at slightly different dates. From the survey we are only able to gather information on whether the firm had such contract or not, so our IV may also capture differential propensity to insure against price risk, which may be related to observable and unobservable characteristics and confound the effect. In order mitigate this concern, we always run our regressions in first differences, using a combination of difference-in-differences and instrumental variables (DiD-IV design). This allows us to net out unobservable but fixed-over-time confounders. We also check for

[5]Kilowatt-hour and standard cubic meter, respectively.

the sensitivity of our results to the inclusion of several firm characteristics, which allow differential trends according to observable covariates.

We find that our IV is strongly predictive of firm-level infra-annual price *changes* in 2021 along the entire distribution. Our research design allow us to estimate a local average treatment effect (LATE) for complier firms that faced lower price increases during the crisis because of fixed-price contracts. In this setting, LATE is a weighted average of firm-specific elasticities with weights that positively depend on how strong is the price change induced by the instrument for that particular firm (Angrist et al., 2000; Angrist and Pischke, 2009).

In order to investigate heterogeneity of the effects, to validate our survey evidence and study more outcomes, whenever possible we match the survey data with confidential administrative records on consumption of different fossil fuels (from the Institute for Environmental Protection and Research – ISPRA) and electricity use (from the Fund for Energy and Environmental Services – CSEA). The fossil fuel and electricity consumption data are only available, respectively, for firms that own industrial plants subject to the *European Emissions Trading System* (EU ETS firms) and firms receiving energy subsidies by the Italian government because of their high electricity intensity (*energivore* firms).

As a first step, we document four key facts: first, during the crisis, average changes in firm-level gas and electricity prices have been substantially lower than corresponding wholesale price changes, also because the retail price does not immediately adjust in presence of fixed-price contracts. Second, energy price changes are very heterogeneous across firms, a fact that is partly due to differences in contractual arrangements (e.g., variable-price vs. fixed-price contracts). Third, before the crisis the share of gas and electricity expenditures in revenues followed a skewed distribution. Even after the start of the crisis, the incidence of energy costs has remained moderate for the majority of firms. Fourth, aggregate industrial energy consumption did not fall in the second half of 2021, net of seasonal factors.

Turning to the causal results, our elasticity estimates indicate very limited average responsiveness to price changes by complier firms: infra-annual price-elasticity point estimates are extremely close to zero for both electricity and natural gas. Since the relevant confidence intervals rule out magnitudes larger than -0.2 with a 5 percent confidence level, these are "precisely estimated zeros". These infra annual elasticities are at the lower end of what it has been previously estimated in the literature with yearly panels, especially for the Italian case.[6]

[6] For the Italian case, using sectoral data and an OLS estimator, Faiella et al. (2021) report a yearly energy elasticity of -0.4 for industrial firms with more than 50 employees. Bardazzi et al. (2015) uses a short panel (2000-2005) of Italian firm data (from Istat) and a SUR to estimate Morishima elasticities of -0.4 for electricity

While average effects for complier firms are close to zero, EU ETS firms have natural gas price elasticities that are much larger, around -0.8. This could be due to the fact that gas is an important input for this type of firms and also that the average price change for natural gas is larger for EU ETS than for non-EU ETS firms. Quite surprisingly though, these firms display very small electricity price elasticities, in the same range as the average complier firm. Thanks to administrative data on several energy input choices for EU ETS firms, we further document that these companies were able to partially substitute the energy content of natural gas with other fossil fuels.[7] Overall, our evidence is consistent with an overall reduction in energy use (measured in Terajoules) by ETS firms exposed to higher prices. Although this is a small subset of companies (around 381 in Italy – 65 of which in our survey sample), it accounts for more than half of aggregate industrial consumption of natural gas and naturally constitutes an important subset of firms to focus on when designing policy interventions that aim to reduce gas demand while preserving economic activity. The fact that the gas elasticity can be so different for this set of large gas consumers is informative and cautions against using a unique parameter for the whole industrial sector when calibrating macroeconomic models. Similarly, given the stark difference between electricity and gas elasticities for this set of large energy consumers, models that use just one energy good could lead to misleading results. Next, we estimate the electricity elasticity for firms with the highest electricity intensity in Italy (so called *energivore*), using both our survey and administrative data. For this sub-sample, we find that the elasticity is quite small exactly as in the rest of the sample.

Finally, we study the consequences of rising energy prices on firms' price setting behaviour. Our estimates suggest that, amid a generalized increase in the price of final goods, those energy-intensive firms that experienced large cost increases raised their prices by a greater amount. However, this differential effect is absent for those energy-intensive firms experiencing milder cost increases due to their insurance coverage.

We contribute to the literature in three ways. First, we provide the first firm-level evidence on the impact of the ongoing energy crisis on input and pricing choices by industrial firms. We do so by proposing a new instrumental variable based on the presence of fixed-price contracts that slow down the transmission of wholesale to retail prices for some firms, but not others. Contrary to previous studies that had to rely aggregate data and time series techniques (Ruhnau et al., 2022), our combination of panel data and IV allow us to observe firm-level price changes within a given year, driven by an instrument that varies at the

and -0.8 for natural gas.

[7]Our data only allow us to look at physical quantities of these inputs and so we cannot infer how costly it was for firms to substitute away from natural gas.

level of firms within sectors and is not confounded by aggregate contemporaneous shocks.[8] Second, we contribute to the literature estimating natural gas and electricity elasticities for firms. While there is more credible evidence for households (Reiss and White, 2005; Jessoe and Rapson, 2014; Auffhammer and Rubin, 2018; Hahn and Metcalfe, 2021), estimates for industrial firms are limited and rely on macroeconomic instrumental variables (see footnote 8). One exception is Marin and Vona (2021) who use a Bartik instrument leveraging firm-level variation in the energy mix and time-series variation in national prices; this strategy allows to identify the overall energy demand elasticity, but not fuel specific ones. Contrary to commercial and residential customers, industrial firms do not use natural gas primarily for heating spaces, but mostly for production purposes. Thus elasticities could be vastly different from those estimated for other sectors of the economy. Third, to the best of our knowledge we are the first to estimate an *infra-annual* price-elasticity of demand for both electricity and natural gas. Due to data limitations, previous studies have only looked at yearly elasticities, if not longer. Our infra-annual electricity estimates for the industrial sector are one order of magnitude smaller than long-run elasticities previously estimated (Csereklyei, 2020). Infra-annual elasticities may be very relevant for policy because they take into account potential adjustment costs that firms face in the very short run.[9] At the same time, our infra-annual elasticities estimated on 2021 data are context and time-specific, and thus should not be used to extrapolate the responses enacted by firms to the further energy price increase occurred in 2022.

The rest of the paper is structured as follows: Section 2 describes the data; Section 3 puts forward four descriptive facts about the energy crisis in 2021; Section 4 presents the identification strategy and the main results; Section 5 focuses on price-setting behaviour by firms, and Section 6 comments on the external validity and concludes.

[8]Previous studies have relied on a number of "macroeconomic" instrumental variables that rely on time-series and cross-country variation: prices paid by the household or industry sector only (Burke and Abayasekara, 2018; Csereklyei, 2020), domestic natural gas reserves and distance weighted reserves in other countries (Burke and Yang, 2016), lagged prices (Graf and Wozabal, 2013), the spot price of Brent crude oil (Davis and Muehlegger, 2010), weather shocks (Hausman and Kellogg, 2015), among others.

[9]Some studies look at the price-elasticity of electricity demand in real time. However this elasticity may reflect time-shifting of consumption across different moments of the day or the week (Lijesen, 2007; Jessoe and Rapson, 2014). In our setting, given the severity of the ongoing crisis, this channel is ruled out.

2 Data and measurement

2.a Description of the data sources

The main data source used in this paper is the *Indagine sulle imprese industriali e dei servizi* (Inquiry into investments of industrial and services firms; henceforth, Invind), an annual survey conducted by the Bank of Italy since 1984 and representative of industrial and services firms with at least 20 employees. The survey is conducted between February and May of every year t and contains information on relevant firm-level variables such as sales, profits, employment, costs, actual and expected price changes and actual and expected investment in year $t-1$. Invind data have been used before in the literature to address a number of issues such as the impact of productivity and demand shocks on firms' growth (Pozzi and Schivardi, 2016), bankruptcy law and bank financing (Rodano et al., 2016), the determinants of investment demand (Guiso and Parigi, 1999; Bond et al., 2015) and the role of management practices during the Covid-19 pandemic (Schivardi et al., 2021).

The 2022 wave of the survey includes a retrospective *ad hoc* section on the 2021 energy crisis, made up of nine quantitative questions, which we report in Appendix A. The survey asks firms to indicate both expenditures (in €) and physical quantities (in MWh and scm) for purchased electricity and natural gas, both during the first (before the crisis) and the second (after the crisis) semester of 2021.[10] Same as every year, the survey was conducted between February and May (of 2022). The survey section also contains qualitative information on strategies that firms had put in place *before* the crisis in order to cope with the price increases, which we use to build our instrumental variable.

Overall, the survey collects information from around 4,000 firms belonging to either industry or services with at least than 20 employees. However, the *ad hoc* energy section was run only among industrial firms with at least 50 employees (1,893 firms). Out of these, 1,570 firms responded to at least one question of the energy section; nevertheless, only 953 firms responded to all the nine questions that are used in this study. Dropping few inconsistent cases we are left with a sample of 907 firms. We rely on this latter subsample for our analyses.[11] We report information on our data validation procedures in Appendix B. We present a discussion of sample attrition and some robustness tests on it could affect our results in Appendix C.

[10] This excludes self-production by firms.

[11] Yet, it should be noted that the baseline estimates of Section 4.b are based on two different sets of respondents, depending on the electricity-related and gas-related available replies. As regards gas consumption, there are 682 respondents who gave non-missing information on quantities, costs and their hedging strategy. For electricity, up to 848 firms provide all the relevant variables.

We supplement the survey data with other confidential administrative information, which we match through firms' unique tax identification numbers. Thanks to data from the Italian Institute for Environmental Protection and Research (ISPRA), we gather information on whether firms have at least one plant subject to the *EU Emissions Trading System* (EU ETS firms), and detailed input use by fuel, available since 2014 at the yearly frequency. We use these data on fuel consumption both to validate the gas consumption measures in our survey, and to study the substitutability away from gas towards other inputs. In addition, we use data from the Fund for Energy and Environmental Services (CSEA) on firms which are eligible for energy subsidies linked to high electricity intensity and levels of consumption (*energivore* firms).[12] For these firms we observe electricity consumption (in MWh) at the monthly frequency since 2018.[13] These companies, around 3,700 in Italy, are likely to belong to the right tail of the energy-intensity distribution. In order to qualify for the subsidy scheme, firms must consume at least 1 GWh of electricity per year. In addition they must belong to a specific set of 4-digit NACE industrial sectors defined by the EU regulation on State Aid; for a sub-set of these sectors there is the additional requirement of having the ratio between electricity expenditure and value added higher than 0.2.

Finally, we also use additional information from the Italian National Institute of Statistics (Istat) on the energy intensity of Italian industrial firms at the level of 2-digit NACE sectors and Eurostat aggregate data on average retail prices and consumption for industrial consumers by consumption bracket. These data are useful to validate our survey measures, as reported below.

2.b Measurement of key variables

Thanks to firm-level information in the survey, we are able to reconstruct unit prices for gas and electricity, separately for the first and second semester of 2021. The biannual frequency is of great use, because the energy crisis unfolded only in the second half of the year.

Firms are asked to *"indicate, even approximately, the purchased quantity and the respective cost"* of electricity and gas, separately for the first and the second semester of 2021. We obtain unit prices in each semester by dividing electricity (or gas) expenditures in € by the respective quantity purchased, measured in MWh or scm. Due to various inconsistencies in the raw

[12]The registry is publicly available on the website of the Fund for energy and environmental services (portale elettrivori, Cassa per i servizi energetici e ambientali, CSEA).

[13]The subsidies grant a permanent discount on the component of the electricity price that is earmarked to finance subsidies for renewable energy generation (*oneri di sistema A3*SOS*). This component was completely lifted for all firms since January 2022, while in the last three quarters of 2021 was lifted for low voltage consumers (e.g. households and small firms).

data, mostly due to erroneous units of measurement, we perform several cleaning procedures. After cleaning, the unit prices measured in Invind match well, both in levels and in changes, with analogous measures for the average firm in different consumption classes from Eurostat. We report results for this and other validation tests in Appendix B.

The survey also asks firms whether **at the beginning of 2021** *"did the firm own any instruments that protected it, wholly or partly from energy price increases over the second half of the year"*. Firms had four options: *"no; yes, through fixed price contracts; yes, through financial derivatives; yes, other instruments."* For the purposes of our analyses we collapse these answers to a binary indicator (Yes or No). In our paper we use this variable as an instrumental variable for the infra-annual change in unit prices, constructed as described right above. We discuss the strength and validity of our instrument in Section 4.a.

3 Descriptive facts

We emphasize four stylized facts about the 2021 energy crisis and the exposure of businesses included in our sample. These are useful for the interpretation of the econometric results.

Descriptive fact #1: *In 2021 wholesale energy prices increased substantially more than retail prices paid by non-household consumers.*

In Figure 1 we report percentage changes in wholesale (from the Italian Regulator of energy wholesale markets, GME) and average retail prices for non-household consumers by consumption class (from Eurostat), separately for electricity and gas.[14] In the second semester of 2021, electricity and gas wholesale prices increased approximately by 150 and 200 percent, relative to the previous semester. Conversely, the increase in retail prices was much lower, ranging from 15 to 20 percent for electricity, and 28 to 66 percent for natural gas. This difference reflects the fact that energy costs are only a fraction of the retail price, which includes taxes and other fees; furthermore, many retail contracts are not indexed to the wholesale price, and fix the price for a certain period (typically 12 to 24 months).[15]

Descriptive fact #2: *Retail energy price changes for industrial firms between the first and second*

[14]For readability purposes, we only report three consumption classes, which cover 81 percent of firms in our survey sample for electricity and 89 percent for gas.

[15]Energy retail price includes several components (fees for transport and distribution, taxes and levies, quantity of energy, power capacity (MW), etc.). Some of these components are fixed costs i.e. not a function of quantity purchased, and thus average price is declining in consumption. In addition, larger consumer have higher bargaining power, making this negative relationship even stronger. As such, the same increase in wholesale price makes for higher percentage change increase in higher consumption classes, where the incidence of energy cost in total retail price is higher, and where retail prices are lower to begin with.

semester of 2021 display substantial cross-sectional variation.

Invind data reveal average price changes similar to the aggregate data: 75 percent for gas and 30 percent for electricity.[16] However, the survey data reveal substantial heterogeneity in the prices firms pay (Figure 2). Notably, the retail price did not even increase for a significant fraction of firms. As we will document later in the paper, a sizable fraction of this heterogeneity is due to the fact that many firms were insured, mostly because they purchased energy via fixed-price contracts. Together with other observable factors - such as geographical location, being part of *energivora* firms, inclusion in the EU ETS - insurance status explains respectively 30 and 20 percent of total variation in prices for electricity and gas. The large portion of unexplained variation is not surprising, because the retail market for business consumers is deregulated, and it is populated by several local and national providers, which sell energy under very diverse contractual arrangements.

Descriptive fact #3: *At the onset of the crisis, the incidence of energy cost followed a skewed distribution.*

We calculate energy cost at the onset of the crisis as the sum of gas and electricity expenditures in the first semester of 2021 (i.e. before the energy crisis). We construct two measures of the incidence of energy costs: as a fraction of turnover and as a fraction of input costs (net of labor cost).[17] The distribution of both variables is very skewed with a long right tail (Figure 3 and 4). The energy cost-turnover ratio has median equal to 1.7 percent and average equal to 2.8 percent; the corresponding statistics are 2.4 and 5.3 percent for the energy cost-input cost ratio. Thus for most firms energy accounts for a small share of costs, but for a minority of firms electricity and gas are critical inputs.

If we build indicators of energy intensity at the sector level using the Invind data, we get sectoral averages that are broadly in line with sector-level energy intensity measures present in aggregate input-output tables from the Italian National Statistical Office (Figure 5). However, regression analyses on the micro-data reveals that less than 10 percent of this variation is accounted for by sectoral differences.[18] The within-sector heterogeneity in energy intensity is depicted in Figure 6a. Furthermore, from the regression analysis it emerges that *energivore* firms display much higher values for both measures; this is not surprising given

[16]See Appendix B for a comparison by consumption class.

[17]Input costs are measured as the total cost of spending on raw materials, consumables, goods for resale, and services in the year (including purchases made by firms in the same group) and of change of stocks of those goods. For both incidence variables we divide by two the denominators, assuming they are uniformly distributed across the two semesters.

[18]We rely on a 7-industry categorization. Using NACE 2-digits dummies, the R-squared increases up to 20 percent, but in some cell we have very few observations.

that *energivora* status is granted based also on similarly defined energy intensities. Finally, also EU ETS firms display much higher incidence of energy costs.

Finally, we calculate the same variables using energy expenditures in the second semester. The median increase in energy cost-turnover ratio is 33 basis points, and the average increase is 1.3 percentage points. The incidence measured as a share of input cost increase on average by 2.5 percentage points, while the median increase is half percentage point. These results suggest that, even in the second half of 2021, the incidence of energy costs remained low for most firms. However, a subset of energy-intensive firms appear highly exposed to any further price increase.

Descriptive fact #4: *Net of seasonal factors, aggregate industrial energy consumption did not fall in the second semester of 2021.*

As a first check on the magnitude of demand elasticities during the 2021 crisis, it is useful to inspect the dynamic of aggregate consumption. Consider in fact that, given the size of the shock, large elasticities should translate into significant drops of energy consumed. As visible from Figure 7, in the second half of 2021 electricity consumption increased relative to the first semester, and gas consumption decreased; however both changes are well in line with what occurred in the previous ten years.

4 The impact of the energy crisis on firms' choices

Estimating the price elasticity of demand is a standard econometric problem. Since quantity and price are jointly determined by supply and demand factors, regressing the (log of the) first on the (log of the) second does not identify the elasticity of interest. A classic solution is to use a supply shifter as an instrument to trace out demand.

In our application it is likely that the surge in firm-level energy prices observed in the second semester is largely due to supply shocks at the European level (i.e. bad weather conditions, unexpected maintenance work in the energy sector, geopolitical tensions). As such, one might think that in our sample even the OLS estimator would be able to trace out the demand elasticity by regressing the change in log quantity on the change in log firm-level price. However, the retail energy price is in general decreasing in quantity because of two factors: a) the energy bill includes components that are not function of the quantity purchased (e.g. transportation fees, component proportional to power availability); b) bargaining power of large consumers. As such, those firms that would decrease their demand of energy in the second semester due to demand shocks, would see their price (unitary cost of energy) increase. The OLS estimator would pick up this variation as well, and it is thus safer to rely on a instrumental variable strategy to identify the elasticity of interest.

In order to estimate the causal effect of electricity and natural gas price swings on the respective input demands by industrial firms, we exploit plausibly exogenous cross-sectional variation in the availability of fixed-price contracts (or similar hedging instruments) subscribed *before* the energy crisis. We use this information to construct an instrumental variable, which we use in a DiD-IV design. Whether a firm was endowed with a fixed-price contract before the crisis is highly predictive of the *change* in electricity and gas unit costs during the crisis and arguably is unrelated to potential *changes* in energy consumption, absent treatment. In what follows, we first provide a detailed discussion on the identification assumptions and we present our IV estimates of the short-run price-elasticity of electricity and gas demand. A comparison of OLS and IV estimates is available in the appendix.

4.a Identification

Our equation of interest reads:

$$\Delta \log(Q_i^s) = \alpha_s + \beta_s \Delta \log(P_i^s) + \epsilon_i^s \tag{1}$$

where $s = \{\text{electricity, gas}\}$. $\Delta \log(Q_i^s)$ is the log change in purchased quantities between the first and the second semester of 2021 for firm i, and similarly for the change in prices $\Delta \log(P_i^s)$. Due to the properties of logs, our coefficient of interest β_s can be interpreted as an elasticity.

We exploit the fact that at the beginning of 2021, approximately six months before the energy crisis, some firms had already in place fixed-price contracts or other types of financial instruments that shielded them, at least partially, from the price surges occurring in the second part of the year. In our data, around 42 percent of firms were not protected; 47 percent had signed fixed-price contracts; 3 percent had purchased derivatives, and 8.5 percent were protected via other instruments. In our analyses we collapse these answers to a binary indicator Z_i ($Z_i = 1$ if protected or $Z_i = 0$ if not protected) and use this as an instrument. In Italy most firms buy energy on the un-regulated market. Fixed-price contracts usually have a standard duration of 12 or 24 months since the subscription date. A non-exhaustive set of examples of how we code the dummy variable Z_i is reported in Figure 8. Given this binary instrument, the first stage of our model thus reads:

$$\Delta \log(P_i^s) = \rho_0 + \rho_1 Z_i + u_i \tag{2}$$

We expect ρ_1 to be negative, that is firms that were protected faced smaller price increases. For our DiD-IV design to be valid, the instrument needs to satisfy four conditions: (i) independence/parallel trends (ii) exclusion restriction (iii) relevance (iv) monotonicity (Angrist and Imbens, 1995a; Hudson et al., 2017). We try to probe the validity of our design with a thorough discussion of these assumptions, together with some identification checks. We discuss these in turn.

A1 (*Independence/parallel trends*): the untreated potential outcomes $\Delta \log(P_i^{s,0})$ and $\Delta \log(Q_i^{s,0})$ are mean independent of Z_i.

This assumption requires that the instrument status is not predictive of average changes in energy prices and quantities that would have occurred absent protection. It is straightforward to construct simple violations: if those firms that expect a larger price surge in the months ahead purchase a fixed price contract in advance for the year 2021, then the estimator may be inconsistent. To mitigate this type of worries, we looked at data on energy futures in European market. We can observe that these remained flat and below pre Covid-19 levels at least until April 2021, thus suggesting that market participants did not foresee any upsurge in energy prices until that point in time. Therefore independence should not be violated. As for quantities, we think confounding factors are less evident. Nevertheless, for the subsample of EU ETS firms, for which we observe natural gas quantities for several years, we provide *pre*-trend tests and show that insured firms were not on a growing or shrinking trend in terms of quantities in the years before the energy crisis.

A second concern stems from the fact that our instrument might also capture differences in propensity to insure across firms, which may independently explain subsequent input quantity choices. As a partial remedy, our DiD-IV design nets out any time-invariant drivers of these differences, such as risk-aversion. Our estimator may still be inconsistent if such characteristics independently explain changes in input quantities. In order to mitigate this concern we test whether firms with different values of our instrument are different on predetermined observables. We report the results of this "balancing" test in Table 1. We can observe some statistically significant differences among key dimensions. Insured firms are bigger (both in terms of sales, employment and investments), are more likely to self-generate a part of their electricity, to have an account of greenhouse gas emissions and be subject to the EU ETS system. For this reason, in all of our DiD-IV design exercises we include these key covariates. In a multi-period DiD this is akin to introducing covariate-specific nonparametric time trends ($\sum_t X_i \cdot \gamma_t$). Our results, which we report in Section 4.b are virtually unchanged when including these controls in the regression, supporting the validity of our design.[19]

A2 (*Exclusion restriction*): $\Delta \log Q_i^s(\Delta \log(P_i^s), Z_i = 1) = \Delta \log Q_i^s(\Delta \log(P_i^s), Z_i = 0)$ for all $\Delta \log Q_i, \Delta \log P_i$.[20]

The exclusion restriction imposes that the instrument has no predictive power on the average change in quantities for a given fuel s, once we condition on price changes of such fuel. The most plausible threat to the exclusion restriction in our setting occurs because the instrument moves two endogenous variables at the same time i.e. the change in the price of electricity and the change in the price of gas.[21] One simple sufficient condition for restoring identification is to assume that the price of electricity is *excludable* from the gas demand equation and viceversa. In this way we would have two endogenous variables and one instrument, but also two *equations*. This restriction is verified if one assumes that gas and electricity are not substitutes nor complements over the considered time horizon. We are comfortable to assume no substitutability in the very short run, as technology is likely fixed. The assumption of no complementarity may be less likely to hold because it can occur even at constant technology.[22] However, given that price changes of the two energy inputs are positively correlated in the sample, this would cause an overestimation of the absolute value of both elasticities.[23] Given that we estimate very small elasticities, this cannot but reinforce

[19]We exclude from the set of controls the dummy for whether the firm self-generates electricity as this would reduce the sample size by 50 percent for both the electricity and gas regressions. In both cases coefficients become marginally positive, around 0.05.

[20]Superscripts are omitted for notational convenience.

[21]Formally, our binary instrument is thus formally defined as $Z_i = \max(Z_i^g, Z_i^e)$, where Z_i^g and Z_i^e are unobserved binary instruments capturing insurance against the price of electricity (e) and the price of gas (g), respectively.

[22]Consider for example a firm that manufactures and packages paper. Assume that the manufacturing phase only requires natural gas and the packaging phase only requires electricity, so that the firm uses both inputs but for different processes. If less paper is produced because of higher gas prices, there will be less paper to be packaged. As a consequence, the firm will use less gas and less electricity. At the level of firms then, electricity and gas will be complements.

[23]The IV estimator can always be interpreted as an OLS estimator of a regression of Y on the predicted value of X, based on the instrument Z, indicated by $\hat{X}$. Formally we have:

$$\beta_{IV} = Cov(Y, \hat{X})/Var(\hat{X}). \tag{3}$$

At this point, the omitted variable formula applies. If the two goods are complements, then the gas (electricity) price enters with a negative sign in the electricity (gas) demand equation. As said, in our sample the predicted gas (electricity) price changes are *positively* correlated with electricity (gas) price changes. By the omitted variable bias formula, the IV estimator would produce elasticity estimates that are smaller (more negative) than the true ones. For the electricity demand equation, we have:

$$\beta_{IV} = \beta + \gamma \cdot Cov(\widehat{\Delta \log P_e}, \Delta \log P_g)/Var(\widehat{\Delta \log P_e}), \tag{4}$$

our conclusion that firms did not strongly react to higher energy prices in 2021.

A3 (*Relevance*): $\Delta \log(P_i^s)$ is a non-trivial function of Z_i.

If the exclusion restriction is respected, then the only reason why we observe changes in prices for observations with different instrument values must be because the instrument *causes* a change in prices (Angrist et al., 2000). We test this assumption by means of a first stage regression and obtain reasonably high Kleibergen and Paap F-statistics (K-P F stat), always above 70 for electricity and above 12 for natural gas.[24] When studying EU ETS firms, possibly due to the small sample size ($N = 62$) we obtain K-P F stats below 10 (around 7 for many specifications). In order to mitigate concerns about weak instruments, in all cases we also report Anderson-Rubin confidence sets that are valid regardless of the strength of the instrument (Andrews et al., 2019).

A4 (*Monotonicity*): either $\Delta \log P_i^s(Z_i = 1) > \Delta \log P_i^s(Z_i = 0)$ or viceversa $\forall\, i$.

The assumption states that the instrument either has a positive impact on the endogenous variable or it has a negative impact and that the sign of this impact is the same for all units i. Following (Angrist and Imbens, 1995b; Angrist et al., 2000), in Figure 9 we provide a test of this assumption by reporting the cumulative distribution function of log price changes for both gas and electricity, depending on the value of the instrument. We see that the distribution for protected firms ($Z_i = 1$) first-order stochastically dominates the distribution for non-protected firms ($Z_i = 0$). This is a necessary condition for monotonicity to hold. If the two distributions crossed, this would imply that the effect of the instrument is positive for some units and negative for others, violating monotonicity. Aside from testing for monotonicity, this type of plot gives a very transparent representation of the variation that is captured by our IV and used to identify the effects. The horizontal difference between the two curves is directly related to the relevance of the IV. As a consequence, the plot illustrates what segments of the price change distribution are more influenced by the instrument. This is a useful piece of information for the interpretation of the instrument, as shown by Angrist et al. (2000) and described below.

A brief remark on the interpretation of the IV estimator: the interpretation of our IV estimates follows the work of Angrist et al. (2000), who study the LATE estimand in simulta-

with $\gamma < 0$ by complementarity and $Cov(\widehat{\Delta \log P_e}, \Delta \log P_g) > 0$ as measured in our sample. The product of the two terms, one positive and one negative, leads to a negative bias term, which biases our estimates downwards. A similar argument leading to identical conclusions follows when looking at the gas demand equation.

[24]In the case of just identified IV this is equivalent to the Olea and Pflueger (2013) effective F-statistic. The latter is the relevant F statistic for testing against weak instruments (Andrews et al., 2019).

neous equation models.[25] In our setting, the LATE-elasticity can be interpreted as a weighted average effect for compliers, with weights that positively depend on how strong is the first stage in different parts of the price change distribution. Figure 9 shows that the instrument bites everywhere in the distribution but that the most relevant price changes are those in the middle. In all of our paper we assume the causal response function is linear in log-log space. In this special case, the IV estimand is:

$$\epsilon_{LATE} = E[\epsilon_i \cdot \omega_i] \quad \text{with } \omega_i = \frac{(\Delta \log(P_{1i}) - \Delta \log(P_{0i}))}{E[\Delta \log(P_{1i}) - \Delta \log(P_{0i})]}, \tag{5}$$

where ϵ_i are firm-level elasticities, and P_{1i}, P_{0i} are potential prices under the different values of the instrument $Z_i = \{0, 1\}$.[26]

4.b Price-elasticities of demand for the whole sample

In Table 2 and 3 we present our baseline estimates for the short-run price elasticity of demand for electricity and natural gas. In Panel (a) we report second-stage estimates from our DiD-IV design, which combines the cross-sectional variation induced by the price protection instrumental variable and the panel variation between the first and second semester of 2021. In order to address potential violations of the independence assumption, as discussed in Section 4.a each column reports a specification with a different set of control variables. In Panel (b) we present the corresponding first-stage estimates and weak IV diagnostics. Specifically we report both the K-P F statistic, equivalent to the Olea and Pflueger (2013) effective F for the just-identified case and the Anderson-Rubin (AR) confidence interval for the second stage coefficient. This confidence interval is robust to weak instruments but not less powerful than the conventional t-test (Andrews et al., 2019). Across all specifications, the K-P F statistic indicates a highly significant difference in price changes across protected and not protected firms. The first stage regression coefficients indicate that this difference is also economically meaningful. In all specifications, on average, protected firms enjoy a discount on their energy price change of 20 percentage points for electricity and 15 percent for natural gas.

Turning to Panel (a), in column (1) of Table 2 the coefficient associated with $\Delta \log P$ electricity is -0.0286. Taken at face value, an elasticity estimate of this magnitude implies that a 20

[25] A simpler and concise exposition is contained in Angrist and Pischke (2009), pp. 186-188.

[26] If the elasticity was not constant at the firm-level, then the IV estimand would be an average elasticity with higher weights to price ranges where the instrument shifts the distribution more strongly (Angrist and Pischke, 2009).

percent price increase (equal to the mean change in our sample) would lead only to a 0.57 percent reduction in electricity demand. Similarly, the coefficient associated with $\Delta \log P$ gas reported in the same column of Table 3 implies that a 43 percent increase in the gas price (again, equal to the mean change in our sample) would lead to a 7.87 percent decrease in gas consumption. In the case of electricity, the point estimates change slightly when introducing different control variables. For natural gas these changes are relatively bigger, especially when introducing sector fixed effects.

Although coefficients are not perfectly stable, the inclusion of control variables does not alter our main conclusion: *on average* firms did not respond much to the energy crisis during 2021. Also when we include all controls at once (column (6) for both tables), we notice that coefficients are very close to zero, indicating little substitution away from these productive inputs, even in face of substantial price increases. Although all of these IV estimates are not statistically different from zero (in spite of a powerful first stage), the confidence intervals are still informative about plausible elasticities that can be ruled out with a 5 percent confidence level. Across all specifications, we can exclude that the price elasticity of electricity demand is larger than -0.2 and that of gas is larger than -0.4 with a 5 percent confidence level. AR confidence intervals are very similar to the t-test-based ones in the case of electricity and somewhat wider in the case of natural gas. This does not alter any of the conclusions. The larger lower bound in the case of natural gas is not indicative of a more price-elastic demand for this input, as the upper bound of the confidence interval is larger too. All of this is a consequence of more imprecise estimates, both because of a less strong first stage and a smaller sample size.[27]

4.c Price elasticities and fuel substitution among EU ETS firms

The evidence presented in the previous subsection suggests that the average complier firm is subject to very low short-run elasticities and thus did not substantially cut energy consumption, even in face of a marked price increase. However, due to the skewed nature of the energy consumption distribution, there may be small groups of firms that get low weights by the IV estimator and for which these elasticities may be larger. In particular, we study firms that have at least an establishment subject to the *European Emission Trading System* (EU ETS firms), the EU cap and trade mechanism for greenhouse gas emissions. These plants are among the largest industrial consumers of natural gas. There are around 381 firms in Italy subject to the policy (accounting for more than half of aggregate industrial consumption

[27] All of our conclusions are qualitatively unchanged if we weight regressions by Invind sampling weights. See Tables A.3 and A.4.

of natural gas), and we are able to match 65 in our survey sample. Following the same identification strategy as in Section 4.a, we present results for this subgroup in Tables 4 for electricity and 5 for natural gas.

Same as for the average complier firm, electricity estimates are again very close to zero. Both the t-test based and AR confidence intervals can reject elasticities larger than 1 with a 5% confidence level across all specifications. The greater imprecision compared to the estimates in the previous subsection is possibly due to the very small number of observations. When we turn to gas, we find substantially larger elasticities compared to the average complier. Point estimates are between -0.548 and -0.847, with AR confidence intervals that exclude elasticities larger than -2.3 in most specifications. In some specifications these confidence intervals are unbounded from below, which is a consequence of the instrument being extremely weak when conditioning on a large set of controls with a small number of observations. Even in these cases the upper bound is always below zero. Although not conclusive, we take this as evidence that gas elasticities are larger for average compliers among EU ETS firms.

To get more convincing evidence, we turn on administrative data on plants subject to ETS, where we observe consumption of different fossil fuels.[28] This data is reported at the annual frequency, so we cannot construct semester-on-semester gas consumption changes. However, given that the data is available since 2014, we can employ a reduced-form dynamic difference-in-differences estimator with leads and lags. This is particularly convenient because it allow us to test for the presence of pre-crisis differential trends between firms that would and would not protected at the beginning of 2021.[29] Concretely, we estimate:

$$\log(Y_{it}) = \mu_i + \gamma_t + \sum_k \lambda_k \cdot Z_i \cdot \mathbf{1}(\text{year} = k) + \varepsilon_{i,t}. \tag{6}$$

In this equation we consider three outcomes Y_{it}: consumption of natural gas, consumption of natural gas substitutes, and consumption of natural gas *and* its substitutes, all of which are measured in Terajoules.[30] μ_i are firm fixed effects; γ_t are calendar year fixed effects; Z_i indicates the price protection instrument. The coefficient of interest is λ_{2021}, which captures the percentage change in the outcome of interest occurred between 2021 and 2020 among insured firms relative to uninsured firms.

[28]The data is described in detail in Section 2.

[29]Of course the protection status could have been different in previous years. Here we are checking whether protection status in 2021 is predictive of differential changes before the energy crisis, which supports the independence of the instrument in this setting.

[30]We include diesel, coal, heating oil, coke, naphta, gasoline, kerosene, LPG, anthracite, lignite, biofuels in gaseous form and other residual category fuels.

The estimated coefficients λ_k are reported graphically in Figure 10, alongside 90 percent confidence bands. Prior to 2020, all outcomes displayed the same dynamics among insured and uninsured firms, thus providing indirect evidence in support of the parallel trends assumption that underlies our difference-in-differences strategy. When looking at the log of natural gas consumption (Panel (a)), we see that in 2021 gas consumption increased by 8.5 percent more among insured firms.[31] A simple back-of-the-envelope calculation leads us to conclude that this change in gas demand implies an elasticity of -0.38, so it is broadly consistent with what estimated in the Invind survey.[32]

When looking at the consumption of natural gas substitutes (Panel (b) of Figure 10) their consumption of substitutes among insured firms increased by 30 percent less in 2021 relative to the other group, which suggests the presence of input substitution. Despite this large *percentage* change, it is unclear how extensive this substitution was. Indeed, before 2021, combined energy from these fuels accounted for approximately 15 percent of the energy obtained from gas, which is by far the main fuel among ETS firms.

To test for this, we use as outcome variable the (log of) total energy from gas *and* its substitutes in equation 6. If substitution were perfect, the coefficient λ_{2021} would be equal to zero, because the post-Covid increase in total energy would have been the same between the two groups, despite one relying more on gas than the other. This is not the case in the ETS data: the right-panel shows that the estimate of λ_{2021} is positive (9.5 percent) and virtually indistinguishable from the same coefficient obtained when only gas was on the left-hand side of the equation. In other words, in 2021 uninsured firms increased total energy consumption by less relative to insured ones, and thus substitution of gas with other fuels was only partial.

4.d Price elasticity of electricity among energy-intensive (*energivore*) firms

In this section we focus on firms that are eligible for energy subsidies linked to high electricity intensity (*energivore* firms). These are the largest industrial consumers of electricity. We describe in detail the characteristics of these firms in Section 2. Following the same identification strategy as before, we present results for this subgroup in Tables 6 for electricity and 7 for natural gas.

The price elasticity of demand for electricity (Table 6) is quite small and in the same ballpark

[31] On average both types of firms increased consumption in 2021 relative to 2020, probably due to the post Covid-19 recovery.

[32] The diff-in-diff estimate is a reduced form one and must be divided by the first stage to get the elasticity. We do so by using price information from Invind. Since we only observe price in 2021, we assume that in 2020 the gas price was equal to the level of the first semester 2021, and for 2021 we take the average of the two semesters.

as the one estimated for the overall sample. Point estimates fluctuate between -0.1 and -0.02. Both t-test-based and AR confidence intervals rule out elasticities larger than -0.4 in all specifications. Estimates for natural gas (Table 7) are more imprecise, with t-test-based confidence intervals never larger than -0.75. The instrument is weaker in the case of natural gas, which explains why some of the AR confidence sets are unbounded (either below or both ways). Although not conclusive we take this as evidence that elasticities do not substantially differ between *energivore* firms and the whole sample.

To corroborate this suggestive evidence from Invind, we turn on administrative data where we observe monthly electricity consumption for the period 2018-2021 for the same subset of firms. As for the ETS exercise, this data allows us to estimate a reduced-form dynamic difference-in-differences with leads and lags. In this case the monthly frequency allows to capture any drop in energy consumption occurred late in the year (e.g. in December when the wholesale price reached its annual peak), which could be hard to detect when looking at changes at the bi-annual frequency as in the survey data. We estimate:

$$\log(Q^e_{it}) = \mu_i + \gamma_t + \sum_k (\lambda_k \cdot Z_i \cdot \mathbf{1}(\text{monthly date} = k)) + \varepsilon_{i,t}. \tag{7}$$

In this equation μ_i indicate firm fixed effects; γ_t indicate month-year fixed effects; Z_i indicates the price protection instrument. The coefficients of interest are $\{\lambda_k\}_{k=2021jul}^{2021dec}$, which capture the percentage change in the electricity consumption occurred in each month k relative to June 2021 (the omitted category) among insured firms relative to uninsured firms.

The estimated coefficients λ_k are reported graphically in Figure 11 alongside 90 percent confidence bands, together with the dynamic of the wholesale electricity price (red line) as reference. Each dot is interpreted as the percentage change in electricity consumption relative to June 2021, the reference period, among insured firms relative to uninsured firms. Overall, these results confirm the previous evidence from Invind: electricity consumption did not change differentially between insured and non-insured firms in the second half of 2021. When focusing on the monthly frequency, it is possible to see a sharp differential rise in electricity consumption in December 2021 (when the wholesale price was almost at 300 euro per MWh), while in the previous months of the second semester the difference between insured and insured was the same as in June 2021. It is hard to draw definitive conclusions from the jump observed in December 2021, without extending the sample to the the first months of 2022, when the wholesale price increased further. We will extend this analysis as soon as these data will be available.

5 Consequences on price setting behaviour

Our results in Section 4.b suggest that the price elasticity of demand is close to zero for the average firm but that EU ETS firms face larger gas elasticities. A natural follow-up question is then: did businesses bear the additional cost or did they manage to pass through the shock to their consumers by increasing prices? Providing an answer is not easy because product prices may not be a choice variable for each individual firm. In competitive markets, all businesses will end up charging the same price for their good regardless of whether they were hit with a cost shock. Whether market prices will increase depends on whether the cost shock hits marginal or inframarginal firms.

Things would be different in the presence of market power, where firms trade-off a higher price with lower demand. Irrespective of the market structure, there is also evidence that firms adjust their price in response to changes in their competitor prices induced by a cost shock, even if they do not suffer the same shock themselves (so called strategic complementarities (Amiti et al., 2019).

The discussion above highlights that the identification strategy adopted up until now is not entirely adequate to identify these effects, because our instrument does not necessarily vary at the level of markets, but at least partly at the level of (insured vs not insured) firms *within* a market. Armed with this particular instrument, even if all hypotheses are respected, the IV estimator would only identify the *differential* behaviour between insured and uninsured firms, leaving out important market equilibrium effects. For example, in the perfectly competitive example our instrument would induce a zero effect by construction, because there would be only one price. While our results cannot be conclusive as for the magnitude of pass-through, they can still be informative for the sign of the effect.

For this section, we exploit the fact that in the Invind survey firms are asked every year to report the annual average percentage change in the price of their own goods, which we define π_{it}. Rather than relying on a simple diff-in-diff strategy, we use triple-difference estimators, where we model the differential evolution of posted final good prices according to whether firms were more or less energy-intensive (according to various definitions), whether the firms were price-protected or not and their interaction. In doing so, we focus solely on electricity, for which the first stage is particularly strong. Concretely, we estimate:

$$\pi_{it} = \mu_i + \gamma_t \tag{8}$$

$$
\begin{aligned}
&+ \sum_k \alpha_k \cdot Z_i \cdot \mathbf{1}(\text{year} = k) && (9)\\
&+ \sum_k \beta_k \cdot W_i \cdot \mathbf{1}(\text{year} = k) && (10)\\
&+ \sum_k \gamma_k \cdot Z_i \cdot W_i \cdot \mathbf{1}(\text{year} = k) + \varepsilon_{i,t}. && (11)
\end{aligned}
$$

In the specification Z_i indicates the price protection instrumental variable and W_i is a dummy variable indicating whether the firm is energy-intensive. As before, μ_i indicate firm fixed effects and γ_t indicate calendar year fixed effects. We omit dummy years relative to 2020 due to multicollinearity issues and interpret all coefficients in deviation from 2020.[33] We consider four proxies for energy intensity: a) electricity intensive (*energivore*) firms; b) EU ETS firms; c) firms in the top 25 percent of energy expenditures, defined as a ratio over input cost or over turnover.

Results are reported in Table 8. In column (1) a simple diff-in-diff specification reveals that in 2021 on average firms' prices increased by 6.6 percent relative to the previous year. However there is no differential effect for insured and uninsured firms (2021 $\times$ Z_i). The difference is only -0.08 percentage points, not significant at conventional levels. Note that this differential effect is remarkably small, when considering that uninsured firms suffer an increase in electricity prices 18 percentage points larger than those suffered by insured firms.

Moving to the richer specifications - columns (2) to (5) - an interesting pattern emerges: irrespective of the proxy used, energy intensive *uninsured* firms appear to increase their prices much more (at least twice as much in three specifications) compared to non-energy-intensive firms, for which output price increases in 2021 is on average 6 percent. However, energy intensive insured firms increased prices much less than their uninsured peers; the latter differential effect is not always statistically significant, but it is quantitatively large irrespective of the proxy used. As a placebo test, in column (6) we use a dummy for large firms (more than 199 employees) instead of the energy intensive proxy; in this case estimates of the differential effects are all very small.

Finally, note that differences in π_{it} between energy intensive firms and the rest of our sample arise exactly in 2021 and not in earlier years; this is true both in case of uninsured (Figure 12) and of insured companies (Figure 13). This result suggests that estimates in Table 8 can be interpreted as the effect of the energy crisis, rather than merely reflecting pre-existing

[33]Note that coefficients α_{2021} and γ_{2021} have a reduced form interpretation, and must be scaled by the corresponding first-stage coefficient from Table 2) to obtain the differential effect induced by a 1 percentage change in the electricity price.

differential trends in pricing behaviour between different groups of firms.

6 Conclusions

In this paper we have documented some stylized facts about energy use among medium and large Italian industrial firms and we have estimated their demand elasticity for both natural gas and electricity. To do so, we have used survey questions especially designed for these purposes combined with an identification strategy to address the endogeneity of energy prices to quantities. Our main results are the following: a) the distribution of energy intensity is very skewed: small for most firms, but quite large for some; b) the extent of the retail price occurred in the second half of 2021 is very heterogeneous across firms; c) both electricity and gas demand elasticity are on average small; gas elasticity is quite large for firms in the EU ETS; d) while most firms significantly raised prices of their own goods, the increase is larger for energy intensive firms, unless they were insured against energy price raises.

Note that our short-run elasticities are valid for 2021, but do not necessarily translate to the following year. First, as the time horizon widens, firms have an ampler set of possibilities to reduce energy consumption; as such, we expect elasticities to be larger in 2022. Second, wholesale prices increased further after the outbreak of the war in early 2022; to the extent that elasticities are non linear and decreasing in the size of the shock (e.g. because it gets marginally more difficult to cut back consumption the more demand is reduced), they will become smaller in 2022. Third, the futures curve suggests that in 2021 market participants expected the energy crisis to be short-lived, but the outbreak of the war made clear that price will remain on high levels for longer time (i.e. until the end of the conflict or until the complete substitution of Russian supplies with other energy sources). To the extent that elasticities might become larger if the price increase is perceived to last longer, we expect 2022 to be different from the previous year.[34] In order to extend our analysis to the current year, we have thus proposed the same questions in the next Invind wave, together with new ones aimed at measuring the aid measures implemented by the government in 2022 to reduce energy prices for firms.

[34] Elasticities might become larger if the crisis lasts longer because, for example, energy efficiency investments which were too expensive in case of a 6-month crisis, could become worth pursuing in case of a much longer period of high prices.

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Tables

Table 1: Balancing test for the instrumental variable

	Insured	Not insured	Diff.	
	mean	mean	b	t
Sales in 2020 (million euro)	209.06	105.40	-103.66*	(-2.26)
Tot. investments (million euro)	12.59	6.16	-6.42*	(-2.02)
Input costs (million euro)	197.09	104.59	-92.51	(-1.85)
Input costs over 2020 sales	0.64	0.65	0.00	(0.19)
Capacity utilization (%)	78.42	78.36	-0.07	(-0.06)
Expected capacity utilization in 2022	81.17	81.17	0.00	(0.00)
Employment	487.74	306.36	-181.38*	(-2.19)
Limited liability company (0/1)	0.28	0.32	0.04	(1.29)
Energy costs over 2020 sales (%)	2.66	3.14	0.48	(1.33)
Firm self-generates some electricity (0/1)	0.56	0.36	-0.21***	(-6.32)
Share of self-generated electricity (%)	17.17	8.86	-8.32***	(-5.55)
Status "Energivora" (energy intensive) (0/1)	0.30	0.22	-0.07*	(-2.49)
Firm uses emission accounting (0/1)	0.40	0.28	-0.12***	(-3.74)
Subject to ETS in 2021 (0/1)	0.09	0.06	-0.04*	(-2.05)
Food and beverages	0.14	0.10	-0.05*	(-2.25)
Textiles & apparel	0.10	0.09	-0.00	(-0.13)
Chem., pharma., rubber	0.18	0.13	-0.06*	(-2.46)
Non-metallic minerals	0.06	0.04	-0.02	(-1.18)
Metalworking industry	0.40	0.48	0.08*	(2.42)
Wood, paper, furniture	0.09	0.11	0.02	(1.14)
Water & waste	0.03	0.05	0.02	(1.80)
50-99 employees	0.26	0.33	0.07*	(2.14)
100-199 employees	0.26	0.27	0.01	(0.50)
200-499 employees	0.27	0.24	-0.03	(-0.94)
500-999 employees	0.12	0.09	-0.02	(-1.21)
1000 and more employees	0.09	0.06	-0.03	(-1.59)
North-West	0.31	0.28	-0.04	(-1.28)
North-Est	0.26	0.21	-0.04	(-1.50)
Center	0.25	0.27	0.02	(0.61)
South and Islands	0.18	0.24	0.06*	(2.31)
Observations	500	407	907	

Note: The table displays means and standard deviation of key variable by instrumental variable status. Our instrument is a dummy variable for whether before the crisis the firm had in place fixed-price contracts or other types of financial instruments that shielded it, at least partially, from the unexpected price increases that occurred in the second part of the same year. *b* in the second-last column indicates the coefficient of a regression of a given covariate on the instrument; *t* in the last column is the associated t-test.

Table 2: Price-elasticity of electricity demand (IV estimates)

	(1) Baseline	(2) Class size FE	(3) Sector FE	(4) Macroregions FE	(5) Controls	(6) All
Panel (a) : Demand equation						
$\Delta \log P$ electricity	-0.0286	-0.0237	0.0118	-0.0389	-0.0186	0.00997
	[-0.216,0.159]	[-0.210,0.163]	[-0.172,0.195]	[-0.223,0.145]	[-0.196,0.159]	[-0.169,0.189]
Panel (b) : First stage estimates						
Protected from price increase (0/1)	-18.70***	-18.73***	-18.70***	-18.81***	-20.17***	-19.72***
	[-22.90,-14.49]	[-22.98,-14.48]	[-22.92,-14.47]	[-23.05,-14.57]	[-24.41,-15.94]	[-23.99,-15.46]
Observations	848	848	848	848	816	816
K-P F stat	76.14	74.94	75.36	75.81	87.47	82.37
AR confidence set	[-.213866, .164186]	[-.208103, .168218]	[-.16235, .208286]	[-.22071, .150424]	[-.187153, .164218]	[-.159609, .201189]

95% confidence intervals in brackets
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table presents IV regressions at the firm-level of the semester-on-semester log changes in purchased quantities of electricity against semester-on-semester log changes in the average unit price of electricity. Self-generation is excluded as it is not purchased. Given the log-log specification, the coefficient is directly interpretable as an elasticity. confidence intervals in panel (a) are based on t-tests with robust standard errors. AR confidence intervals at the bottom of the table are based on Anderson Rubin tests (Andrews et al., 2019) and are robust to the inclusion of weak instruments. Column (1) includes no control variables. Column (2) includes dummies for five size classes: [50-99 employees], [100-199 employees], [200-499 employees], [500-999 employees], [1000 and more employees]. Column (3) includes 11 sector dummies: food and beverages; textiles and apparel; chemicals, pharma and rubber; non-metallic minerals; metalworking industry; wood, paper and furniture; water and waste. Column 4 includes four dummies for geographical macroregions: North-West, North-East, Center, South and Islands. Column (5) includes a series of firm-level controls: 2020 sales, 2021 employment, whether the firm self-generates electricity, whether the firm measures emissions, and whether the firm is energy intensive (*energivora* status). Column (6) includes all of these controls at the same time.

Table 3: Price-elasticity of gas demand (IV estimates)

	(1) Baseline	(2) Class size FE	(3) Sector FE	(4) Macroregions FE	(5) Controls	(6) All
Panel (a) : Demand equation						
$\Delta \log P$ gas	-0.183	-0.179	-0.00607	-0.185	-0.0905	-0.00589
	[-0.627,0.261]	[-0.606,0.248]	[-0.445,0.433]	[-0.621,0.250]	[-0.515,0.334]	[-0.426,0.414]
Panel (b) : First stage estimates						
Protected from price increase (0/1)	-14.02***	-14.37***	-13.56***	-14.18***	-13.56***	-14.18***
	[-21.62,-6.425]	[-22.06,-6.676]	[-21.14,-5.974]	[-21.73,-6.633]	[-23.23,-7.561]	[-22.74,-7.073]
Observations	682	682	682	682	315	315
K-P F stat	13.13	13.45	12.32	13.60	14.89	13.96
AR confidence set	[-.712454, .327942]	[-.688024, .312232]	[-.47612, .570405]	[-.704239, .298562]	[-.544907, .432417]	[-.438845, .545927]

95% confidence intervals in brackets
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table presents IV regressions at the firm-level of the semester-on-semester log changes in purchased quantities of natural gas against semester-on-semester log changes in the average unit price of natural gas. Given the log-log specification, the coefficient is directly interpretable as an elasticity. confidence intervals in panel (a) are based on t-tests with robust standard errors. AR confidence intervals at the bottom of the table are based on Anderson Rubin tests (Andrews et al., 2019) and are robust to the inclusion of weak instruments. Column (1) includes no control variables. Column (2) includes dummies for five size classes: [50-99 employees], [100-199 employees], [200-499 employees], [500-999 employees], [1000 and more employees]. Column (3) includes 11 sector dummies: food and beverages; textiles and apparel; chemicals, pharma and rubber; non-metallic minerals; metalworking industry; wood, paper and furniture; water and waste. Column 4 includes four dummies for geographical macroregions: North-West, North-East, Center, South and Islands. Column (5) includes a series of firm-level controls: 2020 sales, 2021 employment, whether the firm self-generates electricity, whether the firm measures emissions, and whether the firm belongs to the EU ETS. Column (6) includes all of these controls at the same time.

Table 4: Price-elasticity of electricity demand for EU ETS firms (IV estimates)

	(1) Baseline	(2) Class size FE	(3) Sector FE	(4) Macroregions FE	(5) Controls	(6) All
Panel (a) : Demand equation						
$\Delta \log P$ electricity	-0.00480	0.0443	0.192	0.00278	0.183	0.217
	[-0.909,0.899]	[-0.915,1.003]	[-0.586,0.971]	[-0.830,0.836]	[-0.972,1.337]	[-0.753,1.187]
Panel (b) : First stage estimates						
Protected from price increase (0/1)	-18.70***	-18.73***	-18.70***	-18.81***	-15.61***	-15.88***
	[-22.90,-14.49]	[-22.98,-14.48]	[-22.92,-14.47]	[-23.05,-14.57]	[-22.37,-8.856]	[-22.46,-9.289]
Observations	63	63	63	63	63	63
K-P F stat	7.935	7.136	7.533	7.887	5.761	5.004
AR confidence set	[-.972588, ...]	[-.94393, ...]	[-.64097, 1.68595]	[-.922535, 1.53375]	[-1.09984, ...]	[-.938669, ...]

95% confidence intervals in brackets

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table presents IV regressions at the firm-level of the semester-on-semester log changes in purchased electricity against semester-on-semester log changes in the average unit price of electricity. Self-generation is excluded as it is not purchased. Given the log-log specification, the coefficient is directly interpretable as an elasticity. confidence intervals in panel (a) are based on t-tests with robust standard errors. AR confidence intervals at the bottom of the table are based on Anderson Rubin tests (Andrews et al., 2019) and are robust to the inclusion of weak instruments. Column (1) includes no control variables. Column (2) includes dummies for five size classes: [50-99 employees], [100-199 employees], [200-499 employees], [500-999 employees], [1000 and more employees]. Column (3) includes 11 sector dummies: food and beverages; textiles and apparel; chemicals, pharma and rubber; non-metallic minerals; metalworking industry; wood, paper and furniture; water and waste. Column 4 includes four dummies for geographical macroregions: North-West, North-East, Center, South and Islands. Column (5) includes a series of firm-level controls: 2020 sales, 2021 employment, whether the firm self-generates electricity, whether the firm measures emissions and whether the firm is energy intensive (*energivora* status). Column (6) includes all of these controls at the same time.

Table 5: Price-elasticity of gas demand for EU ETS firms (IV estimates)

	(1) Baseline	(2) Class size FE	(3) Sector FE	(4) Macroregions FE	(5) Controls	(6) All
Panel (a) : Demand equation						
$\Delta \log P$ gas	-0.789**	-0.715**	-0.548*	-0.790**	-1.006*	-0.847**
	[-1.547,-0.0314]	[-1.426,-0.00295]	[-1.133,0.0372]	[-1.544,-0.0369]	[-2.064,0.0523]	[-1.679,-0.0152]
Panel (b) : First stage estimates						
Protected from price increase (0/1)	-32.85***	-32.28***	-28.69***	-32.55***	-28.31***	-25.53*
	[-53.17,-12.53]	[-53.51,-11.06]	[-49.44,-7.950]	[-53.65,-11.45]	[-49.07,-7.541]	[-51.88,0.812]
Observations	66	66	66	66	65	65
K-P F stat	10.43	9.253	7.667	9.515	7.445	3.810
AR confidence set	[-2.15237, -.03904]	[-2.05171,-.010138]	[... , .054931]	[-2.23634,-.074963]	[... ,-.001114]	[... ,-.158051]

95% confidence intervals in brackets

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table presents IV regressions at the firm-level of the semester-on-semester log changes in purchased natural gas against semester-on-semester log changes in the average unit price of natural gas. Given the log-log specification, the coefficient is directly interpretable as an elasticity. confidence intervals in panel (a) are based on t-tests with robust standard errors. AR confidence intervals at the bottom of the table are based on Anderson Rubin tests (Andrews et al., 2019) and are robust to the inclusion of weak instruments. Column (1) includes no control variables. Column (2) includes dummies for five size classes: [50-99 employees], [100-199 employees], [200-499 employees], [500-999 employees], [1000 and more employees]. Column (3) includes 11 sector dummies: food and beverages; textiles and apparel; chemicals, pharma and rubber; non-metallic minerals; metalworking industry; wood, paper and furniture; water and waste. Column 4 includes four dummies for geographical macroregions: North-West, North-East, Center, South and Islands. Column (5) includes a series of firm-level controls: 2020 sales, 2021 employment, whether the firm self-generates electricity, whether the firm measures emissions and whether the firm is energy intensive (*energivora* status). Column (6) includes all of these controls at the same time.

Table 6: Price-elasticity of electricity demand for *energivore* firms (IV estimates)

	(1) Baseline	(2) Class size FE	(3) Sector FE	(4) Macroregions FE	(5) Controls	(6) All
Panel (a) : Demand equation						
$\Delta \log P$ electricity	-0.0985	-0.102	-0.0220	-0.102	-0.0985	-0.0188
	[-0.354,0.157]	[-0.362,0.159]	[-0.298,0.254]	[-0.351,0.148]	[-0.354,0.157]	[-0.308,0.270]
Panel (b) : First stage estimates						
Protected from price increase (0/1)	-26.87***	-25.94***	-26.25***	-27.08***	-26.87***	-25.00***
	[-36.00,-17.74]	[-35.18,-16.69]	[-35.54,-16.95]	[-36.35,-17.81]	[-36.00,-17.74]	[-34.55,-15.45]
Observations	228	228	228	228	228	228
K-P F stat	33.63	30.58	30.97	33.14	33.63	26.63
AR confidence set	[-.36168, .175097]	[-.380307, .177164]	[-.306515, .284759]	[-.368837, .165319]	[-.36168, .175097]	[-.316609, .30233]

95% confidence intervals in brackets
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table presents IV regressions at the firm-level of the semester-on-semester log changes in purchased electricity against semester-on-semester log changes in the average unit price of electricity. Self-generation is excluded as it is not purchased. Given the log-log specification, the coefficient is directly interpretable as an elasticity. confidence intervals in panel (a) are based on t-tests with robust standard errors. AR confidence intervals at the bottom of the table are based on Anderson Rubin tests (Andrews et al., 2019) and are robust to the inclusion of weak instruments. Column (1) includes no control variables. Column (2) includes dummies for five size classes: [50-99 employees], [100-199 employees], [200-499 employees], [500-999 employees], [1000 and more employees]. Column (3) includes 11 sector dummies: food and beverages; textiles and apparel; chemicals, pharma and rubber; non-metallic minerals; metalworking industry; wood, paper and furniture; water and waste. Column 4 includes four dummies for geographical macroregions: North-West, North-East, Center, South and Islands. Column (5) includes a series of firm-level controls: 2020 sales, 2021 employment, whether the firm self-generates electricity, whether the firm measures emissions, whether the firm belongs to the EU ETS and whether the firm is energy intensive (*energivora* status). Column (6) includes all of these controls at the same time.

Table 7: Price-elasticity of gas demand for *energivore* firms (IV estimates)

	(1) Baseline	(2) Class size FE	(3) Sector FE	(4) Macroregions FE	(5) Controls	(6) All
Panel (a) : Demand equation						
$\Delta \log P$ gas	-0.238	-0.232	-0.0693	-0.223	-0.238	0.00696
	[-0.712,0.235]	[-0.767,0.302]	[-0.548,0.410]	[-0.649,0.203]	[-0.712,0.235]	[-0.505,0.518]
Panel (b) : First stage estimates						
Protected from price increase (0/1)	-19.14***	-17.13**	-16.31**	-20.09***	-19.14***	-15.09**
	[-32.77,-5.505]	[-30.68,-3.572]	[-30.29,-2.327]	[-33.85,-6.327]	[-32.77,-5.505]	[-29.40,-0.792]
Observations	196	196	196	196	196	196
K-P F stat	7.666	6.212	5.294	8.291	7.666	4.336
AR confidence set	[-1.10834, .28724]	[... , .383265]	[... , .772523]	[-.971638, .233407]	[-1.10834, .28724]	entire grid

95% confidence intervals in brackets
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table presents IV regressions at the firm-level of the semester-on-semester log changes in purchased natural gas against semester-on-semester log changes in the average unit price of natural gas. Given the log-log specification, the coefficient is directly interpretable as an elasticity. confidence intervals in panel (a) are based on t-tests with robust standard errors. AR confidence intervals at the bottom of the table are based on Anderson Rubin tests (Andrews et al., 2019) and are robust to the inclusion of weak instruments. Column (1) includes no control variables. Column (2) includes dummies for five size classes: [50-99 employees], [100-199 employees], [200-499 employees], [500-999 employees], [1000 and more employees]. Column (3) includes 11 sector dummies: food and beverages; textiles and apparel; chemicals, pharma and rubber; non-metallic minerals; metalworking industry; wood, paper and furniture; water and waste. Column 4 includes four dummies for geographical macroregions: North-West, North-East, Center, South and Islands. Column (5) includes a series of firm-level controls: 2020 sales, 2021 employment, whether the firm self-generates electricity, whether the firm measures emissions, whether the firm belongs to the EU ETS and whether the firm is energy intensive (*energivora* status). Column (6) includes all of these controls at the same time.

Table 8: Outcome: percentage change in own price relative to previous year

	(1)	(2)	(3)	(4)	(5)	(6)
2021	6.60***	5.58***	5.85***	6.52***	5.31***	6.86***
	(0.86)	(0.84)	(0.78)	(1.23)	(1.06)	(1.09)
2021 × Z_i	-0.08	-0.00	0.39	0.55	1.18	-0.36
	(1.09)	(1.15)	(1.04)	(1.53)	(1.36)	(1.44)
2021 × *Energivora* status		4.15*				
		(2.50)				
2021 × Z_i x *Energivora* status		-1.00				
		(2.89)				
2021 × ETS			11.35*			
			(6.51)			
2021 × Z_i x ETS			-8.27			
			(7.08)			
2021 × Energy intensive (cost)				1.48		
				(2.91)		
2021 × Z_i x Energy intensive (cost)				-3.35		
				(3.22)		
2021 × Energy intensive (turnover)					5.52*	
					(3.14)	
2021 × Z_i x Energy intensive (turnover)					-5.03	
					(3.51)	
2021 × Large						-0.65
						(1.80)
2021 × Z_i x Large						0.67
						(2.24)
Observations	3948	3948	3948	2832	2885	3948

Standard errors in parentheses

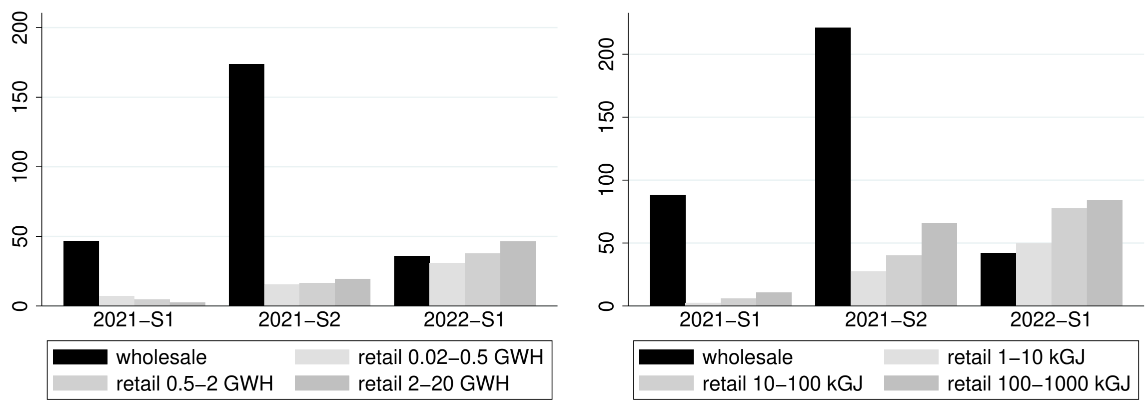
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table presents OLS regressions at the firm-level of percentage output price changes in the previous 12 months against firm effects, time effects, time effects interacted with the protection dummy Z_i, time effects interacted with different firm characteristics (one at the time) and the triple interaction of time effect, firm characteristics and protection dummy.

Figures

Figure 1: Price changes relative to previous semester (%)

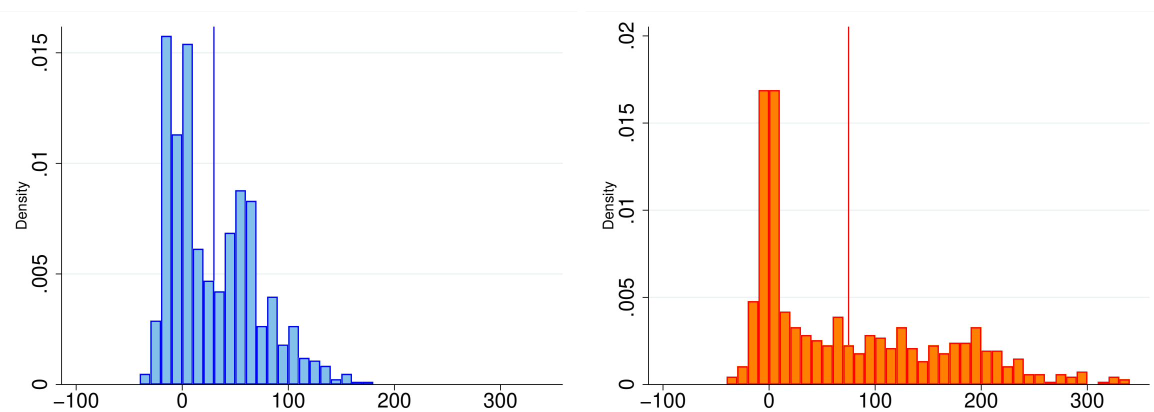
(a) Electricity (b) Natural gas



Note: The figure presents semester-on-semester percentage price changes for electricity (a) and natural gas (b), for different types of prices: wholesale (in black) and retail, divided by consumption classes (in scales of gray). S1 indicates the first semester; S2 indicates the second semester. GWh indicates Gigawatthours and kGJ indicates thousands of Gigajoules. Data from Eurostat and *Italy's Manager of Energy Markets* (GME).

Figure 2: Price changes in the second semester 2021 relative to previous semester (%)

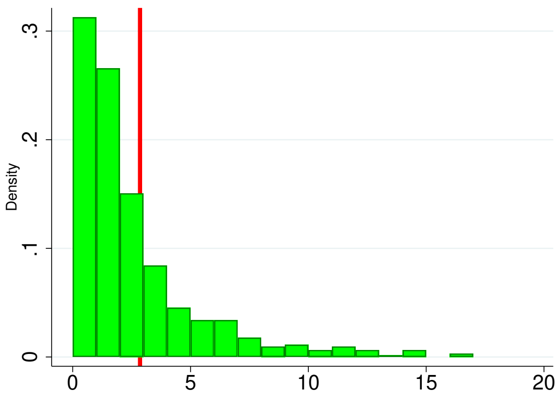
(a) Electricity (b) Natural gas



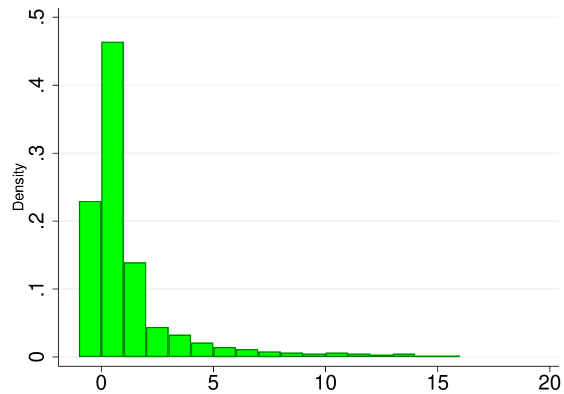
Note: The figure presents the density of semester-on-semester percentage price changes for both electricity (a) and natural gas (b), at the firm level (Invind data). For better visualization, histograms are trimmed at the 1st and 99th percentiles. Vertical lines represent sample means.

Figure 3: Energy cost / turnover (%)

(a) Level in sem I 2021



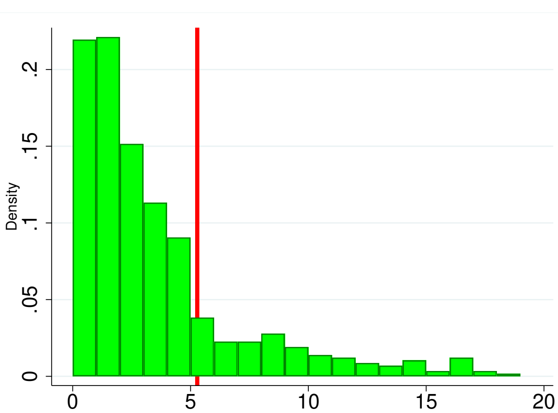
(b) Change sem II vs sem I



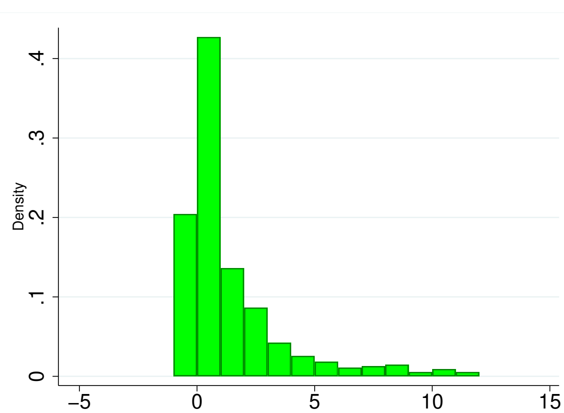
Note: The figure presents the density of the ratio of energy costs to turnover (in percentage points) during the first semester of 2021 (a) and the density of the change in the same ratio between the 1st and 2nd semester of 2021 (Invind data). Energy includes purchased electricity and natural gas. Self-generated electricity is excluded. For better visualization, histograms are trimmed at the 1st and 99th percentiles. Vertical lines represent sample means.

Figure 4: Energy cost / input cost (%)

(a) Level in sem I 2021



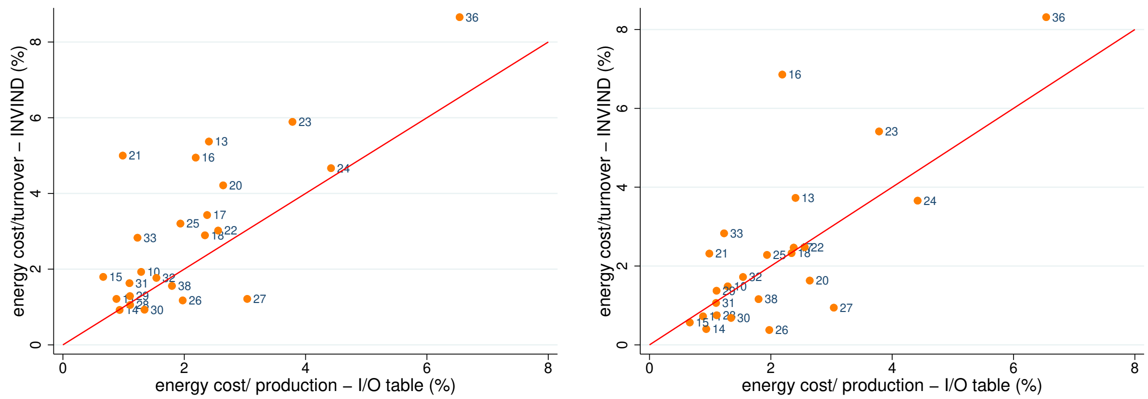
(b) Change sem II vs sem I



Note: The figure presents the density of the ratio of energy costs to input costs (in percentage points) during the first semester of 2021 (a) and the density of the change in the same ratio between the 1st and 2nd semester of 2021 (Invind data). Energy includes purchased electricity and natural gas. Self-generated electricity is excluded. For better visualization, histograms are trimmed at the 5th and 95th percentiles. Vertical lines represent sample means.

Figure 5: Energy cost incidence from Invind vs. from I-O tables

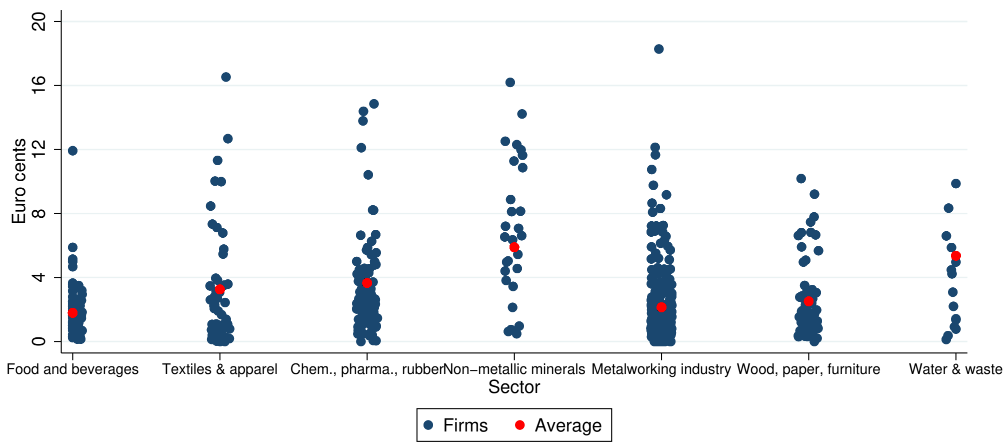
(a) Average incidence in sector (b) Median incidence in sector



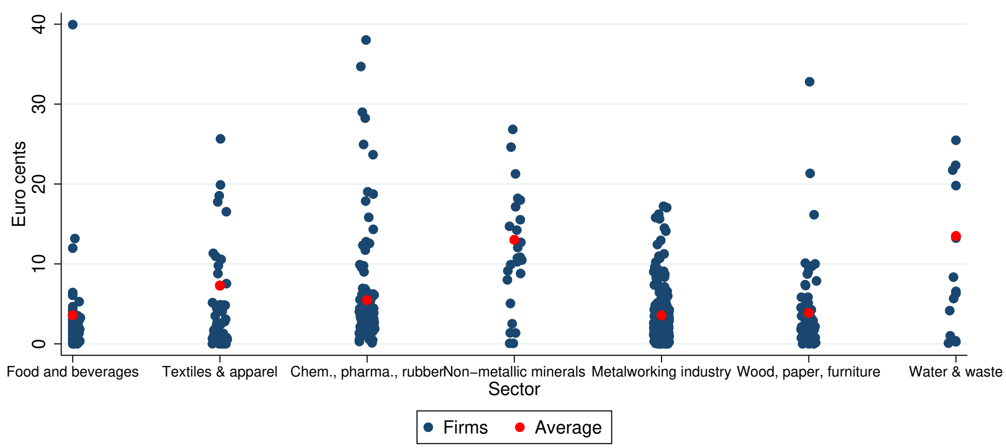
Note: The figure represents a scatterplot of sector-level averages (a) and medians (b) of energy costs to turnover ratios (from Invind) against energy costs to production ratios (from input output tables from the Italian National Institute of Statistics). The labels next to the dots refer to 2-digit NACE codes. The red line is the 45-degree line.

Figure 6: Energy intensity indicators across sectors

(a) Energy expenditure per euro of sales



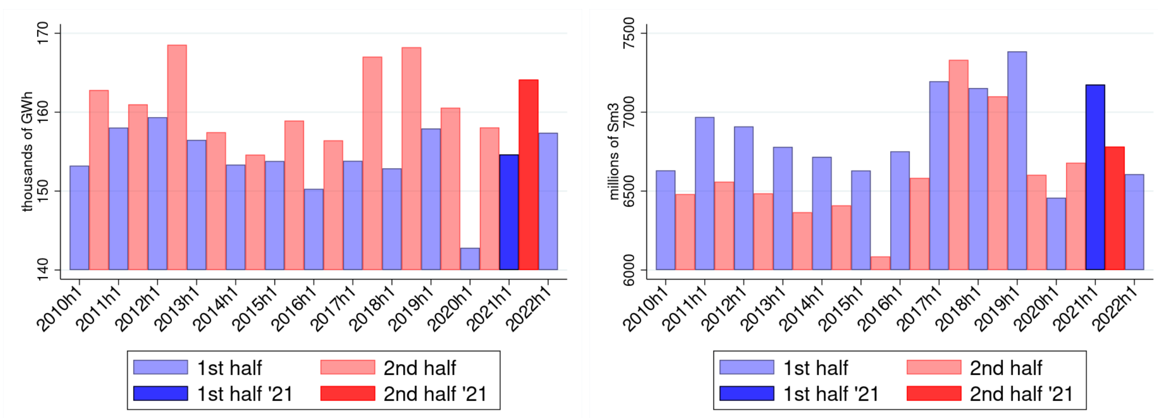
(b) Energy expenditure per euro of input costs



Note: The figure represents a scatterplot of firm-specific energy intensity indicators from Invind survey data. Energy costs are measured as the sum of the expenditure for electricity and natural gas in the first semester of 2021. Sub-figure a) illustrates the ratio between energy costs over turnover. Sub-figure b) displays the ratio between energy costs over input costs. The red dots represent the sector-level average. The indicator has been trimmed at 20 euro cents to improve readability in the first panel, and at 40 cents in the second; hence, ten firms are omitted in from the first plot, while 29 are omitted from the second.

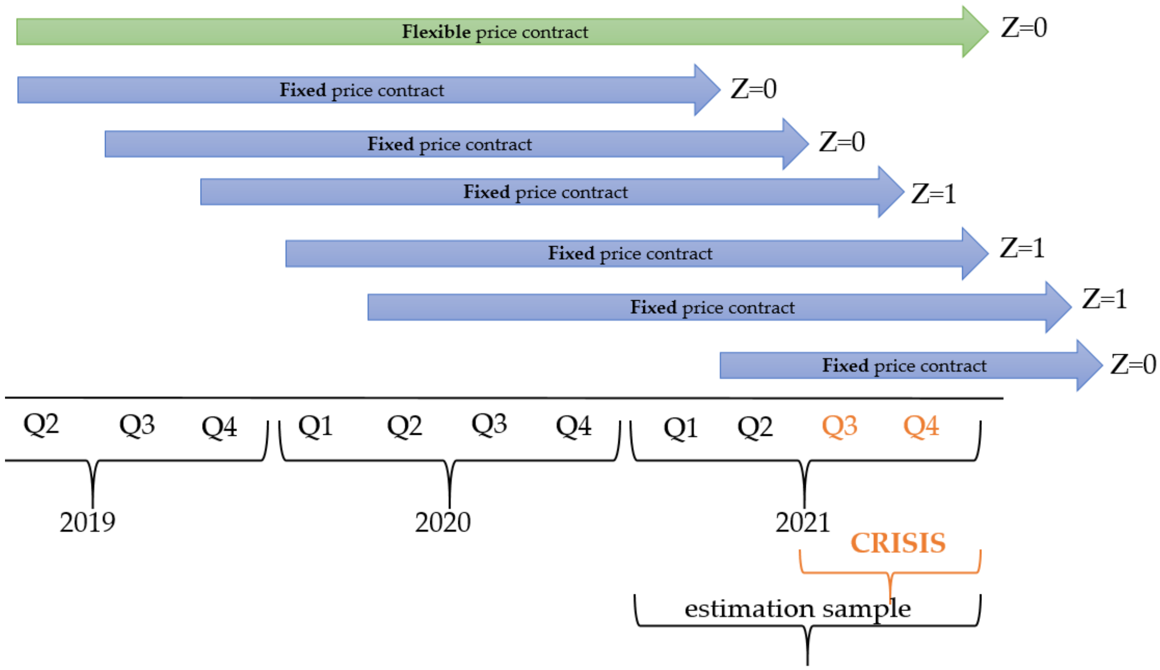
Figure 7: Industrial energy consumption at the bi-annual frequency

(a) Electricity consumption (b) Natural gas consumption



Source: The figure reports levels of electricity (a) and natural gas (b) consumption for each semester since 2010 and up to the first semester of 2021 for Italy. Sources are the main Italian operator for the transport and dispatching of natural gas (SNAM) and the Transmission System Operator for electricity (Terna).

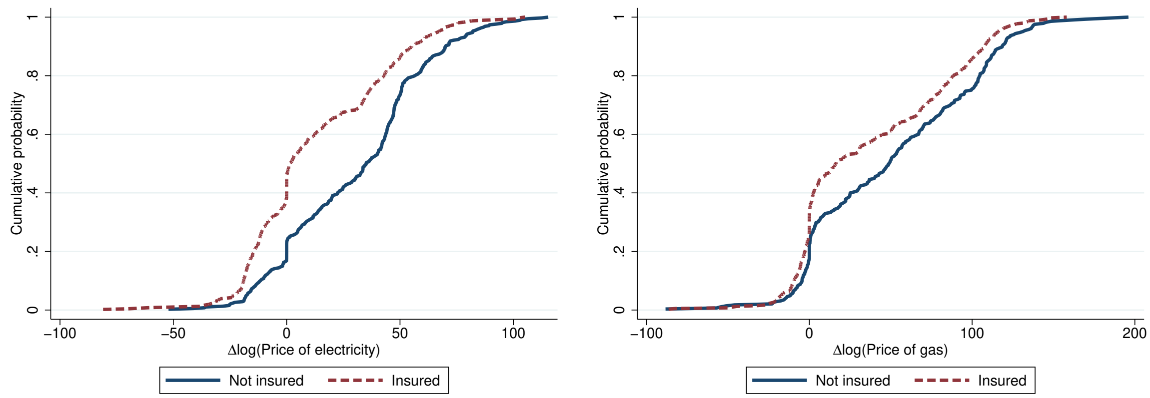
Figure 8: Examples of firms with the instrument switched on or off



Note: examples of the coding of the instrument Z_i based on the question: *"At the beginning of 2021, did your firm own any instruments that protected it, wholly or parly, from energy price increases over the second half of the year?"* In the figure, the arrow begins when a contract is signed and ends when the contract expires.

Figure 9: Monotonicity of the instrumental variable for electricity and natural gas

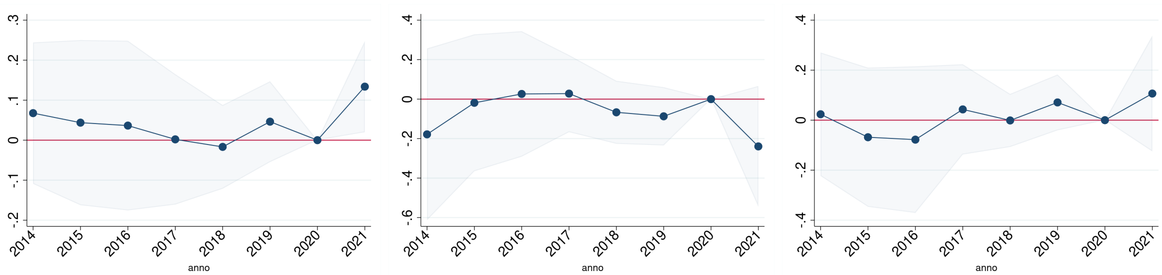
(a) Electricity (b) Natural gas



Note: The left (right) panel shows the cumulative distribution function of semester-on-semester log price changes of electricity (gas), separately for protected ($Z_i = 1$) and not protected ($Z_i = 0$) firms. Estimates on the Invind energy sample.

Figure 10: Consumption of natural gas and its substitutes among EU ETS firms

(a) log gas (b) log substitutes (c) log gas + substitutes

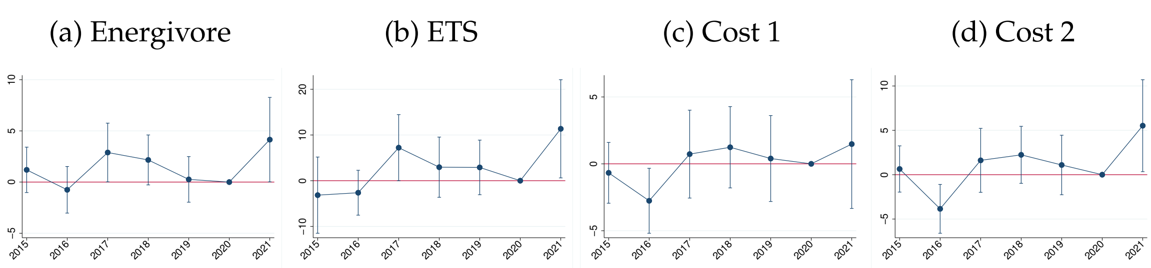


Note: the figure reports the coefficients on the interactions between Z_i and year dummies (equation (6)). Each panel refers to a different outcome: a) the log of gas consumption: b) the log consumption of substitutes of gas i.e. diesel, coal, heating oil, coke, naphta, gasoline, kerosene, LPG, anthracite, lignite, biofuels in gaseous form and other residual category fuels; c) log consumption of gas and its substitutes, as previously defined. Shaded area refers to 90 percent confidence intervals.

Figure 11: Event-study results for electricity; *energivore* sample

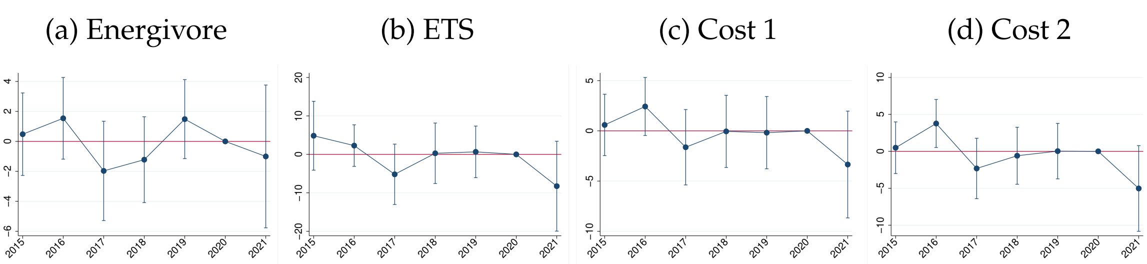
Note: the figure reports the coefficients on the interactions between Z_i and year-month dummies (equation (7)) alongside with 90% confidence intervals as shaded area. The outcome is log electricity consumption. Each dot is interpreted as percentage change in electricity consumption relative to June 2021, which is the omitted category in the time dimension. For reference, we report also the wholesale electricity price (PUN).

Figure 12: Pass-through: interaction between year dummies and dummy for energy intensity



Note: the figure reports coefficients on the interaction between a dummy for energy intensity and year dummies (equation (8)). Four proxies of energy intensity are considered in the four panels: a) energivore; b) ETS; c) firms in the highest quartile of energy cost over input cost (Cost 1); d) firms in the highest quartile of energy cost over revenues (Cost 2). The outcome is the percentage change in the price of final goods relative to the previous year. Bars refer to 90% confidence intervals.

Figure 13: Pass-through: interaction between year FE, dummy for energy intensity, and Z



Note: the figure reports coefficients on the triple interaction between a dummy for energy intensity, year dummies and Z (equation (8)). Four proxies of energy intensity are considered in the four panels: a) energivore; b) ETS; c) firms in the highest quartile of energy cost over input cost (Cost 1); d) firms in the highest quartile of energy cost over revenues (Cost 2). The outcome is the percentage change in the price of final goods relative to the previous year. Bars refer to 90% confidence intervals.

Appendix

Appendix A Survey questions

Figure A.1: 2021 Invind questionnaire administered to firms

Rising energy prices

At the beginning of 2021, did your firm own any instruments that protected it, wholly or partly, from energy price increases over the second half of the year?	E11

1 No
2 Yes, fixed-price contracts
3 Yes, financial derivatives
4 Yes, other instrument

	In the first half of the 2021		In the second half of the 2021	
Please indicate, even approximately, the purchased quantity and the respective cost of the following products:	Purchased quantity	Total cost (thousands of euros)	Purchased quantity	Total cost (thousands of euros)
Electricity	E9A MWh	E7A €	E9B MWh	E7B €
Natural gas	E10A Scm	E8A €	E10B Scm	E8B €

Assuming your firm's electricity consumption (in MWh) in 2021 was equal to 100, what share was covered by **in-house production**?	AE4
If the in-house production share is greater than zero:	
What kind of in-house production do you use?	AE6

1 Only from fossil fuels (e.g. coal, natural gas, etc.)
2 Mostly from fossil fuels
3 Only from renewable sources (e.g. solar panels, geothermal energy, wind turbines, etc.)
4 Mostly from renewable sources
5 Equally from fossil fuels and renewable sources

Appendix B Validation of survey data

In this Appendix we detail the validation procedure implemented to check the quality of the Invind survey data. Considering that respondents might not be familiar with physical units of measurement, we verify whether quantities of gas and electricity assume plausible values. To this end, we rely on two benchmarks to mark observations taking on implausible values and consequently recover the type of mistake made by the respondent, as described in the following algorithm.

First, we compute the average unitary price paid by firms for each semester and compare it with the corresponding average price recorded by Eurostat for the Italian market. The Invind and the Eurostat prices are constructed similarly, as they both include levies and taxes. However, Eurostat includes all non-household consumers, while we only have industrial firms with at least 50 employees. Therefore, we adopt a loose criterion and flag only those observations in which the unit price is not included in a price range defined as half the minimum price and double the maximum of the reference Eurostat statistics across consumption classes and semesters. Given that our unit prices in the Invind data could be out of the sensible range because of mistakes in filling in total expenditure (i.e the numerator) and/or the consumption quantity (i.e. the denominator), we also resort to a second criterion based on the examination of the ratio between energy costs and turnover. We flag observations above and below the 95th and 5th percentile of the distribution, respectively. These correspond to cost-turnover ratios above 50 percent and below 0.1 percent, respectively. Combining the two criteria, it is possible reconcile implausible unitary prices with specific errors in the units of measurement. Table A.1 reports the identified error categories.

In light of the categorization of replies, we adjust the units of measurement re-scaling the values accordingly, as illustrated in columns (3) and (4). Importantly, respondents displaying an inconsistent compilation error between the two semesters were dropped from the final sample.

Starting from the replies on natural gas, 74 percent of the observations has values coherent with both criteria and requires no correction. When both the cost share and the price are larger than the mentioned thresholds, we correct the cost, re-scaling from euro to thousand of euro. This fix was implemented in 2 percent of the observations. In almost 19 percent of the cases the cost share is included in the plausible range, but the price is remarkably small; hence, we suspect that quantities are measured in thousand of scm and therefore they shall be divided by one thousand. Two respondents stated very small amounts as they intended quantities in million scm and gas bills in million euro. After the adjustment, both

Table A.1: Validation of data quality: units of measurement in quantities and expenditure

(1) **Cost-share criterion**	(2) **Price-range criterion**	(3) **Expenditure**	(4) **Quantity**	(5) **Prevalence**
Panel A: Natural gas				
✓	✓	'000 €	scm	74.3%
✗- upper tail	✗- higher price ('000-fold)	€	scm	2.2%
✗- upper tail	✗- higher price (milion-fold)	€	'000 scm	0%
✓	✗- higher price ('000-fold)	'000 €	'000 scm	18.6%
✓	✗- higher price (milion-fold)	'000 €	million scm	0.1%
✗- lower tail	✗- lower price	Million €	scm	0.1%
✓	✗- higher price (II sem. only)	'000 €	'000 scm	0.1%
Residual observations				4.5%
Total				100%
Panel B: Electricity				
✓	✓	'000 €	Mwh	71.1%
✓	✗- lower price	'000 €	Kwh	13.7%
✗	✗- higher price	€	Mwh	0%
✗	✓	€	Kwh	1.9%
✓	✗- higher price	'000 €	Gwh	2.2%
✗- lower tail	✓	Million €	Gwh	0%
✗- lower tail	✗- lower price	Million €	Mwh	0%
✗- lower tail	✗- lower price	Million €	Kwh	0%
Residual observations				11.1%
Total				100%

Note: The table presents the result of the data validation procedure. As respondents might be unfamiliar with physical units of measurement, we reviewed the plausibility of the expenditure and quantity replies, separately for gas (Panel A) and electricity (Panel B). Depending on whether unitary prices satisfy two reference criteria (Column 1 and 2), observations are sorted into mutually exclusive compilation mistakes. In more details, the two checks allow us to determine the univocal units of measurement used by the respondent (Column 3 and 4) compatible with the mistake category. This exercise is performed for both semesters. In case we observe a consistent mistake across semesters, we rescale the values with the goal of harmonising all observations in terms of thousand of euro for expenditure, and Mwh and scm for purchased quantities of electricity and natural gas, respectively. We operate this correction in 17.8 percent of the electricity-related replies and 21.2 percent of the gas-related replies (Column 5).

criteria were satisfied. The remaining 4.5 percent of gas quantities and cost replies cannot be assigned to any recognizable category, and hence are eliminated from the sample used in the analyses.

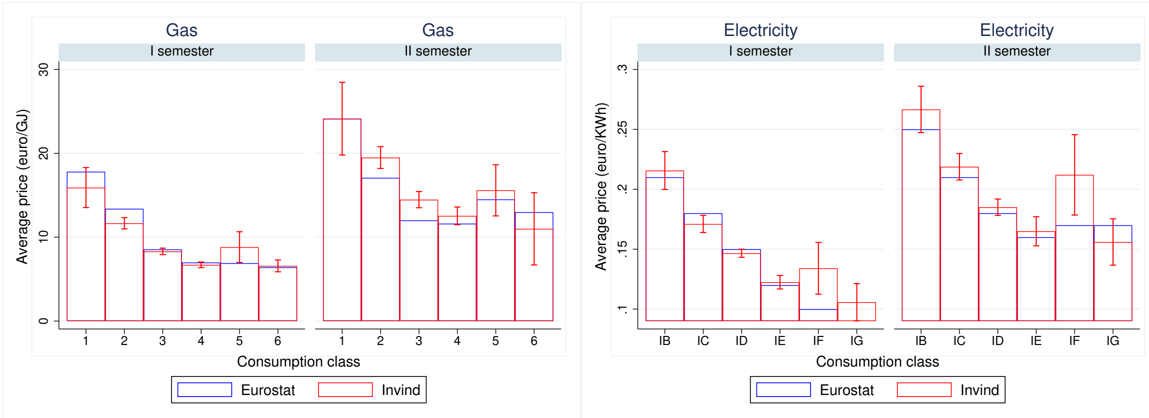
As regards the replies on electricity, in total 71 percent of the respondents answered with plausible values. For 14 percent of observations the cost criterion is satisfied, but the mean unitary price does not reach the lower bound of the plausible values, suggesting that quantities may have been measured in Kwh, instead of MWh. Hence for these observations we divide the quantities by one thousand. In 2 percent of observations in the final sample the price criterion is respected, but it is evident that both its numerator and denominator have to be re-scaled by one thousand, as the cost-over-turnover ratio belong in the right tail of the distribution, indicating electricity bills being measured in euro instead of thousand of euro. Another 2 percent of the sample is flagged as the price seems exceptionally high, i.e. above the reference boundary. It returns in the reference range once multiplied by one thousand, hinting at the fact that electricity was measured in GWh. For 110 companies, corresponding to 11 percent of the sample, this categorization returned undefined, as we cannot reconcile the reference values with any mistake pattern and therefore these observations are not included in the sample for the analysis.

Figure A.1 indicates that after the data cleaning process, the prices in our sample match fairly well the semester-specific average prices published by Eurostat. Next, we visually inspect whether the distribution of the adjusted and unadjusted observations are alike. As shown in Figure A.2, one can be reassured that our correction does not deliver odd alterations in the overall distribution of energy prices, thus suggesting that we have correctly guessed the exact mistake done by misreporting firms.

As a further test on the quality of our database, one could contrast the reported natural gas consumption against the same variable measured on administrative data. In doing so, we rely on data on the annual consumption of energy from fossil fuels (hence also of natural gas) validated by the Italian Institute for Environmental Protection and Research (ISPRA) for the sub-sample of firms regulated under the EU ETS. Figure A.3 indicates a very good match between the reported quantities for this set of firms.

Along the same lines, Figure A.4 depicts the semester-specific electricity use of 249 firms according to the two sources of information: Invind stated electricity consumption and administrative records from the Fund for Energy and Environmental Services (CSEA). Although this validation is possible only for a subset of companies receiving energy subsidies by the Italian government because of their high electricity intensity (*energivore* firms), We

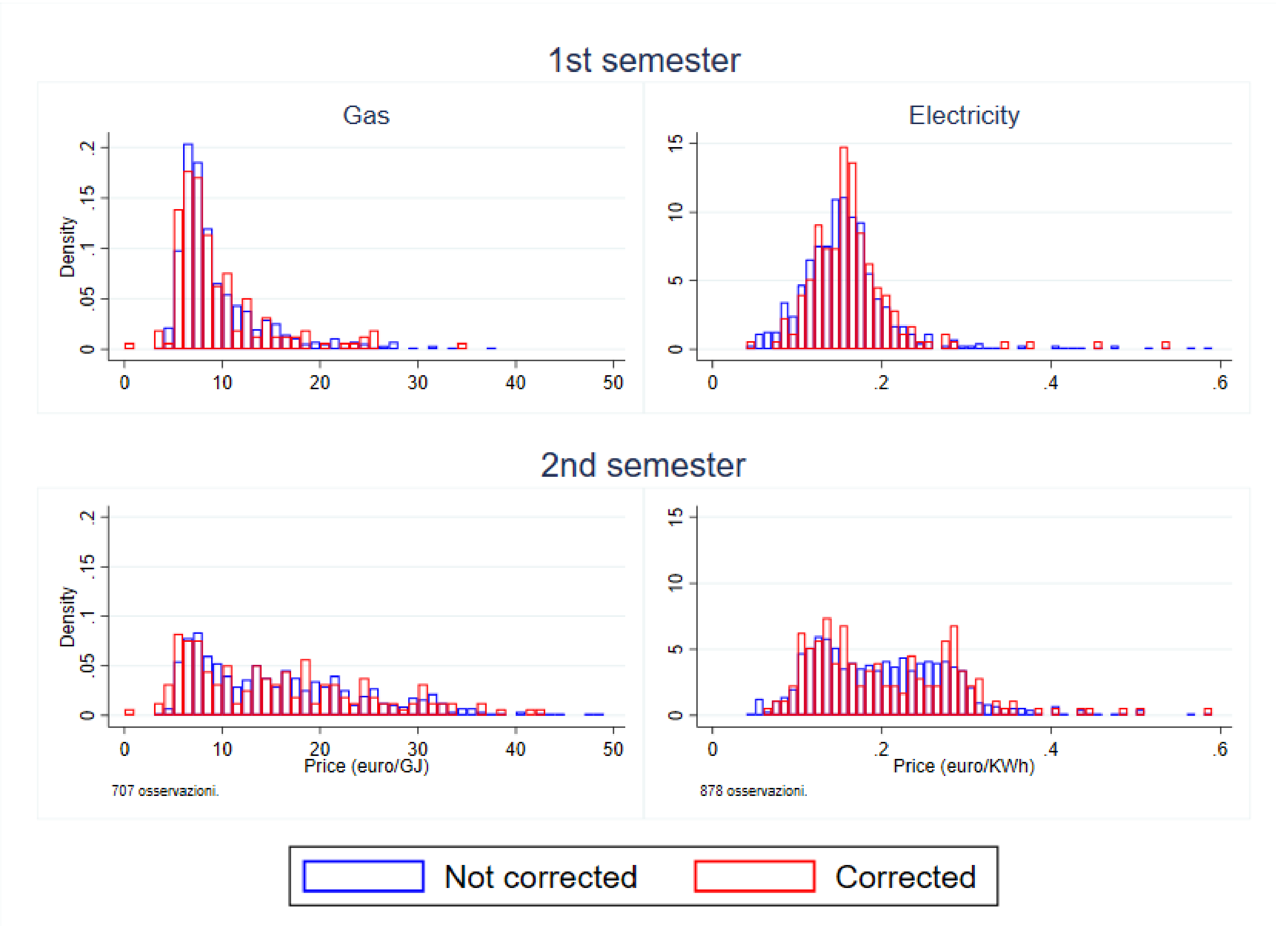
Figure A.1: Data validation: comparison against Eurostat data



Note: the figure reports mean unitary energy prices from the Invind sample (red) and Eurostat (blue) by semester and consumption class. Eurostat non-household consumption bands account for all taxes and levies and are categorized as follows; (i) for electrical energy: IA: customers consuming less than 20 MWh; IB: 20 MWh < consumption < 500 MWh; IC: 500 MWh < consumption < 2000 MWh; ID: 2000 MWh < consumption < 20000 MWh; IE: 20,000 MWh < consumption < 70,000 MWh; IF: 70,000 MWh < consumption < 150,000 MWh; IG: consumption > 150,000 MWh. (ii) for natural gas: I1 : consumption < 1 000 GJ; I2 : 1 000 GJ < consumption < 10 000 GJ; I3 : 10 000 GJ < consumption < 100 000 GJ; I4 : 100 000 GJ < consumption < 1 000 000 GJ; I5 : 1 000 000 GJ < consumption < 4 000 000 GJ; I6 : consumption > 4 000 000 GJ.

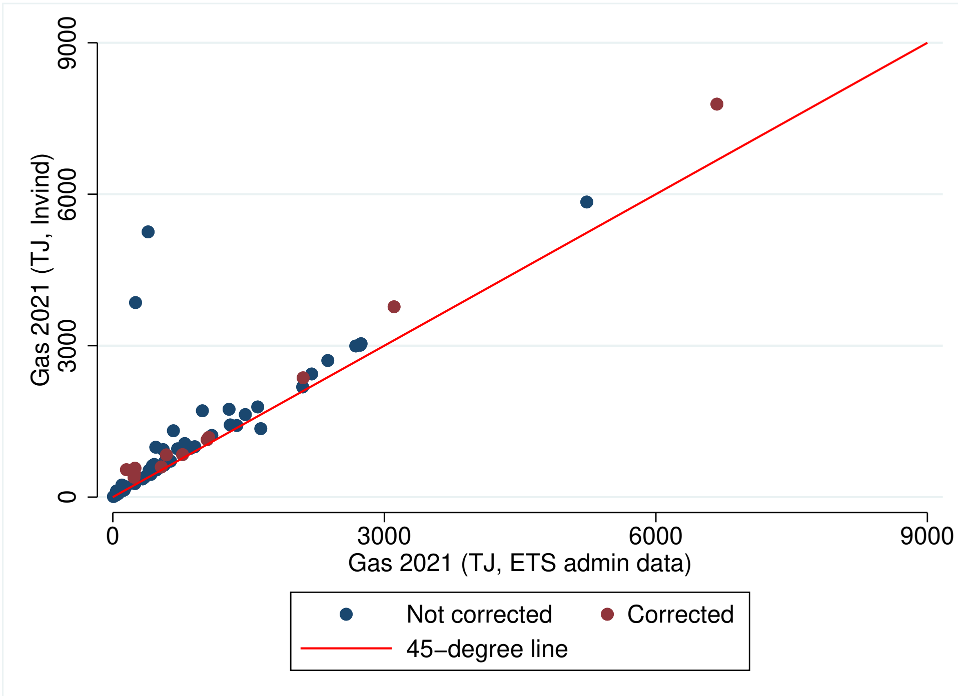
find an accurate correspondence, lending credence to the quality of our self-reported survey data.

Figure A.2: Data validation: comparison of corrected observations



Note: the figure reports the comparison between the distributions of gas and electricity unitary prices for two sets of observations: those that were rescaled and those that were not corrected. Own elaboration from Invind data.

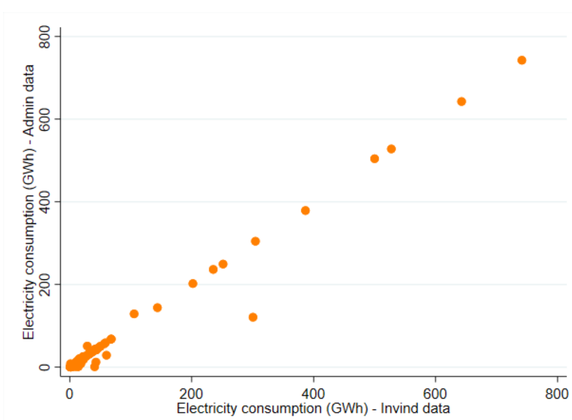
Figure A.3: Data validation: comparison of gas volumes from administrative data source



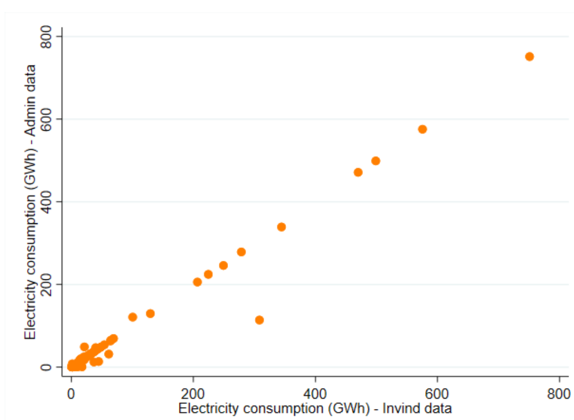
Note: the figure reports consumption of gas volumes (in TJ) for 59 firms present in both the Invind and the ETS samples. The administrative source of data on firms subject to the EU ETS is the Italian Institute for Environmental Protection and Research (ISPRA).

Figure A.4: Data validation: comparison of electricity volumes from administrative data source

(a) I semester



(b) II semester



Note: the figure reports the consumption volumes of electricity (in GWh) for 249 firms present in both the Invind sample and the set of energy-intensive companies registered at the Italian Fund for Energy and Environmental Services (CSEA).

Appendix C Nonresponse bias

One potential threat to the validity of our study is nonresponse bias, which may occur due to practical issues such as the firm not receiving or seeing the questionnaire, ignoring the invitation, or refusing to participate. This can result in differences between the target sample of the survey and the actual sample.

First, to check the severity of this issue, we compare key variables, such as the average unitary price for gas and electricity by consumption class, against a reliable benchmark from an external data source. Specifically, we use statistics from Eurostat to compare the averages of prices in our sample with the reference population of Italian firms. The results, reported in Appendix B provides some reassurance that our sample is not significantly distorted.

In what follows we provide additional analyses and discuss the extent to which the selection of respondents who decide to fill up the energy section of the questionnaire may distort our estimates.

C.a Non response to the energy section

As a first step, we consider the potential selection bias due to attrition during the compilation of the questionnaire. We observe that most respondents begin the energy section but drop out after the first question on insurance, possibly because the following questions require more effort. The questions on quantity and cost of electricity and gas consumption require a precise accounting system and/or the availability of energy bills, as illustrated in Figure A.1. In order to be included in the estimation sample the observation must report valid answers for all the questions on energy consumption and expenditure, as well as the insurance one, as described in Appendix B. Given that 49 percent of the sample of respondents does not reply to the energy section or provide missing or implausible information, it is important to assess the extent of selection in our sample.

We examine whether there exist differences in observables between the firms replying to the energy section of the questionnaire and those that did not. The selected attributes to be compared are observable for *all* respondents of the survey, such as salient firm-characteristics (reported in 2021 but referring to 2020) as turnover, labour force, hours worked, number of layoffs and hirings, and expenditure for intermediate goods; whether the firm belongs to the EU ETS and whether the firm is energy intensive (*energivora* status); class size, sector, and macroregion dummies.

Table A.1 shows the mean values of these variables in different samples, including the full sample (col. 1), the sample of firms starting the energy section and dropping out after the first question (col. (2)), the sample used in the analysis on the demand elasticity for electricity (col. (5)), and the sample used in the analysis on the demand elasticity for natural gas (col. (8)). Columns (3)-(4), (6)-(7), and (9)-(10) illustrate the respective differences from the full-sample means and the T-statistic. In the sample in column (2), the geographic distribution of firms is altered, with the Center and Southern regions being over-represented. Except for this North-South divide, there is no differential behaviour in the response to the question on insurance.

Moving to the estimation samples (column (5) onwards), respondents display slightly larger size in terms of sales (measured in 2020). With respect to the initial sample, in the estimation samples firms with fewer employees are under-represented. The distribution of sectors across the different samples is quite balanced, with the exception of chemicals, pharmaceuticals, and rubber businesses being slightly more represented in the estimation samples, and water and waste companies being less represented. The geographical distribution of firms on which we estimate the demand elasticity for gas and electricity appears to be tilted against the South of the country. Finally, in the estimation sample ETS and energy-intensive firms (*energivora* status) are over represented.

Given the observed imbalances in the sample selection due to attrition, we examine the robustness of our findings to a correction method called "inverse probability weighting"(Wooldridge et al., 2002; Stantcheva, 2022). This method is commonly used to address differential attrition by utilizing the relationships among observed covariates to re-weight the observed data to approximate the distribution in the full data set (Stantcheva, 2022; Glynn and Quinn, 2010). In practice, we run our IV baseline specification weighting observations by the inverse of the probability of being part of the respective estimation sample. The latter probability is obtained as the propensity score from estimating by logit equations 12 and 13, where X_i include all the variables used in the previous balance tests.

$$\mathbf{1}(\text{Electricity sample}_i) = X_i'\beta^e + \varepsilon_i \tag{12}$$

$$\mathbf{1}(\text{Gas sample}_i) = X_i'\beta^g + \xi_i \tag{13}$$

We report results for the propensity score estimation in Table A.2, while in Table A.3, we report both the baseline results (Column 1 and 4) and those obtained by inverse probability weighting (Columns 2 and 5). The two sets of results are very similar, which is reassuring. However, it should be noted that this empirical exercise addresses the issue of selection on

Table A.1: Sample-specific summary statistics

	(1) Full sample	(2) Insurance sample	(3)	(4)	(5) Electricity sample	(6)	(7)	(8) Gas sample	(9)	(10)
	mean	mean	Δ	t-stat	mean	Δ	t-stat	mean	Δ	t-stat
Sales in 2020	0.13	0.13	-0.01	(-0.36)	0.16	-0.06*	(-2.07)	0.19	-0.10**	(-2.98)
Costs for interm. goods in 2020	5.28	5.36	-0.44	(-0.30)	6.69	-2.61	(-1.63)	7.35	-3.28	(-1.74)
Labour force in 2020	349.18	347.41	9.48	(0.19)	406.38	-105.91*	(-2.00)	460.06	-175.96**	(-2.81)
Hours worked in 2020	0.52	0.51	0.03	(0.36)	0.59	-0.14*	(-2.06)	0.67	-0.24**	(-3.10)
Hirings in 2020	0.32	0.32	0.03	(0.53)	0.36	-0.08	(-1.41)	0.36	-0.06	(-0.97)
Separations in 2020	0.34	0.34	0.00	(0.05)	0.38	-0.07	(-1.33)	0.38	-0.06	(-1.08)
Status (energy intensive)	0.22	0.23	-0.03	(-1.44)	0.27	-0.09***	(-4.35)	0.29	-0.10***	(-4.93)
Subject to ETS in 2021	0.06	0.06	-0.01	(-0.39)	0.07	-0.03*	(-2.41)	0.10	-0.06***	(-4.66)
Food and beverages	0.13	0.14	-0.02	(-1.17)	0.13	0.01	(0.66)	0.12	0.02	(1.14)
Textiles & apparel	0.09	0.09	0.01	(0.35)	0.09	0.01	(0.41)	0.09	0.00	(0.33)
Chem., pharma., rubber	0.13	0.14	-0.03	(-1.34)	0.16	-0.05**	(-3.11)	0.16	-0.04*	(-2.50)
Non-metallic minerals	0.04	0.04	-0.01	(-1.16)	0.05	-0.01	(-0.95)	0.05	-0.01	(-0.74)
Metalworking industry	0.44	0.43	0.05	(1.75)	0.44	-0.00	(-0.12)	0.45	-0.01	(-0.27)
Wood, paper, furniture	0.11	0.10	0.01	(0.35)	0.09	0.02	(1.57)	0.11	-0.00	(-0.20)
Water & waste	0.05	0.05	-0.00	(-0.27)	0.04	0.02*	(2.39)	0.03	0.04***	(3.87)
50-99 employees	0.34	0.34	-0.01	(-0.38)	0.29	0.09***	(4.12)	0.26	0.13***	(6.12)
100-199 employees	0.28	0.28	0.03	(0.99)	0.27	0.03	(1.36)	0.26	0.04	(1.74)
200-499 employees	0.23	0.23	-0.01	(-0.32)	0.26	-0.05*	(-2.45)	0.27	-0.06**	(-2.79)
500-999 employees	0.08	0.08	-0.02	(-0.99)	0.10	-0.05***	(-3.71)	0.12	-0.07***	(-4.61)
1000 and more employees	0.06	0.06	0.01	(0.43)	0.08	-0.02	(-1.93)	0.10	-0.05***	(-3.80)
North-West	0.30	0.28	0.15***	(5.18)	0.30	0.01	(0.55)	0.33	-0.05*	(-2.09)
North-Est	0.23	0.21	0.09***	(3.45)	0.24	-0.01	(-0.61)	0.28	-0.08***	(-3.86)
Center	0.22	0.24	-0.11***	(-4.96)	0.26	-0.07***	(-3.40)	0.24	-0.03	(-1.48)
South and Islands	0.25	0.27	-0.14***	(-6.35)	0.21	0.07***	(3.32)	0.15	0.16***	(8.22)
Observations	1844	1500			848			682		

Note: The table presents sample means for a set of firm characteristics in the full (Invind) sample (col. (1)) and three different sub-samples (col. (2)-(5)-(8)). The full sample of respondents to the 2021 Invind wave, net of those belonging to NACE 35 and 19, consists of 1844 firms, as described in Appendix D. The insurance sample refers to companies also replying to the question *"at the beginning of 2021 the company had (even if partial) hedging tools against the rising energy prices that occurred in the second half of 2021"*. The electricity sample and the gas sample are our estimation samples, i.e. reporting valid answers for all the questions on the respective input consumption and expenditure, together with the insurance question, as described in Appendix B. In col. (3)-(4), (6)-(7), and (9)-(10), Δ indicates the difference between the mean value of a given sample and the corresponding mean in the full sample. T-stat is the t-test statistic for the difference in means. Stars indicate conventional significance levels: * is 10%, ** is 5% and *** is 1%.

observables. Yet, we are still exposed to the threat of selection on unobservable , something we try to address in the next section.

C.b Differential nonresponse between insured vs. non-insured

One issue that remains unresolved is the possibility that insured companies may be more or less likely to respond to the energy section of the survey compared to uninsured companies. This could pose a threat to our identification strategy, which relies on insurance status as our instrument. Even if price-protection instruments were randomly assigned in the population of interest, differential response rates between insured and uninsured could be problematic.

Table A.2: Determinants of replying to the energy section of the questionnaire

	Electricity sample	Gas sample
Labour force in 2020	0.0001	0.0000
	(0.0001)	(0.0001)
Hours worked in 2020	-0.1101	-0.0843
	(0.0789)	(0.0728)
Costs for intermediate goods in 2020	0.0004	-0.0005
	(0.0009)	(0.0009)
Sales in 2020	0.0643	0.1462**
	(0.0668)	(0.0708)
Hirings in 2020	0.0258	0.0168
	(0.0392)	(0.0374)
Separations in 2020	-0.0230	-0.0347
	(0.0386)	(0.0379)
Status (energy intensive)	0.1047***	0.1002***
	(0.0313)	(0.0299)
Subject to ETS in 2021	-0.0093	0.0812
	(0.0571)	(0.0545)
Food and beverages	0.1072	0.1420*
	(0.0672)	(0.0726)
Textiles & apparel	0.0831	0.1256*
	(0.0704)	(0.0750)
Chem., pharma., rubber	0.1689**	0.1760**
	(0.0671)	(0.0716)
Non-metallic minerals	0.1154	0.1257
	(0.0838)	(0.0872)
Metalworking industry	0.1219**	0.1496**
	(0.0605)	(0.0663)
Wood, paper, furniture	0.0463	0.1420*
	(0.0694)	(0.0737)
50-99 employees	-0.2056**	-0.2932***
	(0.0813)	(0.0780)
100-199 employees	-0.1700**	-0.2450***
	(0.0798)	(0.0764)
200-499 employees	-0.0847	-0.1576**
	(0.0760)	(0.0723)
500-999 employees	0.0241	-0.0325
	(0.0765)	(0.0720)
North-West	0.0082	0.1441***
	(0.0339)	(0.0345)
North-Est	0.0410	0.1941***
	(0.0358)	(0.0359)
Center	0.1341***	0.1796***
	(0.0359)	(0.0363)
Observations	1844	1844

Note: The table presents the results of a logit regression of the the binary indicator for being the two energy-related samples of interest on a set of covariates that are observed for the full sample of respondents to the survey. The reported coefficients are marginal effects indicating the change in probability of being in the energy-related sample that is observed for a unitary increase in the independent variable. The reference omitted categories are 1000 and more employees for firm size; the water & waste sector; and the South and Islands region.

Table A.3: Robustness test on price-elasticity estimates: inverse probability weighting

	(1) Electricity	(2) Electricity	(3) Electricity	(4) Gas	(5) Gas	(6) Gas
$\Delta \log P$ electricity	-0.0286	-0.0234	0.0113			
	[-0.216,0.159]	[-0.210,0.163]	[-0.163,0.186]			
$\Delta \log P$ gas				-0.183	-0.265	-0.0526
				[-0.627,0.261]	[-0.614,0.0842]	[-0.350,0.244]
Observations	848	848	848	682	682	682
Inverse probability weighting	NO	YES	YES	NO	YES	YES
Controls	NO	NO	YES	NO	NO	YES
K-P F stat	76.14	71.41	80.68	13.13	14.79	16.38
AR confidence set	[-.213866, .164186]	[-.200178, .175871]	[-.154457, .19821]	[-.712454, .327942]	[-.723673, .094766]	[-.358549, .301348]

95% confidence intervals in brackets
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table presents IV regressions at the firm-level of the semester-on-semester log changes in purchased electricity (and gas) against semester-on-semester log changes in the average unit price of electricity (and gas). Self-generation is excluded as it is not purchased. Given the log-log specification, the coefficient is directly interpretable as an elasticity. Confidence intervals are based on t-tests with robust standard errors. AR confidence intervals at the bottom of the table are based on Anderson Rubin tests (Andrews et al., 2019) and are robust to the inclusion of weak instruments. While Column (1) and (4) report our baseline specification of Table 2 and 3 as reference, Column (2)-(3) and (5)-(6) illustrate the results of IV regressions weighting units with the inverse of the probability of belonging to the estimation sample, as in Stantcheva (2022). Column (3) and (6) also include the following control variables: sales, employment, costs, hiring and separations in year $t-1$, dummies for five size classes, sector and macroregion fixed effects, whether the firm measures emissions, whether the firm belongs to the EU ETS and whether the firm is energy intensive (*energivora* status).

If insurance status influences the likelihood of responding to the rest of the survey, we are uncertain about the extra participants' impact on the outcome distribution. For example, if a company's lack of insurance results in skyrocketing prices, they may be less likely to respond to the survey due to other pressing concerns. In this scenario, our IV strategy would underestimate the first stage, and the second stage could change in any direction depending on the firm's quantity response.

To address this issue, we regress a dummy variable equal to one if the firm did not answer the energy-related questions on two binary indicators: one indicating whether the respondent states that the company is insured, while the other indicates whether the respondent states that the company is uninsured against energy price shocks. Non-repliers to the insurance question form the omitted category. We can perform this regression because, as previously stated, a large number of firms (1500) answered the first question on insurance status, while far fewer firms responded to the subsequent questions on energy consumption and expenditure (848 for electricity and 682 for gas). Table A.4 presents the findings of regressions (14) and (15), both with and without accounting for the set of controls discussed in Section C.a.

$$\mathbf{1}(\text{Not in electricity sample}_i) = \theta_0 + \theta_1 \text{Insured}_i + \theta_2 \text{Not insured}_i + \varepsilon_i \tag{14}$$

$$\mathbf{1}(\text{Not in gas sample}_i) = \theta_0 + \theta_1 \text{Insured}_i + \theta_2 \text{Not insured}_i + \varepsilon_i \tag{15}$$

Results show that insured firms are more likely to end up in the estimation sample, both relative to firms not responding to the insurance question, and to uninsured firms.

Table A.4: Insurance status and non-reply

	(1)	(2)	(3)	(4)
	Electricity sample		Gas sample	
Insured	-0.637***	-0.630***	-0.542***	-0.550***
	[-0.672,-0.602]	[-0.667,-0.592]	[-0.578,-0.505]	[-0.589,-0.511]
Not Insured	-0.497***	-0.522***	-0.372***	-0.427***
	[-0.532,-0.461]	[-0.560,-0.483]	[-0.406,-0.337]	[-0.465,-0.389]
$H_0 : \theta_1 - \theta_2 = 0$, p-value	0.00	0.00	0.00	0.00
Observations	1844	1844	1844	1844
Controls	NO	YES	NO	YES

Note: The table presents regressions at the firm-level of a dummy equal to one if the firm is not responding to the energy-related questions against two binary indicators: one equal to unity if the respondent states that the company is insured, and the other equal to unity if the respondent states that the company is not insured against energy price shocks. Columns (2) and (4) include the following controls: as turnover, labour force, hours worked, number of layoffs and hirings, and expenditure for intermediate goods reported in 2021 but for the previous year; whether the firm belongs to the EU ETS and whether the firm is energy intensive (*energivora* status); class size, sector, and macroregions.

C.c Addressing differential nonresponse

One concern arising from the previous analysis is that differential non-response rates between insured and uninsured firms could be induced by unobservables which also explain differential performances between insured and uninsured firms. Since we cannot address selection on unobservables directly, we try to bound our treatment effects to best and worst-case scenarios, thanks to a partial identification approach proposed by Lee (2009). The approach involves identifying the additional share of firms, denoted as s, that responded to the price-quantity table because they were insured. As it is not possible to reintroduce unobserved uninsured firms, we proceed to trim observations from the outcome distribution among the insured firms, first below the s-th percentile and then above the $1 - s$-th percentile. Under an intuitive monotonicity assumption, whereby treatment assignment affects participation into the sample in one direction for all units, this procedure provides bounds for the average treatment effect of insurance on our outcomes of interest. Intuitively, we are testing whether results are different when the group of "sample compliers" (those induced by the instrument to respond to energy questions) comes from the upper part or the lower part of the outcome distribution. Lee (2009) developed this method for the case where the treatment is (conditionally) random. Given that we are working in an IV framework, we apply his procedure separately for the reduced form and the first stage.

Lee (2009) bounds for electricity Let us start form the first stage, so the outcome is the log change in electricity price in this case. Recall that in this application insured units ($Z = 1$) are more likely to be part of the sample than uninsured ones ($Z = 0$), as reported in Table A.4. In the first polar case, where the missing uninsured (MU) firms are those who had the highest price increase conditional on $Z = 0$, we should trim the top of the insured distribution, and thus this would strengthen our first stage. In the opposite case, where the MU are those who had the lowest price increase among the uninsured, we should trim the bottom of the insured distribution, and thus this would weaken our first stage. In the first case, a possible interpretation is that uninsured firms that experienced the worst price shock were too busy dealing with the crisis to bother replying to the rest of the survey. In the second case, the interpretation is that firms that experienced the weakest price shock (conditional on being uninsured) did not think the energy crisis was important enough to deserve the time needed to reply to the next section of the survey. The corresponding estimated first-stage bounds for electricity are -31.5 and -9; thus in both cases the first stage works in the appropriate direction, which is reassuring. We find the first case more plausible.

Combining first-stage and reduced forms bounds, we get four IV bounds (see Table A.5). To assess the plausibility of the assumptions behind each polar case, it is necessary to characterize the MU in each of them. In the case where MU have experienced the largest price increase and have reduced demand the most conditional on being uninsured (south-west corner in Table A.5), the missing units can be characterized as elastic firms. In this case, elasticity would be bounded at -0.24, which is definitely higher than our baseline without controls (-0.03), but still a relative small value. In the case where MU have experienced the largest price increase and have reduced demand the least conditional on being uninsured (south-west corner in Table A.5), the missing units can be thought as very inelastic firms (or even with positive elasticity). This yields a positive elasticity bound, which is not very reasonable, and can be discarded. Let us now move to the upper part of Table A.5. Firms in the north-west corner can be characterized as the most elastic, since they have decreased demand the most, while having experienced the smallest price shock, conditional on being uninsured. In this case, the true elasticity would be very high (-0.86), completely reverting the conclusion of our paper. However, we find this possibility unlikely, as we expect that (at least some) firms so sensitive to small energy price changes should be willing and able to answer our questions on energy consumption. Finally, MU in the north-east corner have experienced the lowest price increase, and decreased demand the least, conditional on being uninsured; in this case the bound yields a positive elasticity close to unity, which is unreasonable and can thus be discarded.

Lee (2009) bounds for gas When the outcome is natural gas, the first stage holds only in the case where MU are assumed to have experienced the largest price increase among uninsured (lower row of Table A.6), which we consider a more plausible scenario than the opposite one

Table A.5: IV bounds for different hypothesis on the missing uninsured (MU) - Electricity

		ΔQ experienced by the MU are...	
		the lowest	the highest
ΔP experienced by the MU are...	the lowest	$\frac{7.8}{-9} = -0.86$	$\frac{-8.7}{-9} = +0.96$
	the highest	$\frac{7.8}{-31.5} = -0.24$	$\frac{-8.7}{-31.5} = +0.28$

Note: figures at the numerator refer to the reduced form estimates, those at the denominator at the first-stage estimates.

(upper row). The failure of the first stage in the latter case is not surprising in light of the fact that the instrument is in general much weaker when the outcome is natural gas, as already discussed in the paper.

Let us now focus on the lowest row. In the case where MU can be characterized as elastic firms (south-west corner in Table A.6), elasticity would be bounded at -0.5, which is much higher than our baseline without controls (-0.18). In the case where MU are very inelastic (south-east corner), the elasticity would be positive (0.3). We conclude that for natural gas, the bounds are not very informative.

Table A.6: IV bounds for different hypothesis on the missing uninsured (MU) - Gas

		ΔQ experienced by the MU are...	
		the lowest	the highest
ΔP experienced by the MU are...	the lowest	$\frac{20}{7} = +2.8$	$\frac{-14}{7} = -2$
	the highest	$\frac{20}{-42} = -0.5$	$\frac{-14}{-42} = +0.3$

Note: figures at the numerator refer to the reduced form estimates, those at the denominator at the first-stage estimates.

Appendix D Additional results

D.a OLS vs IV results

In this section we discuss the differences between OLS and IV in our setting and report both OLS and IV estimates of the elasticity of interest, both with and without controls.

There are several potential explanations for the differences between OLS and IV. First, OLS estimates might be biased due to the endogeneity of prices and quantity. As explained in the paper, in our application changes in prices surely reflect in part the large supply-side shocks that took place in the European wholesale market. However, they might also reflect firm-level demand shocks. In general, the OLS coefficient exploits variation generated by both supply and demand shocks, and thus fail to identify the elasticity of demand. Note that demand shocks make price and quantity negatively correlated. In fact, the price in the retail energy market is a decreasing function of purchased quantities (see Figure A.1), due to the presence of some energy bill components that are not a function of the purchased quantity (e.g. for maximum available power capacity) and due to higher bargaining power by larger consumers. As such in our application, even if the true elasticity of demand is zero (as suggested by the IV), we would expect the sign of the OLS estimate to be negative.

This is exactly what we find in Tables A.1 and A.2: in the case of electricity, the OLS estimates (-0.15) are negative and larger than IV (-0.03), and statistically different from zero. Similar differences arise in the natural gas regressions, especially when focusing on those that include covariates.

Lacking data on specific sub-components of the energy bill, in order to quantify how much the presence of fixed components and bargaining power affects the OLS estimate, we compare how much the price varies across consumption classes using Eurostat data from before the crisis (the same data depicted in Figure A.1). The seven consumption classes covers the entire spectrum of firms ranging from small entrepreneurs to large energy-intensive establishments, and thus we believe that price differences across those mostly reflect bargaining power and incidence of fixed components, rather than quantity responses to price changes. Concretely, we regress the log of electricity (or gas) price of a given consumption class against the log of the mid-point energy consumption for that same class, before the crisis ($N = 7$). The OLS coefficient in this case is -0.14, which is almost identical to the OLS coefficient in our application.

A second explanation for the differences between the two estimators is that OLS implicitly weights observations by the inverse of variance (yielding an ATE type of estimate), while

IV implicitly weights by the strength of the first-stage (yielding a LATE type of estimate). As such, in presence of treatment heterogeneity, these two estimates might differ even in absence of endogeneity. Finally, there is always the possibility that the exclusion restriction is violated, which would bias the IV estimate. To the extent that in the paper we build a convincing case for the exclusion restriction to hold, we believe that using IV is the most credible way to identify the elasticity of interest.

Table A.1: Price-elasticity of electricity demand: OLS vs IV estimates

	(1) OLS	(2) IV	(3) OLS	(4) IV
$\Delta \log P$ electricity	-0.154***	-0.0286	-0.146***	0.0152
	[-0.206,-0.101]	[-0.216,0.159]	[-0.198,-0.0945]	[-0.166,0.196]
Observations	848	848	848	848
Controls	NO	NO	YES	YES
K-P F stat		76.14		73.84
AR confidence set		[-.213866, .164186]		[-.156729, .208986]

95% confidence intervals in brackets

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table presents OLS and IV regressions at the firm-level of the semester-on-semester log changes in purchased electricity against semester-on-semester log changes in the average unit price of electricity. Given the log-log specification, the coefficient is directly interpretable as an elasticity. Confidence intervals are based on t-tests with robust standard errors. AR confidence intervals at the bottom of the table are based on Anderson Rubin tests (Andrews et al., 2019) and are robust to the inclusion of weak instruments. Columns (3) and (4) include the following controls: dummies for five size classes, sector dummies, dummies for geographical macroregions, 2020 sales, 2021 employment, whether the firm self-generates electricity, whether the firm measures emissions, whether the firm belongs to the EU ETS and whether the firm is energy intensive (*energivora* status).

Table A.2: Price-elasticity of gas demand: OLS vs. IV estimates

	(1) OLS	(2) IV	(3) OLS	(4) IV
$\Delta \log P$ gas	-0.150***	-0.183	-0.112***	-0.00645
	[-0.208,-0.0928]	[-0.627,0.261]	[-0.168,-0.0561]	[-0.431,0.418]
Observations	682	682	682	682
K-P F stat		13.13		12.58
AR confidence set		[-.712454, .327942]		[-.46118, .551239]

95% confidence intervals in brackets

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table presents OLS and IV regressions at the firm-level of the semester-on-semester log changes in purchased natural gas against semester-on-semester log changes in the average unit price of natural gas. Given the log-log specification, the coefficient is directly interpretable as an elasticity. Confidence intervals are based on t-tests with robust standard errors. AR confidence intervals at the bottom of the table are based on Anderson Rubin tests (Andrews et al., 2019) and are robust to the inclusion of weak instruments. Columns (3) and (4) include the following controls: dummies for five size classes, sector dummies, dummies for geographical macroregions, 2020 sales, 2021 employment, whether the firm self-generates electricity, whether the firm measures emissions, whether the firm belongs to the EU ETS and whether the firm is energy intensive (*energivora* status).

D.b Weighted estimates

Table A.3: Price-elasticity of electricity demand (weighted IV estimates)

	(1) Baseline	(2) Class size FE	(3) Sector FE	(4) Macroregions FE	(5) Controls	(6) All
Panel (a) : Demand equation						
$\Delta \log P$ electricity	0.0158	0.0213	0.0369	0.0192	0.0159	0.0488
	[-0.207,0.238]	[-0.200,0.243]	[-0.183,0.257]	[-0.201,0.240]	[-0.210,0.242]	[-0.177,0.274]
Panel (b) : First stage estimates						
Protected from price increase (0/1)	-17.88***	-17.86***	-17.84***	-18.09***	-18.56***	-18.46***
	[-23.21,-12.55]	[-23.20,-12.52]	[-23.18,-12.50]	[-23.51,-12.68]	[-23.88,-13.24]	[-23.81,-13.12]
Observations	848	848	848	848	816	816
K-P F stat	43.34	43.12	43.01	43.01	46.93	45.92

95% confidence intervals in brackets

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table reports IV estimates for the price-elasticity of demand for electricity.

Note: The table presents IV regressions at the firm-level of the semester-on-semester log changes in purchased electricity against semester-on-semester log changes in the average unit price of electricity. Self-generation is excluded as it is not purchased. Given the log-log specification, the coefficient is directly interpretable as an elasticity. confidence intervals in panel (a) are based on t-tests with robust standard errors. AR confidence intervals at the bottom of the table are based on Anderson Rubin tests (Andrews et al., 2019) and are robust to the inclusion of weak instruments. Column (1) includes no control variables. Column (2) includes dummies for five size classes: [50-99 employees], [100-199 employees], [200-499 employees], [500-999 employees], [1000 and more employees]. Column (3) includes 11 sector dummies: food and beverages; textiles and apparel; chemicals, pharma and rubber; non-metallic minerals; metalworking industry; wood, paper and furniture; water and waste. Column (4) includes four dummies for geographical macroregions: North-West, North-East, Center, South and Islands. Column (5) includes a series of firm-level controls: 2020 sales, 2021 employment, whether the firm self-generates electricity, whether the firm measures emissions, whether the firm belongs to the EU ETS and whether the firm is energy intensive (*energivora* status). Column (6) includes all of these controls at the same time. Regressions are weighted using survey weights.

Table A.4: Price-elasticity of gas demand (weighted IV estimates)

	(1) Baseline	(2) Class size FE	(3) Sector FE	(4) Macroregions FE	(5) Controls	(6) All
Panel (a) : Demand equation						
$\Delta \log P$ gas	-0.172	-0.141	-0.0845	-0.160	-0.0687	-0.0470
	[-0.735,0.390]	[-0.696,0.414]	[-0.616,0.447]	[-0.710,0.391]	[-0.631,0.494]	[-0.571,0.477]
Panel (b) : First stage estimates						
Protected from price increase (0/1)	-13.41***	-13.51***	-13.47***	-13.71***	-13.93***	-14.22***
	[-23.30,-3.519]	[-23.48,-3.547]	[-23.35,-3.600]	[-23.58,-3.833]	[-24.22,-3.644]	[-24.61,-3.830]
Observations	682	682	682	682	650	650
K-P F stat	7.085	7.088	7.180	7.430	7.072	7.225

95% confidence intervals in brackets

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table reports IV estimates for the price-elasticity of demand for electricity.

Note: The table presents IV regressions at the firm-level of the semester-on-semester log changes in purchased natural gas against semester-on-semester log changes in the average unit price of natural gas. Self-generation is excluded as it is not purchased. Given the log-log specification, the coefficient is directly interpretable as an elasticity. confidence intervals in panel (a) are based on t-tests with robust standard errors. AR confidence intervals at the bottom of the table are based on Anderson Rubin tests (Andrews et al., 2019) and are robust to the inclusion of weak instruments. Column (1) includes no control variables. Column (2) includes dummies for five size classes: [50-99 employees], [100-199 employees], [200-499 employees], [500-999 employees], [1000 and more employees]. Column (3) includes 11 sector dummies: food and beverages; textiles and apparel; chemicals, pharma and rubber; non-metallic minerals; metalworking industry; wood, paper and furniture; water and waste. Column 4 includes four dummies for geographical macroregions: North-West, North-East, Center, South and Islands. Column (5) includes a series of firm-level controls: 2020 sales, 2021 employment, whether the firm self-generates electricity, whether the firm measures emissions, whether the firm belongs to the EU ETS and whether the firm is energy intensive (*energivora* status). Column (6) includes all of these controls at the same time. Regressions are weighted using survey weights.

Table A.5: Outcome: percentage change in own price relative to previous year. Weighted estimates.

	(1)	(2)	(3)	(4)	(5)	(6)
2021	6.88***	6.23***	6.30***	6.87***	5.82***	6.98***
	(0.86)	(0.85)	(0.82)	(1.12)	(0.99)	(1.01)
2021 x Z	-0.28	-1.11	0.22	-0.06	0.31	-0.45
	(1.10)	(1.11)	(1.08)	(1.47)	(1.32)	(1.31)
2021 x Energivora		3.08				
		(2.68)				
2021 x Z x Energivora		2.11				
		(3.15)				
2021 x ETS			15.08**			
			(7.46)			
2021 x Z x ETS			-13.32*			
			(7.91)			
2021 x intense (over tot. cost)				1.16		
				(2.55)		
2021 x Z x intense (over tot. cost)				-1.10		
				(3.14)		
2021 x intense (over turnover)					5.34*	
					(2.84)	
2021 x Z x intense (over turnover)					-2.77	
					(3.46)	
2021 x Large						-0.50
						(1.84)
2021 x Z x Large						0.79
						(2.28)
Observations	3948	3948	3948	2832	2885	3948

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table presents OLS regressions at the firm-level of percentage output price changes in the previous 12 months against firm effects, time effects, time effects interacted with the protection dummy Z_i, time effects interacted with different firm characteristics (one at the time) and the triple interaction of time effect, firm characteristics and protection dummy. Regressions are weighted by survey weights.