A retrospective evaluation of the GDF/Suez merger: Effects on the Belgian gas hub

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Abstract: We present an ex-post analysis of the effects of GDF's acquisition of Suez in 2006, which created one of the world's largest energy companies. We perform a series of econometric analyses on the market for trading at the Zeebrugge gas hub in Belgium. Removing barriers to entry and facilitating access to the hub through ownership unbundling were an important part of the objectives of the remedies imposed by the European Commission. Our analyses show a robust price decline after the merger. Additional evidence on traded volumes and hub participants is in line with an increased liquidity at the hub post-merger. This suggests the remedies were effective in limiting the potential anti-competitive effects of the merger. Moreover, it suggests that ownership unbundling has generated improved access to the hub. Therefore, the remedies may have done more than simply mitigate the potential anti-competitive effects of the merger; they may have effectively created competition.

Keywords: Mergers, Ex-post Evaluation, Gas sector, Hub prices, trade volumes and participants
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1. Introduction

Competition policy enforcement activity in the EU energy sectors has increased significantly over the last fifteen years, especially with regards to merger cases. Between 2000 and 2014, the European Commission (EC) has handled over 270 merger cases in the energy sectors, as opposed to just 20 in the five years prior to the year 2000 (European Commission, 2015).[1] In some of these decisions, the EC required merging parties to offer substantive remedies to, in principle, offset the anti-competitive effects of the merger (see e.g. Polemis, 2018).

In reality, though, some of these remedies may have gone further than that. As argued by many commentators, they may have been used to promote market liberalisation and, in particular, to achieve effective unbundling of network and supply activities. The EC's view is that the EU liberalization directives in energy have not been wholly effective (Federico, 2011).[2] Member States have forced a number of compromises to defend their national energy champions, which limited or delayed the EU directives' effectiveness. Ensuring fair and transparent third-party access to transport facilities, and non-discrimination against non-vertically integrated firms, have been especially difficult (Harrison and Mordaunt, 2012).

In response, the EC is said to have tried to "overcome significant obstacles to Europeanization endemic to the energy sector" (Eberlein, 2012) by using "windows of opportunities" created by large European cross border merger proposals (Pakalkaite, 2014). Others argue that, indeed, the EC is shaping the overall development of the EU energy markets through its merger policy decisions, and that the energy sector has been taken "out

[1] Moreover, since 2003, a significantly higher share of the merger cases in gas and electricity has received in-depth investigations. As a result, some mergers have been prohibited (such as the acquisition of Gas de Portugal by Energias de Portugal and ENI), while others have been abandoned (such as the acquisition of the Hungarian MOL by the Austrian company OMV).

[2] In contrast to the view of the EC, the (theoretical) academic literature does not provide unambiguous support of the positive effects of ownership unbundling, neither in terms of consumer welfare or in terms of its effects on investment incentives (Bolle and Breitmoser, 2006, Crémer et al., 2006, and Pollitt, 2008).

of the domain of (national) sector-specific regulation and put under the auspices of (EU) competition policy" (Hellwig, 2009).

This paper provides a quantitative ex-post analysis of one of the most important mergers in the EU energy sector in recent decades, the Gaz de France's (GDF) acquisition of Suez in 2006. We analyse whether the merger and the associated remedies imposed by the EC restored, or indeed improved, initial market outcomes. We focus on the impact on the market for trading in Belgium's Zeebrugge (ZEE) hub.[3] The ZEE hub is a key part of the European gas market and an important target of the remedies imposed by the EC. Indeed, prior to the merger, the ZEE hub had suffered from limited infrastructure access and liquidity issues. Part of the remedies, which included ownership unbundling, aimed to free up access to the hub. If effective, the remedies should have allowed for a higher liquidity through more entry, higher traded volumes, and lower prices in the hub. This is not only important for the stakeholders of the wholesale hub gas market but also for end-users and consumers, as wholesale prices affect retail prices.[4]

A series of Difference-in-Difference (DiD) analyses, which compare the evolution of prices at the ZEE hub relative to different counterfactual hubs before and after the merger, suggests that the remedies did more than successfully limit the merger's potential anti-competitive effects. Indeed, we find that not only did prices not increase, but they *declined*. This decline in prices supports the view that ownership unbundling improved access to the hub. We also

[3] Gas-trading hubs provide services to facilitate exchanges between buyers and sellers in wholesale markets, enabling them to find sufficient volumes of supplies or to sell excess capacity in the short-term. A hub, if operating efficiently, can promote competition in the market and allow for price transparency. This, in turn, results in a reduction in transaction costs, a more secure supply and a wider choice for buyers. A hub can be a physical installation, where gas flows are connected to and pass through this point (as in Belgium) or they can be virtual whereby no precise geographical location is specified (as in the UK or in the Netherlands). For an interesting account of the development of gas hubs in Europe, see Miriello and Polo (2015).

[4] Indeed, a well-functioning hub, with prices correctly reflecting demand and supply conditions, are also considered key for the entry of retail gas players (Keyaerts et al. 2009). As further explained by Stern and Rogers (2012), after the dramatic fall in wholesale gas hub prices in the late 1980s, regulators refused to allow gas utilities in the US to pass through their high long-term contractual prices with producers to their customers. In the UK, British Gas had also contracted with producers at prices higher than the evolving wholesale hub price in the 1990s, and lost substantial numbers of customers as hub prices began to fall.

provide evidence of the improvement of other outcomes at the ZEE hub in the post-merger period. In particular, we find evidence of increased entry, trading volumes and investment. Therefore, the remedies may have gone further than mitigating the potential anti-competitive effects of the merger. Given the absence of indicated efficiency gains by the merging parties, our analysis supports the view that the EC's merger policy actions may have been used as tools to improve market outcomes in the EU energy markets.

Our paper provides, to the best of our knowledge, the first quantitative analysis of the ex-post effects of a merger and its associated remedies in the energy sectors.[5] Despite the importance of merger policy in energy markets, we are not aware of any retrospective study that provides an ex-post evaluation of the EC's merger decisions.[6] This may be because of complex market structures, or due to the specific technical features of energy markets. Notwithstanding these difficulties, retrospective studies are essential tools to assess past competition policy decisions and improve decision-making for future cases.

There are some studies providing analyses of competition policy enforcement decisions in energy markets. However, most of the existing studies provide a qualitative, rather than a quantitative, analysis. Leveque (2006) argues that competition policy in the energy sector raises specific problems which require tailored solutions and call for a tougher approach to mergers.[7] Newbery (2007) compares the approach taken by the EU and the US regarding merger analysis in the energy sectors. He claims that, in contrast to the US, mergers between

[5] Several papers, on the other hand, have performed ex-ante simulations of the effects of potential mergers and divestitures on electricity prices (see e.g., Brown and Eckert, 2018, Morris and Oska, 2008, and Wolak and McRae, 2008). Apart from trying to predict prices, the simulation approach substantially differs from ours as it needs more assumptions to predict prices: it requires structural assumptions on both the demand side and the supply side, as well as an estimation of demand parameters.

[6] A growing number of retrospective studies analyse the effects of mergers in a large variety of other industries. See Ashenfelter, Hosken and Weiberg (2009) and Duso (2012) for reviews of the literature. Most of these studies only consider price effects, while we also have some evidence on other outcome variables.

[7] An interesting debate was sparked by Gas Natural's launch of a hostile bid on Endesa's shares in 2005. Two groups of leading academic scholars debated regarding the approach the EC should be adopting when facing a merger. Barquin et al. (2006) called for a stricter approach to be taken in the case of electricity mergers whereas Padilla et al. (2005) argued the opposite.

energy companies in Europe have been traditionally subject to rather relaxed standards (up until 2005). Pozzi (2004) shows that antitrust enforcement activity in a given year in the US electricity sector has a negative effect on industry profits in subsequent years.

There are also few studies that analyse the performance of wholesale gas markets. Most of the existing analyses focus on assessing the degree of integration between hubs, which is, in itself, an important goal of the EU energy strategy. Heather (2012) and Petrovich (2013), for example, examine the integration of gas hubs using price correlations. Rupérez Micola and Bunn (2007) test for the existence of market power on the arbitrages performed in the "Interconnector" pipeline linking the gas hubs in Belgium and the UK. Massol and Banal-Estanol (2018) develop a methodology to examine the overall efficiency of the arbitrages performed between gas hubs and use the same Interconnector pipeline as a case study.

The structure of this paper is as follows. In section 2, we review the EC's GDF and Suez merger within the context of the ZEE hub. We then present the data and the empirical analysis of the impact on the market for trading on the ZEE hub in section 3. Section 4 discusses our main results on the effect of the merger on prices at the ZEE hub. In section 5 we discuss limitations and several robustness checks. In section 6 we present additional evidence on the evolution of non-price variables such as traded volumes, entry and investment. Section 7 concludes and discusses policy implications following on from the empirical analysis.

2. The EC's merger decision within the context of the ZEE hub

This section presents the main features of the GDF/Suez merger decision within the context of the Belgian wholesale gas market and, in particular, of the ZEE hub (for further institutional details, see European Commission, 2006). We also identify the dates of the key merger events, which are crucial for our empirical approach.

The Belgian gas network is an integrated network used for domestic transmission, as well as for international transit. The ZEE hub was, and remains, crucial for this transit role; the pipelines that go through the ZEE hub are being used almost exclusively for transit (Heather, 2012).[8] The role of the hub became more prominent leading up to 2006 with the gradual liberalization of gas markets. As explained in detail by Miriello and Polo (2015), the ZEE hub, as well as the other European hubs, were initially developed to cope with local network balancing needs. However, they turned out to become a source of gas procurement as the previously monopolized industry structure gradually became more fragmented.

At that time, GDF and Suez groups were the two most important players in the Belgian gas sector. Suez, the largest company in the market, had a majority stake in Distrigas (which was dominant in gas wholesale and supply) and in Fluxys (which controlled gas infrastructure, transit, storage and transport).[9,10] Through these companies, Suez also controlled GIE Finpipe (which owned the rTR/vTn pipelines on which the ZEE hub is physically located), Distrigas & Co (which managed the capacity rights on these rTR/vTn pipelines) and Huberator (which operated the ZEE hub). Thus, while the first two gas directives of the European Parliament (98/30/EC and 2003/55/EC) had led to "legal unbundling", in terms of ownership, Suez still controlled the access to the ZEE hub. GDF, on the other hand, was the second largest competitor in Belgian gas wholesale and supply markets.

In May 2006, the European Commission received prior notification of a concentration between the GDF group and the Suez group via an exchange of shares. The Commission's investigation found that, given the horizontal and vertical overlaps between the two companies' activities, the proposed transaction raised significant competition concerns across all levels of the Belgian gas market. Of particular concern was the "market for trading

[8] See Figure A2 in Appendix C for a map of pipelines in Europe.
[9] While the firm was originally named Distrigaz, it was later renamed Distrigas. We use Distrigas throughout.
[10] Wholesale markets refer to the import of gas from abroad and trading between gas shippers (among others, on the hub). Supply refers to the markets where gas is sold to large customers, power generators or retailers.

at the ZEE hub", which was considered a relevant product market on its own, separated from the various gas-supply markets in Belgium (European Commission, 2006).

The Commission was concerned that the merger might further impede access to the ZEE hub, which was already suffering access and liquidity issues. Actual or potential competitors of Distrigas wishing to access the hub had to obtain capacity rights through an entry/exit agreement with Distrigas & Co, which was the capacity rights seller of the transit network. According to the Commission, these negotiations, which took place on a bilateral non-transparent basis, posed a problem in terms of access. For example, there were issues regarding access to information, because Distrigas could obtain details of the positions of competitors. This may have undermined confidence and discouraged market entry.

In response to the EC's concerns, GDF and Suez offered extensive remedies, including (i) the divestiture of the Suez group's holdings in Distrigas to a third party, and (ii) the reorganisation of the activities of Fluxys, split into two new entities, Fluxys SA and Fluxys International, and relinquishing of all control over these. Fluxys SA was to own the entire Belgian gas transmission and transit system, thus including GIE Finpipe and Distrigas & Co. Fluxys International was to own Huberator as well as other Belgian and international assets.

These remedies were intended to facilitate the entry of new competitors, in particular in the market for trading of the ZEE hub. They were intended to generate increased access to the hub, which in turn should lead to higher liquidity and volumes traded and to lower prices. In **November 2006**, as a result of these remedies, the Commission concluded that the merger would not significantly impede competition and approved the merger.

The merger and associated remedies were implemented around **June 2008**. In May, Suez sold Distrigas to ENI. In June, Distrigas & Co. was transferred to Fluxys SA. In July, the partial sale of Fluxys reduced Suez's shareholding in Fluxys SA to 45% and Fluxys International to 60%. Moreover, it was agreed that decisions in Fluxys SA and Fluxys

International would be taken by the same board. In July, the newly created GDF Suez officially came into existence.

3. The empirical methodology

We aim to quantify the effects of the merger and associated remedies on the prices at the ZEE hub. In order to estimate the effect of the merger on hub prices, we use as main analysis the Difference-in-Difference (DiD) approach – a methodology that is widely used in policy evaluation exercises.[11] The basic idea behind the DiD methodology is to compare what happened to the treated group (here the ZEE hub, which is affected by the merger) before and after the merger with what happened to a control group. The control group represents what would have happened to the treated group in the absence of the treatment. The double differencing removes the common factors that might be otherwise confounded with the effect of the merger, thereby allowing the identification of the effect of the merger on prices.

3.1 The data

The dependent variable for the main econometric analysis is the daily transaction price for day-ahead wholesale natural gas traded during working days, as published by Platts, both for the Belgian hub (ZEE) and for the Dutch hub (TTF). TTF is the counterfactual we use to identify the causal effect of the merger on prices (see the next subsection for a description of our empirical model and, in particular, the selection of the control hub). Our sample period extends from January 2005 until December 2011. For each working day (i.e., Monday to Friday), these data reflect the price range of a standardized quantity of natural gas to be delivered at a constant flow rate throughout the next working day, after assessment (e.g., Friday's assessment reflects Monday's delivery). All prices are denominated in €/MWh.

In order to test the robustness of our findings to alternative control groups, additional price data from Platts for other European hubs is used. In particular, we obtained data for two

[11] Most merger retrospective studies use this methodology. See Weinberg (2008) for a review.

German hubs (Gaspool & Netconnect Germany) and one Italian hub (Punto di Scambio Virtuale). Data for these hubs is only available from June 2007 onwards.

A set of control variables have been collected from various sources and included in the estimation equation. The data sources for the variables used in our analysis are described in Table A.1 in Appendix A.

3.2 The econometric model

In our empirical model, hub prices depend on a set of demand- and supply-side variables, as well as on merger-related dummies. In particular, we estimate the following equation:

$$p_{it} = \alpha + \beta treat_i + \gamma post_j + \delta treat_i \times post_j + \omega_1\, power_{it} + \omega_2\, oil_t + \omega_3\, coal_t + \rho_1\, temp_{it} + \rho_2 temp_t^2 + \sum_{d=1}^{4} \varphi_d D_{d,t} + \sum_{m=1}^{11} \mu_m M_{m,t} + \sum_{y=2005}^{2010} \vartheta_y Y_{y,t} + \epsilon_{it} \quad (1)$$

The dependent variable p_{it} is the daily price in hub *i* at time *t*. The regressors are demand-side variables such as season and business cycles (day *D*, month *M*, and year *Y*), as well as temperature (*temp*).[12] Supply-side controls are indices of power prices as well as oil products, to which gas prices are typically related (*power, oil, coal*).[13]

The identification strategy crucially relies on the comparison between the ZEE hub and the control hub. The dummy *treat* is therefore equal to 1 for the treated observations, i.e. prices at the ZEE hub. The dummy $post_j$ is equal to 1 in the period after each event related to the merger took place (as discussed below, we use different definitions of the *post* variable,

[12] We also account for non-linearities in the effect of temperature by including a quadratic term in the regression. We also show an alternative specification, using heating degree days, a measure of the energy demand needed to heat a building derived from outside temperature, instead of temperature. Our main results are unchanged. See Table B.5 in Appendix.

[13] Our demand-and supply-side variables are similar to those that Böckers and Heimeshoff (2014) use in the estimation of electricity prices.

which explains the *j* subscript). The interaction dummy $treat \times post_j$ is the main variable of interest as its coefficient represents the price difference between the ZEE hub and the counterfactual hub after the merger-related events.

Finally, we allow for autocorrelation and heteroskedasticity of the error terms by estimating Newey-West standard errors. In our main specification we assume the maximum lag order of autocorrelation to be equal to one week (seven days). In Section 5, we present the results of several robustness checks, including different modeling assumptions on the error structure (see Section 5.4 for a discussion).

3.2.1 Identification of the control group

The robustness of the identification strategy depends on the selection of a suitable control group, which represents what would have happened in the absence of the merger and remedies. In principle, the control group should be unaffected by the event and also have similar characteristics, in so far as possible, to the treated group.

In this context, the choice of the control group proves to be a challenging task for several reasons. First, there are no other hubs in Belgium, therefore we are required to use a hub in another country as a counterfactual. Comparing different countries is generally problematic in the context of a DiD approach, as different countries have different institutional features and are, therefore, subject to different shocks. However, within this context, we are comparing hubs which, in spite of being located in different countries, are marketplaces sharing most features. With this in mind, the structure and functioning of the hubs is similar.

Another issue is data availability. There is limited data availability for the European hubs in the mid-2000's. A final issue representing difficulties with comparator selection is the fact that European hubs are, at least to some extent, interconnected. This implies that the possibility that a major event affecting one hub does not affect another hub cannot be ruled out.

Taking into account these limitations, we identified the TTF hub in the Netherlands as the most suitable control hub.[14] There are three reasons for our selection. First, at the time of the merger, the ZEE hub and TTF hub were the two largest hubs in continental Europe in terms of liquidity (CREG, 2006b). Second, data is available for the TTF hub for the pre-merger period. And third, the degree of interconnection between the ZEE hub and the TTF hub was low at the time of the decision. Indeed, during the merger investigation the EC concluded that the ZEE and TTF hubs belonged to a different market (European Commission, 2006).

It must, however, be mentioned that the NBP in the UK and TTF hubs became more connected with the opening of the BBL pipeline that connects the Netherlands and the UK since December 2006. This may have indirectly increased the connection between the TTF and ZEE, as the ZEE hub and the NBP hub were part of the same market. Still, the EC decision took the opening of this link into account when deciding that the TTF and the ZEE belong to a different market (European Commission 2006).[15]

However, in order to test the sensitivity of our results to the choice of the counterfactual, we replicate our main analysis with three alternative hubs as controls: two German hubs (Gaspool and Netconnect Germany) and one Italian hub (Punto di Scambio Virtuale). These hubs, especially the Italian hub, were less connected to the ZEE hub, as demonstrated by the price correlations (Petrovich, 2013). The results of these analyses, which are in line with those with the TTF as control hub, are illustrated in Section 5.1. In the same subsection, we

[14] One difference between the Belgian hub and the Dutch hub is that the ZEE hub is a physical hub (i.e. where the gas physically passes through the hub) while the TTF hub is a virtual hub (i.e. where gas enters only virtually after entering into a national system). However, this aspect does not pose too serious problems in terms of comparing both hubs, as even in virtual hubs the gas physically passes through, albeit at a national level as opposed to a local level.

[15] The EC came to that conclusion based on studies made by the British regulator Ofgem and the Belgian regulator CREG.

also address the issue of interconnection and we provide evidence indicating that there have been no spillover effects of the merger from the ZEE into the TTF hub.

Figure A1 in Appendix C compares prices at the ZEE hub to those in the Netherlands. This comparison is only intended to observe high-level trends at a high level, because it does not control for any of the demand and supply variables that are included in our regression. The two prices follow a similar pattern, consistent with the 'common trend assumption' on which the DiD strategy hinges.[16] This assumption states that the treatment and control group would follow the same trend in the absence of the treatment. Some short-term price spikes at the end of 2005 and beginning of 2006 reflect external events, including a cold snap in the UK and a shortage in the UK's main gas storage facility due to a fire outbreak. This had an immediate impact on the spot prices given the interconnectedness between the Belgian and UK markets.[17]

3.2.2 Identification of the treatment period

To quantify the impact of the two main events related to the merger decision, two different definitions of the 'post' period are used. We can identify two periods as most relevant to assess the overall effect of the merger, namely:

1. The period after the official publication of the Commission's decision (14 November 2006).

2. The period after the merger was effectively finalised and the different structural remedies were implemented. We do not know the exact dates of the June 2008

[16] Note further that in terms of general liquidity, the Dutch TTF and Belgian ZEE hub are also comparable pre-merger. Indeed, the EC Energy Inquiry (2007, p. 41) states that "*On each of the most important Continental hubs, incumbents were significant buyers during the 2003-2004 period (49% by volume of all TTF purchases, 58% in Zeebrugge, […]). Gas producers were the next most important group of traders. New entrant suppliers bought small volumes at Zeebrugge (2% of all purchases), and almost nothing anywhere else on the continent […].*"
[17] BBC News. 2006. 'Gas shortage sends prices soaring': http://news.bbc.co.uk/1/hi/business/4802786.stm.

remedies. Furthermore, in July 2008 the merger was finalized. We choose therefore an intermediate date (30 June) as our relevant treatment date, which aims to capture both the remedies and the finalization of the merger.

Before we explain in detail our identification strategy, it is worth noting that when mergers are subject to authorisation, it is common in the retrospective evaluation literature to consider the date of the decision as the main relevant date to assess the effect of the merger (see for instance Choné and Linnemer, 2012; Björnerstedt and Verboven, 2016; Aguzzoni et al., 2016). In the case under consideration, there was a time lapse of more than one-and-a-half-years between the Commission's decision and the actual consummation of the merger and its remedies. Given this sequence, it could be fairly assumed that the parties started negotiating the terms of the merger and sale of assets at the time of the decision. More generally, it is likely that after the decision, other market operators began to adjust their strategies anticipating the finalisation of the merger and its remedies. Therefore, an assessment of the price impact at the time of the decision itself is important.[18] In Section 5.2 we discuss several robustness checks on the timing of the merger effects.

Our identification strategy aims at quantifying both the individual effect of each of the two events and their overall effect. These effects are summarised in Figure 1 below. We now describe our identification strategy for both the individual events and the overall effect of the merger-related events.

[Insert Figure 1 about here]

Individual Effects of the Merger Events

[18] There are at least three merger retrospective studies finding evidence of price changes before the merger was actually completed (Kim and Singal, 1993; Prager and Hannan, 1998; and Borenstein, 1990).

To disentangle the effect of each event, we run two separate regressions on two different sample periods, each of which represents the relevant before/after period around each event. The individual effects are therefore identified on a reduced sample period. Since the two events are far enough apart in time (more than one and a half year between each other), the number of observations on which we identify each treatment effect is quite large. Therefore, these should not be regarded as merely short-run effects. They represent the individual effect of each merger-related event in the period in which the event is assumed to display its effects.

To measure the effect of each of the two events, we thus consider two different definitions of the $post_j$ dummies, one for each event j, and of the form:

(2)
$$Post_1 = \begin{cases} 1, & for\ t \in (T_1, T_2) \\ 0, & for\ t \in (T_0, T_1) \end{cases}$$

$$Post_2 = \begin{cases} 1, & for\ t \in (T_2, T\ \ ) \\ 0, & for\ t \in (T_1, T_2) \end{cases}$$

where T_1 and T_2 are the dates of event 1 and 2 respectively, and T_0 and T are the initial and last dates in our sample period.

Overall Effect of the Decision

To measure the cumulative effect of the merger and of its associated remedies, we identify the treatment effect from an overall perspective. In particular, we assume that the overall effect of the decision can be observed only from when the merger and associated remedies have been implemented. This is done by comparing what happened after these were implemented and the period prior the Commission's decision. We thus exclude the implementation period (December 2006-June 2008) and consider the following *post* dummy:

(3)
$$Post_{overall} = \begin{cases} 1, & for\ t \in (T_2, T\ \ ) \\ 0, & for\ t \in (T_0, T_1) \end{cases}$$

3.2.3 Hypotheses

The different hypotheses to be tested on prices are that (i) if the merger is anti-competitive and the imposed remedies did not yield their desired effect, then the merger leads to an increase in prices. On the other hand, (ii) if the merger is not anti-competitive (due to initial conditions and/or due to the effectiveness of the imposed remedies), then the merger does not yield to an increase in prices. Finally, (iii) if the merger remedies did more than offset the anti-competitive effects of the merger, then the merger leads to a decrease in prices.

4. Main results

We present here the results of the main set of estimations using the econometric framework described above. We show two specifications for each treatment; one where the effect is measured from the exact day of the event, and one in which we drop a three-month window surrounding the event. This is commonly done in the retrospective merger literature to take into account possible anticipation/delay effects (see section 5.2 below for further discussion on this issue). In both regressions, we estimate Newey-West standard errors while allowing for autocorrelation of up to 7 lags.

4.1 Individual Effects of the Merger Events

In Table 1 we present two main sets of regressions, one for each definition of the 'post' period, corresponding to the two different events related to the merger.

[Insert Table 1 about here]

In the first two columns of Table 1, we present estimation results where the relevant event is the Commission's decision (event 1). As explained above, this regression is run on the period

1 January 2005 (beginning of our sample period) to 30 June 2008 (date of event 2). The positive coefficient for the *treat* dummy indicates that prices at the ZEE hub were on average higher than at the TTF hub over the period under consideration, controlling for other observable variables. However, the interaction coefficient *Treat*post*$_1$, which is negative and significant in both specifications, suggests that there was a price decline at the Belgian hub relative to the control hub after the decision. The decision led to a price decrease of 13% at the ZEE hub with respect to the counterfactual hub.[19] The result is slightly larger if a time window around the decision is excluded (column 2). The coefficients for the other control variables generally have the expected signs, indicating a positive relationship with the prices of other inputs and a negative effect of temperatures.

Columns (3) and (4) in Table 1 show the results of the regressions that consider the effects of the 2008 events, namely the effective divestitures of Distrigas, Distrigas & Co and partial divestitures of Fluxys, and the consummation of the merger (event 2). These regressions are run on the period from 14 November 2006 (date of event 1) and 31 December 2011 (end of the sample period). The coefficients of the treatment effect variable show that the events around June 2008 also had a negative and significant impact on price at the ZEE hub, relative to our control hub. The consummation of the merger led to a price drop of 2% in the Belgian hub with respect to the Dutch hub.[20] This finding holds true (and is even reinforced) if a window of three months around that date is dropped. Note that this is an *additional* price decrease with respect to the first event. This suggests that there was a price decline at the Belgian hub, relative to the control hub, after the effective implementation of the merger and associated remedies, although this additional effect is smaller than the effect of the decision.

[19] The interaction coefficient indicates that the price difference between the two hubs is 2.36 euro lower in the ZEE hub after the decision, and the average price at the Dutch hub is 18.05 euro in that period. In order to compute this price change in percentage terms, we divided the interaction coefficient by the average price at the TTF hub in the post-merger period.

[20] The price difference between the two hubs is -0.38 euro after event 2, and the average price at the Dutch hub is 18,54 euro in that period.

4.2 Overall effect of the merger

We also estimate the total effect of the merger and of its associated remedies, assuming that this effect can only be observed when the merger and associated remedies have been implemented. We thus estimate the effect of the previously defined $post_{overall}$ dummy and exclude the implementation period (15 November 2006 to 30 June 2008) from the analysis. As in the previous sets of estimations, we show in column (1) of Table 2 the baseline specification, and additionally estimate a regression where we drop a three-month window around the merger implementation and associated remedies (column (2)).

[Insert Table 2 about here]

The interaction coefficients are negative and significant, which provides evidence that the overall effect of the merger and of its associated remedies was a decrease in prices at the hub. As expected, the magnitude of the estimated overall impact is similar to the sum of the estimated individual effects of the two main events, suggesting that these were the principal determinants of the merger effect: the overall price drop attributable to the merger is 15%.[21]

5. Limitations and robustness checks

Assessing the effect of mergers through the DiD approach in the context of gas hubs is challenging. Indeed, we have a setting with a single treated unit (the Zee hub) on which the merger is expected to have an effect, and a limited set of potential counterfactual units. Merger retrospective studies normally deal with multiple markets/products that are affected by the merger and that can be compared with (counterfactual) markets/products that are not affected. In that context, it is possible to control for fixed effects at the market or product level. In our context, we can only compare a single hub with a limited number of other hubs and it is therefore more difficult to control for other factors that may have affected prices.

[21] The overall price difference between the two hubs is -2.73 euro, and the average price at the Dutch hub is 18.54 euro after the merger.

Against this backdrop, it is crucial to address the (i) choice of an appropriate counterfactual, (ii) the correct definition of the 'before' and 'after' merger periods and (iii) the absence of confounding effects. The following three subsections discuss each of these assumptions in turn, outline potential issues in our context and provide robustness check analyses. We also discuss issues related to the potential autocorrelation of the error term in section 5.4.

5.1. Choice of counterfactual

In the DiD approach, the control group should represent what would have happened to the treated group in the absence of the treatment. That is why the control group should have similar characteristics to the treated group and be unaffected by the event. As discussed in Section 3.2.1, identifying a suitable counterfactual is a challenging task in our context, given the low number of existing hubs available to choose from. In our main analysis, we have not only one hub that is being treated by the merger but also only one hub that can be used as control hub for both events (the TTF hub, because of data availability). As confounding effects cannot be averaged out through the presence of multiple hubs in treatment and control groups, the similarity assumption is all the more important in our context.

Moreover, to make sure that the control is unaffected, the treatment and control units should ideally belong to separate markets. Unfortunately, all the European hubs are, at least to some extent, interconnected. Although the degree of interconnection between the ZEE and the TTF hubs was low at the time of the decision, the opening of the BBL pipeline connecting the TTF and the UK's NBP might have indirectly increased the ZEE-TTF interconnection, as the ZEE hub and the NBP hub were part of the same market.

The above-mentioned features of the market under consideration make the identification strategy particularly challenging in our context. Therefore, we performed several exercises to corroborate the evidence of our baseline DiD. The analyses presented in this and in the following subsections bring several pieces of evidence that all point in the direction of showing a negative price effect of the GDF/Suez merger.

To check the validity of our choice of counterfactual, the rest of this subsection provides two types of robustness checks:

- We show that the event 2 results are similar if, instead of TTF, we use, as counterfactuals, other (less-connected) hubs such as Germany's Gaspool and Netconnect as well as Italy's PSV.
- We disentangle the effects in the two hubs and show that (real) prices declined post-merger in the ZEE hub, whereas they stayed the same in the TTF hub. This finding provides evidence on the degree of interconnection between the two hubs and suggests that it is unlikely that there have been spillover effects from the ZEE hub into the TTF hub.

Alternative counterfactuals

In order to test the sensitivity of our results to the choice of the counterfactual, we gathered additional price data for other European hubs. In particular, we obtained data for two German hubs (Gaspool and Netconnect Germany) and one Italian hub (Punto di Scambio Virtuale). These hubs, especially the Italian one, were less connected to the ZEE hub, as shown by the price correlations (Petrovich, 2013).[22] One of the reasons why we did not use these hubs as main control group in the first place is that data for these hubs is only available since June 2007, and therefore only allow us to evaluate the effect of the merger implementation and associated remedies (event 2), but not of the merger decision (event 1). Using these hubs as alternative controls is our first robustness check.

[22] As shown in figure 8 in Petrovich (2013), in terms of initial price convergence, the points of departure for these hubs are also different: the German hubs had higher absolute prices than the Belgian ZEE hub in 2007/2008, whereas the Dutch TTF hub had prices that were lower than the Belgian hub (there is no data for the Italian hub in this figure). This fact makes the German hubs additionally interesting as alternative counterfactuals. We thank an anonymous referee for leading us to this figure.

As we show in Table 3, results are quite consistent: the merger implementation and associated remedies led to lower prices in the ZEE hub relative to all these other control hubs. This suggests that the finding of a negative price effect of the merger does not depend on the choice of the Dutch TTF as a counterfactual hub in the DiD analysis.

[Insert Table 3 about here]

Disentangling the price effects in the ZEE and TTF hub

We now provide evidence showing that there have been no spillover effects of the merger from the ZEE into the TTF hub. We run a slightly different specification of our main DiD estimations, which allow us to more cleanly interpret the effects on the two hubs separately. The DiD results can also be used to identify a simple "before/after" merger effect, i.e. a comparison of the prices before and after the merger in the treated and in the control group. Indeed, the coefficient of the *post* dummy variable is the effect on the control group (the TTF hub in our case), whereas the sum of the coefficients of the *post* dummy and of the interaction dummy (*treat*×*post*) can be interpreted as the effect on the treated group (the ZEE hub) when leaving out the comparison with the control hub.

The clean interpretation of these variables in our main regressions (Table 1) is not possible because they include year dummies in order to control for time effects in prices, due to, for instance, general inflation. Thus, in order to correctly calculate the effect post-merger in the control group one would have to leave out the year dummies in the regressions. That is the approach we follow in the regressions of Table 4 below. Note further that in this specification, given the absence of year dummies, we use real instead of nominal prices in order to account for inflation.

[Insert Table 4 about here]

As can be seen from Table 4, first of all, the coefficients on the interaction variable are again negative and significant in the three columns: thus, real prices have gone down in the ZEE hub after the merger when compared with the TTF hub. Second, the coefficients of the *post* dummy variable are not significant for any of the columns. Thus, real prices in the Dutch TTF have not changed significantly post-merger. Furthermore, the price change at the ZEE hub only, i.e. not controlling for general tendencies in gas markets by comparing ZEE prices with TTF prices, is negative and significant (this is represented by the sum of coefficients *post* and *treat*×*post*). For event 1, this effect is 0.1922 - 2.5176= -2.3254 and we tested that it is significantly different from zero. For event 2, this effect is -0.0449 - 0.6330 = -0.6779 (and significantly different from zero). Finally, for the total effect, the sum is 0.0226 - 3.1371 = -3.1145 (and significantly different from zero). Thus, in a simple before/after comparison of the effect of the merger on the ZEE hub, we find that real prices in the Belgian ZEE hub have gone down. They instead remained unchanged at the TTF hub, thereby suggesting that the effect of the merger is not driven by the interconnection between the two hubs. This analysis, therefore, suggests that the ZEE and the TTF do not belong to the same relevant market.

5.2. Timing of the effects

Retrospective merger studies are faced with the task of correctly defining 'before' and 'after' periods. This task is often challenging because there might be both anticipation effects (i.e. strategic behaviour that may take place before the merger is approved) and delayed effects (i.e. it might take time before the merger is finalised, particularly if remedies have to be implemented). Indeed, evidence from both the banking (Prager and Hannan, 1998) and airline sectors (Kim and Singal, 1993) suggests that the merging firms may change their prices before the merger is consummated. On the other hand, it is good practice to exclude a period following the merger to account for the possibility that price changes take some time to materialize. Therefore, it is common in the literature to exclude from the analysis a three-month or six-month time window surrounding the merger (see for instance Ashenfelter et al., 2014). In order to address this issue, in our main analysis we dropped a three-month window around each of the two events. In Table 5 we further confirm that indeed our results are robust across different definitions of dropped time windows (one and six months).

A related issue has to do with the fact that the GDF/Suez merger was announced in February 2006 and the Commission decision is dated November 2006. One could therefore argue that even dropping a six-month window around the decision date is not enough, as the effects might have started to materialize earlier. In order to address this concern, we extended the time window for event 1 so that we compare the period before February 2006 with the period after November 2006. These results, reported in column 3 of Table 5, show that the joint effect of the merger announcement and the Commission's decision is significantly negative: the price dropped by an even larger amount if we exclude this interim period. This result indicates that the price decrease has started to materialize already in this period.

[Insert Table 5 about here]

Furthermore, another issue related to the timing is the sample period in which we measure the effects of the merger. In our main analysis, we are able to quantify a rather long-run price effect of the merger, given that the merger and associated remedies were finalized in mid-2008 and we have observations until 2011.[23] It may also be interesting to understand whether effects in a shorter time window differs from our main analysis. For this purpose, we limit the data to a period of 12 months before and 12 months after the merger for events 1 and 2. The results of this analysis, presented in Table 6, show that there is again a negative impact of both event 1 and event 2 on prices in the ZEE hub when compared to the Dutch TTF. We actually observe that the effect is even bigger in magnitude than in our baseline analysis, which might be due to the fact that most of the impact materializes within a one-year period.

[23] The possibility to study long-run price effects of a merger is often limited by data availability; see Weinberg (2008) for a discussion.

The longer the time period considered, the more the effect gets diluted, as it tends to diminish over time.[24]

[Insert Table 6 about here]

Finally, when adding an additional year (2012) to our analysis, results available upon request show that price effects at the ZEE hub compared to the TTF hub are still negative around event 2 (the merger implementation), but they are not significant anymore. On the other hand, the overall price effects, i.e. the total effect of the merger by event 1 and event 2 taken together, are negative and significant at the 1% level. This may be explained by the fact that prices at the ZEE hub went up again in 2012, relative to the TTF hub. This has as a consequence that, when taking this longer period into account, relative price changes are not so big anymore around event 2, but are still large and significant when taking all merger events together.

5.3. Confounding effects

There are potentially other factors that may interfere with the effect of the merger. The diff-in-diff framework should alleviate this concern, because comparing the evolution of prices for the treatment and for the control groups allows to isolate the effects that can be attributed only to the merger decision from the effects induced by changes in other variables that may have simultaneously affected both the treatment and the control group. Furthermore, in order to reduce the possible bias due to other non-common confounding factors, we also control

[24] Our DiD results are confirmed when using a very different empirical approach: a regression discontinuity (RD) design on the ZEE hub around the merger dates. This approach has the advantage of being very simple and parsimonious in terms of assumptions required, because it does not require to identify a control group. Moreover, as the RD focuses on a very short-run effect, it allows to alleviate the concern about potential confounding factors when studying long-term effects. In particular, by zooming in on short-run effects, we should be able to alleviate to some extent the role played by the introduction of the BBL, which took place approximately one month after the merger decision (event 1). Unreported results confirm our main findings: prices declined in the ZEE hub, both after event 1 and event 2. Results are available on request.

for demand- and supply-related variables that may differently affect the two hubs. It is, however, virtually impossible to include all possible factors that might have played a role. We therefore perform a placebo analysis where we choose three random events: one before, one in between, and one after our events 1 and 2. Each placebo event was chosen the 1st of December in order to have large enough time windows and non-overlapping periods between the different events.[25]

The result reported in Table 7 indicate that our three placebo events show no effect on prices. This suggests that is unlikely that our findings are driven by other non-common confounding factors.

[Insert Table 7 about here]

Finally, the results for the first event and for the overall effect might partly be driven by the large price movements that took place in the period prior to the merger decision.[26] In order to test the sensitivity of our results to these price shocks, we run our regressions on a reduced sample where we cut out the periods with the extreme spikes (November 2005 and March 2006). The magnitude of the interaction coefficient, shown in Table B.1 in Appendix B, becomes smaller but the sign is unchanged and still significant.

5.4. Error term

As is usual with high-frequency data, the estimation strategy has to deal with the issue of autocorrelation in the error term. We address this in three ways. First, we estimate our regressions with Newey-West standard errors, assuming heteroskedasticity and allowing for autocorrelation of the error term up to some lag. In our main specification, the specified

[25] We have chosen a window of 12 months before and 12 months after each random event. For the middle event we had to choose a shorter window to not overlap with event 2. We chose therefore a window of 6 months before and 6 after the second random event.

[26] Note that our estimation results for event 2 are unaffected by the price spikes, since the sample period for the corresponding regressions does not include the period before 2007.

autocorrelation lag is 7, but as a robustness check we tried other lag orders.[27] Another common way to deal with this issue is to estimate bootstrapped standard errors, something we also undertake as a robustness check. Finally, we reduce the frequency of data from daily to weekly.

These robustness checks, shown in Tables B.2, B.3 and B.4 in Appendix B, yield very similar results to those presented in Tables 1 and 2. In particular, in all regressions the coefficient of the *Treat*Post* variable is negative and significant for the first two events and the overall effect.

6. Effect of the merger on traded volumes, entry and investment

The merger might have affected the development of the ZEE hub along other (non-price) dimensions. Specifically, it might have had an impact on hub liquidity —i.e. traded volumes and number of active participants — and investment decisions. Unfortunately, the data available to us for these variables is more limited, both in terms of frequency and length of period. Moreover, we only obtained data from the Belgian hub.[28] This implies that we cannot perform a DiD analysis with these outcome variables around the two merger events as in the previous section. We believe, however, that the results of these analyses are useful to give a broader picture of the developments that took place at the ZEE hub after the merger.

In particular, we gathered information on: i) traded volumes (monthly from January 2005 until December 2011 and daily from January 2007 until December 2011); ii) number of active participants, defined as those traders that sell/buy a positive amount of gas (monthly from January 2005 until December 2011); iii) market shares of entrants, defined as those that were not buying or selling at the beginning of the sample period (monthly from January 2005

[27] We only show the results with lag order 1 but results for lag orders up to 7 are available upon request.
[28] We are indebted to Fluxys for sharing these data with us.

until December 2011); iv) investment on the Belgian gas infrastructure (yearly from 2005 until 2011).

With regards to trading volumes at the ZEE hub, we compare volumes before the merger with volumes after the merger, controlling for the relevant supply- and demand-related variables. Given the low frequency of the data before 2007, we can only perform an econometric analysis from 2007 onwards. As an illustration, however, it is useful to look at the plot of monthly volumes for the whole period 2005-2011.

[Insert Figure 2 about here]

Figure 2 shows that trading volumes at the ZEE hub are substantially higher post-merger versus pre-merger: the period before event 1 had an average monthly traded volume of 1316 GWh, whereas the period after event 2 exhibited an average volume of 1950 GWh; especially after the merger implementation (event 2), one can observe a discrete jump.

The higher traded volumes after the merger consummation is also confirmed by the econometric analysis on the daily data for the period 2007-2011. This analysis can only consider event 2, since event 1 is outside the sample period. We estimate the following equation for trading volumes at the ZEE hub:

(4) $$Vol_t = \alpha + \beta post_2 + \omega_1 \, power_t + \omega_2 \, oil_t + \omega_3 \, coal_t + \rho_1 \, temp_t + \rho_2 temp_t^2 + \sum_{d=1}^{4} \varphi_d D_{d,t} + \sum_{m=1}^{11} \mu_m M_{m,t} + \epsilon_t$$

The dependent variable Vol_t is the daily volume at day *t*. The key variable in this regression is the $post_2$ dummy, which captures the difference between volumes in the post-merger implementation period and volumes in the pre-merger implementation period. We include as controls the same demand-side and supply-side variables that we have in the price regressions.[29]

The results shown in Table 8 show that trading volumes at the ZEE hub significantly increased after the implementation of the merger and its associated remedies (event 2). The increase in volumes is about 27% with respect to the period before event 2. Although one should be cautious in attaching a causal interpretation to these results because of the absence of a counterfactual, they are consistent with improved post-merger outcomes in terms of liquidity as shown by higher trading volumes at the hub.

[Insert Table 8 about here]

As further measures of hub liquidity, we obtained monthly data on the number of active participants and market shares of entrants from January 2005 until December 2011 (see Heather and Petrovich, 2017, for a discussion on liquidity metrics). When we first look at Figure 3, we see a clear upward trend in number of active participants. Indeed, whereas at the beginning of 2005 it starts with 35 active traders, at the end of 2011 there were 55 traders actively buying or selling gas on the ZEE hub.

[Insert Figure 3 about here]

Furthermore, when looking at the market shares of entrants (Figure 4 below), one can see that particularly after event 2 (June 2008), these market shares have jumped and nearly quadrupled from about 2.5% to 10%.

[Insert Figure 4 about here]

[29] The only difference with those regressions is that we did not include year dummies because otherwise we could not interpret the ‘post’ coefficient.

As we can see in Figure 5, the investment in the Belgian gas infrastructure also shows two clear upward jumps (one post-2006 and the second post-2009).

[Insert Figure 5 about here]

In sum, while this part of the analysis can only serve as illustration, the results are in line with improved post-merger outcomes: (i) traded volumes on the ZEE hub have increased, and (ii) the number of active participants and market shares of entrants have gone up as well. These figures are consistent with a more liquid hub post-merger. Furthermore, the investment trend is upwards in Belgium. As such, this additional evidence does not contradict the statement that the merger and the associated remedies have had a pro-competitive impact on the Belgian ZEE hub and induced a higher investment in gas infrastructure in Belgium.

7. Conclusions

This paper provides a quantitative ex-post analysis of the GDF's acquisition of Suez in 2006. The results of the empirical analysis suggest that the EC's decision and the implementation of the merger and its associated remedies had an impact on wholesale gas prices in Belgium. Prices at the ZEE hub fell relative to the TTF hub following the Commission's merger decision (November 2006) and the implementation of associated remedies (June 2008).

The ex-post evaluation of such a large European energy merger is important and warrants analysis. The setting is admittedly imperfect, especially because we have a single treated unit (the ZEE hub) on which the merger is expected to have an effect. In this context we can only compare a single hub with a limited number of other hubs and it is therefore difficult to control for other factors that may have affected prices. We do, however, extensively discuss potential limitations of our setting, and perform a set of additional analyses and robustness checks, not necessarily aimed at solving all the potential issues, but at adding further and different pieces of evidence that —although each by itself might be imperfect— point towards the same results and thus, we believe, strengthen our conclusions.

As a whole, the evidence suggests that the remedies were effective in limiting the potential anti-competitive effects of the merger, as the net effect of merger and remedies shows a price decline and increased liquidity at the hub. It is not possible to disentangle the anti-competitive effects of the merger and the pro-competitive effects of the remedies, as they occurred around the same time. However, the net effect is informative of which effect dominated. The estimated decline in prices also supports the view that ownership unbundling has generated better access to the hub. In this respect, the remedies seem to have done more than simply mitigate the potential anti-competitive effects of the merger. It must be mentioned that one cannot totally exclude that price declines are a consequence of some efficiency gains generated by the merger. However, this is unlikely, given that no potential efficiency gains at the hub were indicated by the merging parties.

Our results are thus in line with the view that for merger transactions that fall within its jurisdiction, the EC can bypass Member States in applying EU merger policy in order to reduce the power of national champions. Indeed, through the application of merger remedies, the EC can ensure third-party access and unbundle vertically integrated companies. These remedies may at first sight seem like an overreach of limiting potential anti-competitive effects. However, the European Court of Justice pointed out in its judgment of ENI/EDP/GDP merger that there exists no legal problem to the EC pursuing liberalization of energy markets through its merger policy actions, as it is aimed at increasing competition.[30]

[30] Case T-87/05 EDP-Energias de Portugal SA v Commission [2005] ECR II-3745.

More detailed retrospective research in this area is needed to shed further light on these issues. In particular, while outside the scope of this study, it would be interesting to investigate the exact channels through which merger policy impacts prices and investment in gas markets. Moreover, the precise relationship between merger policy and energy regulation warrants a deeper analysis, given the importance of both policy tools in energy sectors (see also Duso et. al, 2017, for a broader analysis along these lines).

In any case, while much work remains to be done, we believe that our study is a start and can be useful to researchers, practitioners and policy makers (in government, competition authorities or regulators) who study and follow EU energy markets and who are interested in how changes in market structure and EC's decisions shape market outcomes.

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Figures and Tables

Figure 1 Individual and overall effects of the events related to the merger

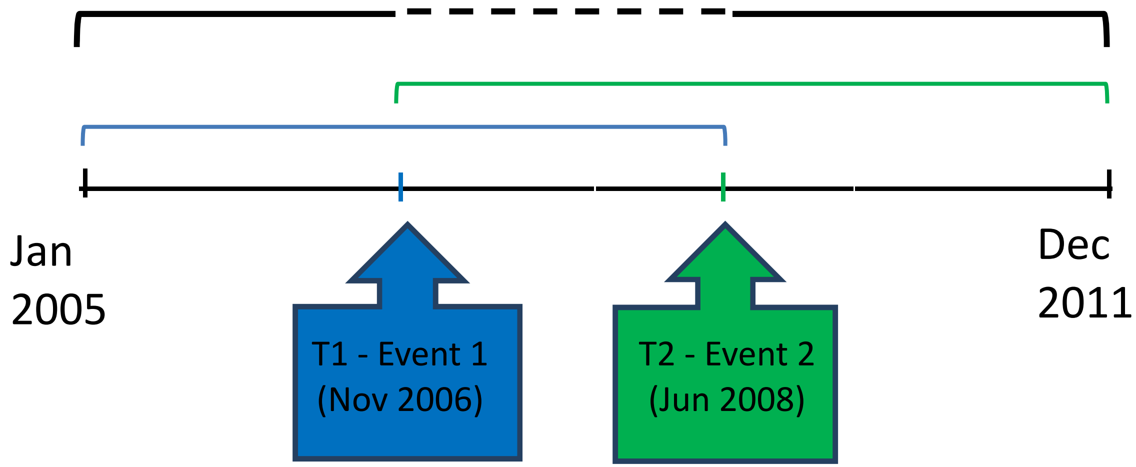

Figure 2: Monthly volumes at the ZEE hub

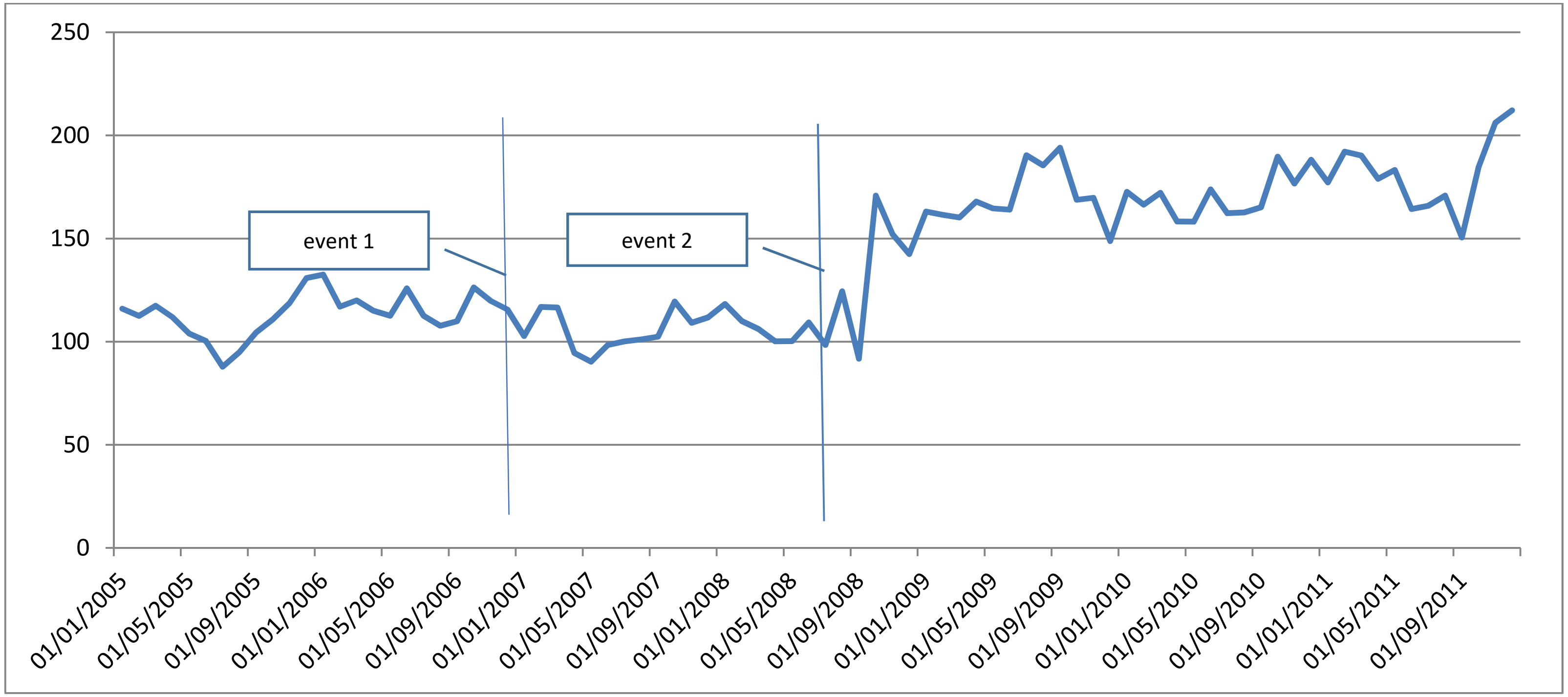

Figure 3: Active Participants at the ZEE hub, by month

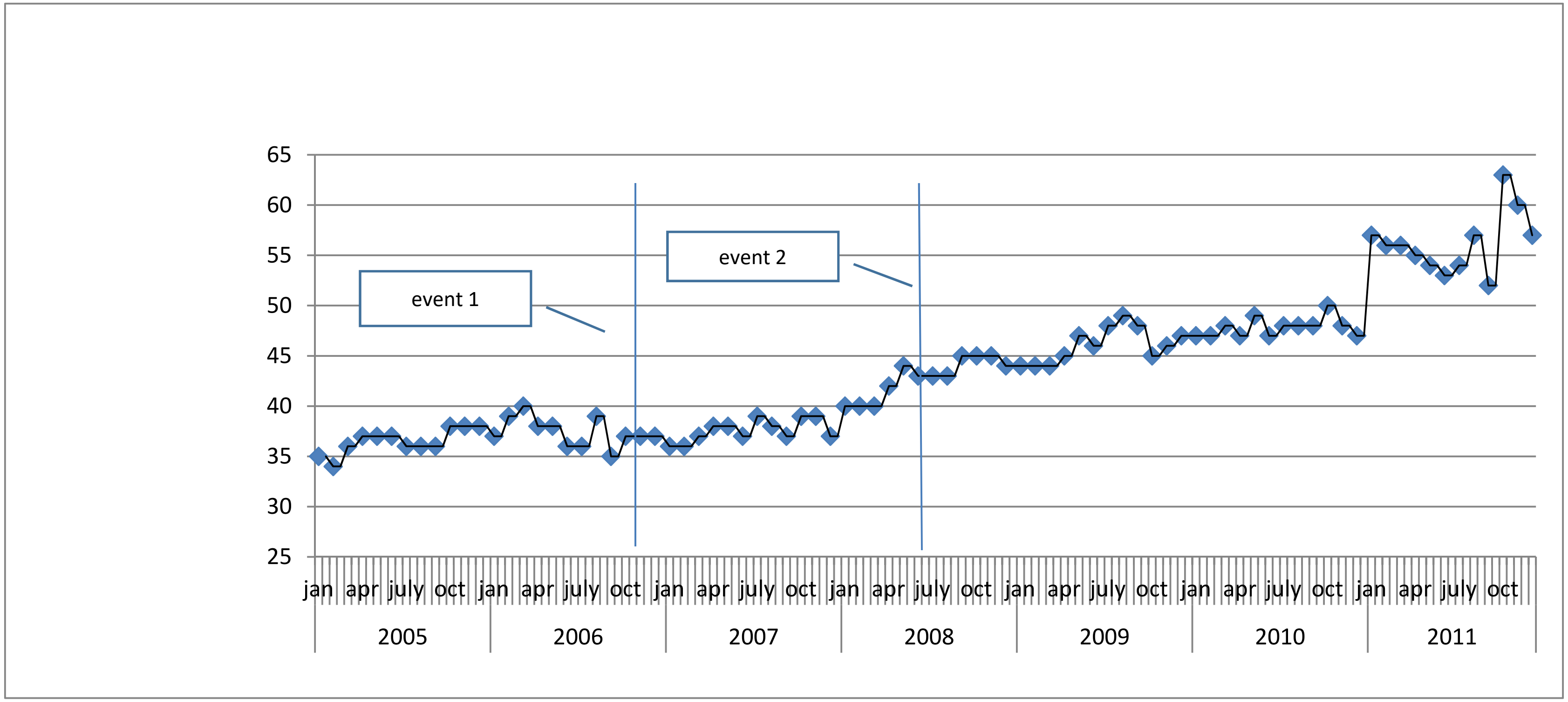

Figure 4: Monthly market shares of entrants at the ZEE hub

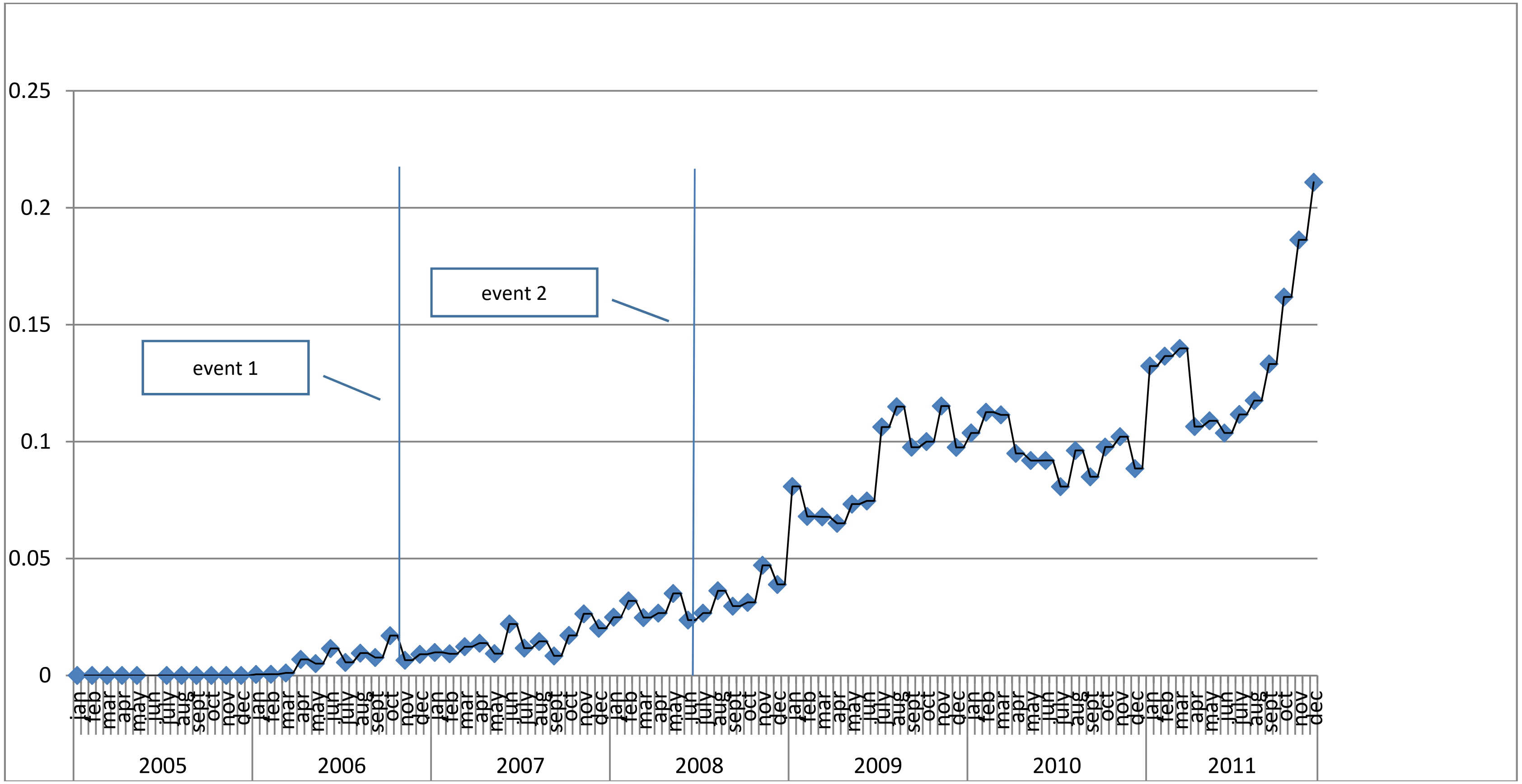

Figure 5: Yearly investment in gas infrastructure Belgium (capex in million Euro)

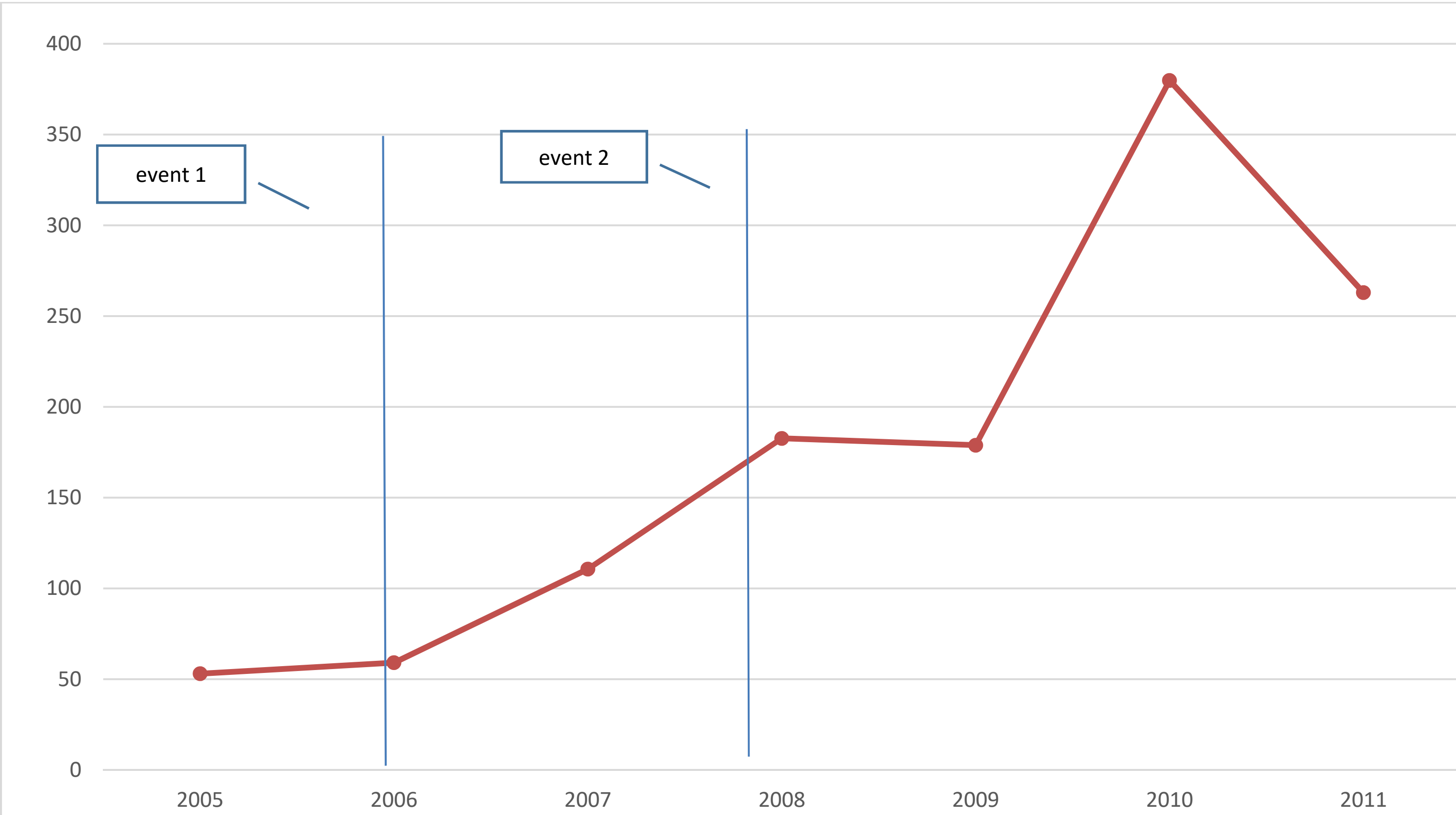

Table 1 – Price effects of individual events

	(1) Baseline	(2) 3-months window dropped	(3) Baseline	(4) 3-months window dropped
Treat	2.593***	2.940***	0.235***	0.224***
	(0.576)	(0.601)	(0.0610)	(0.0655)
$Post_1$	1.339	-0.597		
	(1.358)	(2.646)		
$Treat*Post_1$	-2.364***	-2.604***		
	(0.586)	(0.612)		
$Post_2$			0.351***	0.456***
			(0.101)	(0.109)
$Treat*Post_2$			-0.384***	-0.410***
			(0.0721)	(0.0755)
Power	0.003	0.0023	0.001	0.0009
	(0.0053)	(0.005)	(0.0007)	(0.0007)
Oil	0.670***	0.661***	0.970***	0.972***
	(0.0925)	(0.0978)	(0.0062)	(0.0063)
Coal	0.166*	0.161*	-0.0219	-0.0224*
	(0.0971)	(0.0959)	(0.0134)	(0.0131)
Temperature	-0.226**	-0.242**	-0.0048	-0.0047
	(0.104)	(0.110)	(0.0103)	(0.0103)
Temperature squared	0.0055	0.0059	5.43e-05	0.0001
	(0.0035)	(0.0037)	(0.0004)	(0.0004)
Constant	-6.713	-6.365	1.177***	1.166***
	(5.689)	(5.756)	(0.301)	(0.301)
Observations	1,759	1,636	2,586	2,460
F-test	597.95	571.31	7,652.96	6,986.05

Note: The dependent variable is the daily gas price at the hub. In all specifications, we also include day, month, and year dummies. Newey-West standard errors are reported in parentheses. The symbols ***, **, * represent significance at the 1%, 5%, 10% levels respectively.

Table 2 - Overall price effect of merger

	(1) Baseline	(2) 3-months window dropped
Treat	2.595***	2.940***
	(0.576)	(0.602)
$Post_{overall}$	3.964	4.231
	(2.900)	(2.892)
$Treat*Post_{overall}$	-2.734***	-3.116***
	(0.584)	(0.610)
Power	0.0013	0.0007
	(0.0085)	(0.0087)
Oil	0.717***	0.717***
	(0.0760)	(0.0771)
Coal	0.002	0.0036
	(0.0505)	(0.0509)
Temperature	-0.143**	-0.147**
	(0.0644)	(0.0648)
Temperature squared	0.0033	0.0038
	(0.0024)	(0.0024)
Constant	3.288	3.058
	(3.134)	(3.219)
Observations	2,715	2,588
F-test	633.93	581.74

Note: The dependent variable is the daily gas price at the hub. In both specifications, we also include day, month, and year dummies. Newey-West standard errors are reported in parentheses. The symbols ***, **, * represent significance at the 1%, 5%, 10% levels respectively.

Table 3: Alternative counterfactual hubs

	(1) Gaspool	(2) Netconnect Germany	(3) Punto di Scambio Virtuale
Treat	0.693***	0.258***	-2.515***
	(0.109)	(0.0588)	(0.203)
$Post_2$	0.957***	0.988***	3.124***
	(0.162)	(0.147)	(0.434)
$Treat*Post_2$	-1.085***	-0.806***	-3.820***
	(0.119)	(0.0774)	(0.297)
Power	-0.001	7.79e-05	0.0023
	(0.0014)	(0.0012)	(0.0018)
Oil	0.972***	0.974***	0.817***
	(0.0076)	(0.00950)	(0.0373)
Coal	-0.0453***	-0.0487**	-0.362***
	(0.0137)	(0.0192)	(0.0506)
Temperature	-0.0198**	-0.0206*	0.110***
	(0.009)	(0.0105)	(0.0421)
Temperature squared	0.0005	0.0004	-0.0063***
	(0.0004)	(0.0004)	(0.0015)
Constant	-0.349*	0.317	5.235***
	(0.185)	(0.195)	(0.750)
Observations	2,292	2,292	2,289

Note: The dependent variable is the daily gas price at the hub. In all specifications, we also include day, month, and year dummies. Newey-West standard errors are reported in parentheses. The symbols ***, **, * represent significance at the 1%, 5%, 10% levels respectively.

Table 4: Real prices without year dummies

VARIABLES	(1) Event 1	(2) Event 2	(3) Overall
treat	2.1749***	-0.3389***	2.1745***
	(0.5614)	(0.0757)	(0.5684)
$post_1$	0.1922		
	(0.5389)		
$interact_1$	-2.5176***		
	(0.5945)		
$post_2$		-0.0449	
		(0.1213)	
$interact_2$		-0.6330***	
		(0.0892)	
$Post_{overall}$			0.0226
			(0.4550)
$Interact_{overall}$			-3.1371***
			(0.5752)
tempsq	0.0024	0.0000	0.0010
	(0.0033)	(0.0005)	(0.0022)
temp	-0.1358	-0.0103	-0.0739
	(0.0927)	(0.0125)	(0.0568)
brent_crude_spot	0.6928***	0.8585***	0.7283***
	(0.0737)	(0.0043)	(0.0573)
coal	-0.0214**	-0.0202***	-0.0283**
	(0.0105)	(0.0064)	(0.0117)
power_prices	0.0073	0.0051***	0.0058
	(0.0066)	(0.0014)	(0.0084)
Constant	5.2567***	1.7133***	4.4337***
	(1.7327)	(0.1481)	(1.3247)
Observations	1,759	2,586	2,715

Note: The dependent variable is the daily gas price at the hub in real terms. We also include day and month dummies. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Dropped time windows

	(1) Event 1 1 month dropped	(2) Event 1 6 months dropped	(3) Event 1 Announcement/ decision dropped	(1) Event 2 1 month dropped	(2) Event 2 6 months dropped
Treat	2.659***	3.320***	3.7330***	0.225***	0.187***
	(0.271)	(0.299)	(0.7844)	(0.0329)	(0.0358)
$Post_1$	1.210	1.076***	3.2397		
	(0.993)	(0.365)	(2.7344)		
$Treat*Post_1$	-2.399***	-2.991***	-3.5064***		
	(0.276)	(0.321)	(0.7891)		
$Post_2$				0.365***	0.487***
				(0.0628)	(0.0705)
$Treat*Post_2$				-0.386***	-0.402***
				(0.0401)	(0.0422)
Power	0.00337	0.0112**	0.0050	0.0010**	0.0009*
	(0.0042)	(0.0053)	(0.0052)	(0.0005)	(0.0005)
Oil	0.671***	0.723***	0.6761***	0.971***	0.979***
	(0.0525)	(0.0471)	(0.1254)	(0.0038)	(0.0037)
Coal	0.168***	-0.0253***	0.2540***	-0.0220**	-0.0236***
	(0.0461)	(0.0069)	(0.0854)	(0.0086)	(0.0085)
Temperature	-0.226***	-0.132*	-0.2582**	-0.0037	-0.0037
	(0.0649)	(0.0671)	(0.1122)	(0.0076)	(0.0075)
Temperature squared	0.0055**	0.0018	0.0071*	-1.38e-05	0.0002
	(0.0022)	(0.0024)	(0.0039)	(0.0003)	(0.0003)
Constant	-6.951**	4.454***	-13.2437***	1.166***	1.078***
	(2.790)	(1.267)	(4.4092)	(0.170)	(0.167)
Observations	1,719	1,512	1,380	2,546	2,336
F-test	195.95	555.51	691.9	19,886.89	19,708.92

Note: The dependent variable is the daily gas price at the hub. In all specifications, we also include day, month, and year dummies. Newey-West standard errors are reported in parentheses. The symbols ***, **, * represent significance at the 1%, 5%, 10% levels respectively.

Table 6: Short-run effects on events 1 and 2 (12 months before and after)

	(1) Event 1	(2) Event 2
Treat	4.1973***	0.4726***
	(1.0032)	(0.0737)
$Post_1$	-0.8607	
	(1.3444)	
Treat*$Post_1$	-4.1532***	
	(1.0353)	
$Post_2$		0.5396***
		(0.1238)
Treat*$Post_2$		-0.5152***
		(0.1099)
Power	0.0068	0.0008
	(0.0070)	(0.0012)
Oil	0.7052***	0.9636***
	(0.1076)	(0.0082)
Coal	-0.0911	-0.0193**
	(0.0806)	(0.0083)
Temperature	-0.0008	0.0001
	(0.0050)	(0.0010)
Temperature squared	-0.0366	-0.0162
	(0.1502)	(0.0246)
Constant	5.3954*	0.4162*
	(2.8347)	(0.2318)
Observations	1,009	1,008
F-test	52.74	7071

Note: The dependent variable is the daily gas price at the hub. In all specifications, we also include weekday dummies. Newey-West standard errors are reported in parentheses. The symbols ***, **, * represent significance at the 1%, 5%, 10% levels respectively.

Table 7: Placebo analysis

	(1) Event P1 (12 months B-A)	(2) Event P2 (6 months B-A)	(3) Event P3 (12 months B-A)
Treat	2.1430***	0.3972***	-0.3343***
	(0.7495)	(0.0908)	(0.0674)
Post	-0.1241	0.1613	-0.0096
	(3.8851)	(0.1637)	(0.1015)
Treat*Post	0.6118	0.1193	0.0524
	(1.0719)	(0.1304)	(0.1032)
Power	-0.0037	0.0017*	0.0069***
	(0.0117)	(0.0009)	(0.0025)
Oil	0.6505***	0.9505***	0.9926***
	(0.1120)	(0.0180)	(0.0068)
Coal	-0.0320	-2.0482	-0.0089
	(0.0751)	(1.2714)	(0.0109)
Temperature	-0.2790*	0.0374	0.0087
	(0.1663)	(0.0347)	(0.0126)
Temperature squared	0.0091	-0.0013	0.0003
	(0.0058)	(0.0014)	(0.0005)
Constant	8.0606*	0.2781	0.0424
	(4.8723)	(0.4010)	(0.2159)
Observations	969	500	1,010
F-test	38.34	6900	6101

Note: The dependent variable is the daily gas price at the hub. In all specifications, we also include weekday dummies. The 'post' variable is a dummy that takes value of 1 in the 12 months after each event (6 months for event P2) and 0 in the 12 months before (6 for event P2). Newey-West standard errors are reported in parentheses. The symbols ***, **, * represent significance at the 1%, 5%, 10% levels respectively.

Table 8: Before-and-after regression on trading volumes at the ZEE hub

	(1) Baseline	(2) 3 months dropped
$Post_2$	339.3311***	423.0726***
	(50.7454)	(55.7431)
Power	-0.7115*	-0.7117*
	(0.4204)	(0.4127)
Oil	2.6139	2.6035
	(2.4627)	(2.5124)
Coal	33.9345***	28.3757***
	(3.6785)	(4.0779)
Temperature	-5.1699	-4.1331
	(6.0629)	(5.6221)
Temperature squared	0.3180	0.2862
	(0.2413)	(0.2302)
Constant	1,251.9802***	1,241.6026***
	(61.8204)	(59.7883)
Observations	1,262	1,199
F-test	147.8	148.1

Note: The dependent variable is the daily trading volumes at the ZEE hub in GWH. We also include day and month dummies. Newey-West standard errors are reported in parentheses. The symbols ***, **, * represent significance at the 1%, 5%, 10% levels respectively.

Appendix

A. Data

The variables used in the estimation, together with their corresponding data sources, are reported in Table A.1 below, with the descriptive statistics reported in Table A.2.

Table A.1 Description of the variables

Variable	Description and source
Price	Daily hub prices at time t for hub i. Measured in €/MWh (Source: Platts data base)
Temperature	Daily temperature data for Belgium and the Netherlands in degrees Celsius (Sourced: national administrative bodies).
Oil	Daily spot price for Brent crude oil. Measured in $/bbl (Source: Platts data base)
Coal	Average daily price of coal. This is a combined price series of two sources which measures the daily European reference price for coal imports into North-Western Europe.
Power	Daily price at the power exchange. Since the Belgian Power Exchange (Belpex) started operating on 21 November 2006, there is no data for Belgium before 2007. Dutch power prices for 2005 and 2006 are used as a proxy for this time period[31] (Source: Platts Database).
Volumes	Net traded monthly volumes at the ZEE hub in Gigawatt/Hour (GWh) (Source: Huberator)
Active participants	Monthly number of active participants, where 'active' is defined as those traders that sell/buy a positive amount of gas in that particular month (source: Fluxys).
Market share of entrants	Monthly market shares of entrants, where 'entrants' are defined as traders that are not buying or selling at the beginning of the sample period
Investment	Investment: yearly data on investment on the Belgian gas infrastructure (Source: Fluxys)

[31] We tested whether Dutch power prices were similar enough to the Belgian ones: the correlation between the two price series at later dates is high (.93) and therefore deemed to be high enough to use as a proxy for this gap in the data series.

Table A.2 Descriptive statistics of the variables used in the main estimations

Variable	Obs	Mean	Std. Dev.	Min	Max
Price	3,532	18.77	6.96	3.89	98.44
Temp	3,532	10.77	6.47	-10.07	28.4
Oil	3,532	19.10	7.95	3.88	98.43
Coal	3,532	16.97	19.42	0.01	75.1
Power	3,532	61.82	26.91	16.32	432.83

B. Additional tables and robustness checks

Table B.1: Robustness checks dropping periods of price spikes

VARIABLES	(1) Event 1	(2) Overall effect
Treat	1.341***	1.342***
	(0.418)	(0.421)
$Post_1$	0.783	
	(0.982)	
Treat*$Post_1$	-1.114***	
	(0.430)	
$Post_{overall}$		1.414
		(2.412)
Treat*$Post_{overall}$		-1.483***
		(0.427)
Power	0.0006	-0.0020
	(0.0020)	(0.0027)
Oil	0.731***	0.789***
	(0.0712)	(0.0570)
Coal	0.103	-0.0261
	(0.0757)	(0.0411)
Temperature	-0.195*	-0.140**
	(0.106)	(0.0637)
Temperature squared	0.0052	0.0038*
	(0.0034)	(0.0020)
Constant	-2.946	4.528*
	(4.225)	(2.411)
Observations	1,669	2,625
F test	880.77	1,031.08

Note: The dependent variable is the daily gas price at the hub. The sample size cut out November 2005 and March 2006 (the periods with the most extreme spikes). In all specifications we also include day, month, and year dummies. Newey-West standard errors are reported in parentheses. The symbols ***, **, * represent significance at the 1%, 5%, 10% levels respectively.

Table B.2: Robustness checks on error term for event 1

	(1) Newey-West 1 lag	(2) Bootstrapped s.e.	(3) Weekly average
Treat	2.593***	2.593***	2.519***
	(0.350)	(0.269)	(0.563)
$Post_1$	1.339	1.339*	1.102
	(0.921)	(0.718)	(1.509)
$Treat*Post_1$	-2.364***	-2.364***	-2.290***
	(0.357)	(0.275)	(0.573)
Power	0.0030	0.0030	0.0053
	(0.0046)	(0.0043)	(0.0135)
Oil	0.670***	0.670***	0.656***
	(0.0658)	(0.0523)	(0.112)
Coal	0.166***	0.166***	0.165*
	(0.0605)	(0.0468)	(0.0969)
Temperature	-0.226***	-0.226***	-0.280**
	(0.0774)	(0.0643)	(0.141)
Temperature squared	0.0055**	0.0055**	0.0067
	(0.0026)	(0.0022)	(0.0054)
Constant	-6.713*	-6.713**	-6.294
	(3.538)	(2.690)	(5.671)
Observations	1,759	1,759	366
F-test	1,255.90	-	555.51

Note: The dependent variable is the daily gas price at the hub in columns (1) and (2) and weekly average gas price in column (3). In all specifications, we also include day, month, and year dummies. The symbols ***, **, * represent significance at the 1%, 5%, 10% levels respectively.

Table B.3: Robustness checks on error term for event 2

	(1) Newey-West 1 lag	(2) Bootstrapped s.e.	(3) Weekly average
Treat	0.235***	0.235***	0.225***
	(0.0402)	(0.0330)	(0.0610)
$Post_2$	0.351***	0.351***	0.371***
	(0.0708)	(0.0604)	(0.103)
$Treat*Post_2$	-0.384***	-0.384***	-0.381***
	(0.0482)	(0.0402)	(0.0718)
Power	0.001*	0.0010*	0.0002
	(0.0006)	(0.0005)	(0.0016)
Oil	0.970***	0.970***	0.977***
	(0.00445)	(0.00376)	(0.0070)
Coal	-0.0219**	-0.0219**	-0.0186
	(0.0091)	(0.0088)	(0.0140)
Temperature	-0.0048	-0.0048	-0.0012
	(0.0084)	(0.0075)	(0.0153)
Temperature squared	5.43e-05	5.43e-05	-0.0002
	(0.0004)	(0.0003)	(0.0007)
Constant	1.177***	1.177***	1.064***
	(0.203)	(0.174)	(0.327)
Observations	2,586	2,586	534
F-test	14,870.81	-	7,642.00

Note: The dependent variable is the daily gas price at the hub in columns (1) and (2) and weekly average gas price in column (3). In all specifications, we also include day, month, and year dummies. The symbols ***, **, * represent significance at the 1%, 5%, 10% levels respectively.

Table B.4: Robustness checks on error term for the overall effect of the merger

	(1) Newey-West 1 lag	(2) Bootstrapped s.e.	(3) Weekly average
Treat	2.595***	2.595***	2.522***
	(0.350)	(0.260)	(0.558)
$Post_{overall}$	3.964**	3.964***	3.992
	(1.798)	(1.344)	(3.020)
$Treat*Post_{overall}$	-2.734***	-2.734***	-2.667***
	(0.355)	(0.264)	(0.565)
Power	0.0013	0.0013	0.0010
	(0.0071)	(0.0063)	(0.0175)
Oil	0.717***	0.717***	0.716***
	(0.0542)	(0.0417)	(0.0920)
Coal	0.0020	0.0020	0.0033
	(0.0315)	(0.0235)	(0.0528)
Temperature	-0.143***	-0.143***	-0.167*
	(0.0450)	(0.0362)	(0.0856)
Temperature squared	0.0033**	0.0033**	0.0038
	(0.0017)	(0.0014)	(0.0037)
Constant	3.288	3.288**	3.257
	(2.013)	(1.556)	(3.204)
Observations	2,715	2,715	560
F-test	1,658.09	-	697.62

Note: The dependent variable is the daily gas price at the hub in columns (1) and (2) and weekly average gas price in column (3). In all specifications, we also include day, month, and year dummies. The symbols ***, **, * represent significance at the 1%, 5%, 10% levels respectively.

Table B.5: Baseline regressions with heating degree days (*hdd*) as a control variable instead of temperatures

VARIABLES	(1) Event 1	(2) Event 2	(3) Overall
treat	2.5974***	0.2344***	2.5940***
	(0.5805)	(0.0608)	(0.5788)
$post_1$	1.3266		
	(1.4351)		
$interact_1$	-2.3746***		
	(0.5910)		
$post_2$		0.3592***	
		(0.1023)	
$interact_2$		-0.3853***	
		(0.0717)	
$Post_{overall}$			3.6116
			(2.8069)
$Interact_{overall}$			-2.7230***
			(0.5836)
hdd	0.0063	-0.0001	0.0034
	(0.0059)	(0.0004)	(0.0024)
brent_crude_spot	0.6780***	0.9710***	0.7231***
	(0.0983)	(0.0064)	(0.0759)
coal	0.1450	-0.0226*	-0.0057
	(0.0953)	(0.0137)	(0.0489)
power_prices	0.0047	0.0011	0.0020
	(0.0048)	(0.0007)	(0.0076)
Constant	-9.0919	1.1661***	1.7416
	(5.9520)	(0.2628)	(3.3542)
Observations	1,760	2,586	2,716

Note: The dependent variable is the daily gas price at the hub. We also include day, month, and year dummies. Daily heating degree days are obtained by linear interpolation from monthly data (Eurostat). Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

C. Figures

Figure A1 - Evolution of prices at the ZEE hub and at the TTF hub, 2005 - 2011

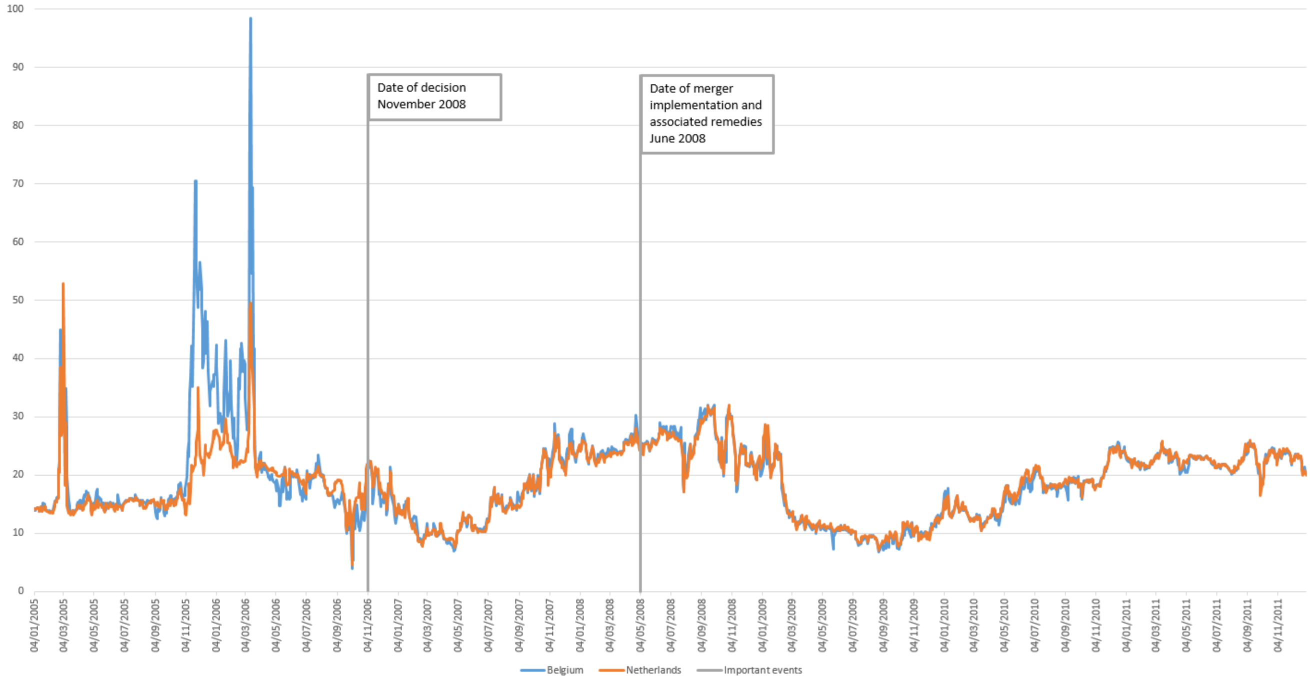

Figure A2 – Map of the EU gas pipeline system

Source: Argiro Roinioti. (2014). The outlook for natural a gas trading hub in South-East Europe. Institute of Energy for South-East Europe.