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- authors: Alessandro Olper, Daniele Valenti, Valentina Raimondi and Daniele Curzi
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The EU Enlargements Treatment Effect on Agricultural Policy

Abstract

This paper exploits the sequences of the European Union (EU) enlargements to investigate their impact on the incoming countries' agricultural policy. We use a quasi-experimental approach – the Synthetic Control method – to identify, country-by-country, changes in the level of agricultural protection of the incoming countries, in comparison to a counterfactual scenario. Our results suggest that earlier 1973 and 1985 EU enlargements show a significant increase in the rate of assistance to agriculture of incoming countries, namely what we call a positive enlargement treatment effect. The opposite result holds for the 1995 and 2004 enlargements, where incoming countries significantly reduced their level of assistance to agriculture, in comparison to a counterfactual scenario. Our empirical findings appear coherent with the evolution and changes in the EU decision-making rules, as well as with insights from the political economy of fiscal federalism.

Keywords: EU Enlargement, Agricultural Policy, CAP, Political Economy, Synthetic Control Method.

JEL Classification: D72, F13, O13, P16, Q18.

1. Introduction

The aim of this paper is to answer the question: Does the entrance into the EU lead to a reduction or an increase in the level of agricultural protection in the incoming countries. To this purpose, we study to what extent EU incoming countries significantly changed their agricultural policy, and looking particularly the direction of this change.

To answer this question, we exploited the sequence of EU enlargements of the last fifty years, by comparing two different agricultural policy scenarios. The first one considers countries' adoption of the actual common agricultural policy (CAP) developed within the EU institutional framework. The second one examines a hypothetical agricultural policy scenario under a counterfactual situation where each Member State has implemented its own agricultural policy at the national level, under the hypothesis of non-EU entrance.

Our research design exploits the property of a quasi-experimental approach – the Synthetic Control Method (SCM) (see Abadie et al. 2010). More specifically, we investigate what we call the “EU Enlargement effect”, by exploiting four different enlargements. The first one refers to the United Kingdom (UK), Ireland and Denmark in 1973; the second one to Spain and Portugal in 1986; the third one to Austria, Finland, and Sweden in 1995; and finally, the 2004 EU enlargement considers five out of eight new member states, i.e., Czech Republic, Hungary, Poland, Slovakia, and Slovenia. For each new EU incoming country, we compare the level of agricultural protection in the post-enlargement period (ten years) with the level of protection of its own counterfactual scenario. The latter is built exploiting a sample of non-EU countries and it is estimated by using the SCM in the ten years pre-dating the formal accession date. Our outcome variable of interest, the level of agricultural protection, is measured as the nominal rate of assistance (NRA) in agriculture (see

Anderson and Nelgen, 2013). The NRA incorporates border policies, such as tariffs and non-tariff barriers, which in the period under investigation represented the bulk of the EU CAP support. However, it is worth noting that it does not include other kind of CAP subsidies, such as rural development payments, or direct (decoupled) income payments introduced by the Fischler reform. From this perspective, the results of our counterfactual analysis, which consider the policy outcomes delivered by the EU decision making process, can be evaluated only in terms of its economic dimension. This of course may represent a potential limitation of our analysis, because other EU policy objectives (e.g. social and environmental ones), are disregarded.

As explained in the methodological section, this research design offers several conceptual and practical advantages compared to standard econometric tools used in previous related studies (see for example Von Cramon-Taubadel et al. 2013).

Our main findings show that the EU enlargement treatment effect on agricultural policy crucially depends on the timing of the EU enlargement. Considering the first two enlargements (i.e., 1973 and 1986), EU incoming countries show a positive treatment effect, that is a significant increase of the level of agricultural protection vis-à-vis the counterfactual. Interestingly, this effect is progressively reversed moving to the 1995 and 2004 EU enlargements. In the last two cases, our results show that EU incoming significantly reduced the level of agricultural protection compared to the counterfactual. We argue that these results are in accordance with the evolution of the EU decision-making process, and a political economy interpretation of the fiscal federalism.

Our paper makes both a methodological and an empirical contribution. From a methodological point of view, we propose a simple but coherent research design to address identification problems in studying questions of comparative politics. Yet, by focusing on the agricultural policy, our results contribute to a broad literature studying the political

economy of the CAP and the evolution and functioning of the EU decision-making process (e.g. Tsebelis, 1994; Crombez, 2008; Swinnen 2015; Greer and Hind, 2012; Greer, 2017), as well as the emerging literature on the European Added Value (EAV) (e.g. Medarova-Bergstrom et al. 2012; Heinemann et al. 2013). From this perspective, we can draw some policy implications from our study.

The remainder of the paper is organized as follows. Section 2 presents an overview of the conceptual and applied works on the CAP decision process. Section 3 illustrates the methodology (based on the SCM), the data and the variables used in our analysis. The empirical results are presented in Section 4. Finally, Section 5 concludes.

2. Background and related literature

In this section we shortly review the empirical and theoretical literature on the EU decision-making process. Our focus is primarily on studies related to the CAP, also considering the emerging literature on the European Added Value (EAV) based on fiscal federalism.¹

2.1 Studies on the CAP decision-making process

The analysis of the European Union (UE) decision-making process is an important research area in the fields of political economy and comparative politics. One relevant policy area that attracted the attention of many scholars is the Common Agricultural Policy (CAP). This policy as an effect of its historical importance in the EU budget² represents an ideal domain to better understand how the EU decision-making process works in practice (see De Gorter and Swinnen, 2002; Pokrivcak et al. 2006; Greer, 2017).

¹ The first formal definition of EAV appeared in a Commission working paper accompanying the Commission's proposal for the Multiannual Financial Framework in 2011, titled: "A Budget for Europe 2020". This document defines EAV as "the value resulting from an EU intervention which is additional to the value that would have been created by member state action alone." See Medarova-Bergstrom et al. (2012) and Heinemann et al. (2013) for further discussion about EAV.

² The share of the EU budget items absorbed by the CAP, was equal to around 80% in the early phase (1970) and it is currently equal to around 40% (2018).

Within the broad class of empirical studies on the political economy of the CAP, we can distinguish two main approaches. The first one refers to the informal narrative analysis of the CAP decision-making process (e.g., Neville-Rolfe, 1984; Senior-Nello, 1984; Petit et al. 1985; Runge and Von Witzke, 1986; Moyer and Josling, 1990; Swinnen, 2001). The second one, embeds more quantitative (mainly reduced form) econometric studies where the EU policy outcome is explained by a set of economic and political variables (e.g., Burton, 1985; Gallagher, 1988; Von Witzke, 1986; Mahé and Roe, 1996; Olper, 1998).

The narrative studies often exploit specific reform episodes to understand the complex interaction among EU institutions, interest groups and national interests. Historically, the EU decision-making process has been interpreted as a game played at the national and the supranational inter-state bargaining levels (Olper, 1998). At least before the institutional reforms of the last decades,³ the division of power between the Commission (the agenda setter) and the Council of Ministers, as well as the Council voting-rules, were important factors in explaining the supranational inter-state bargaining level. The European Commission has played a relevant role as policy initiator and mediator of national interests. On the one hand, the Commission is often seen as the natural European target of the main agricultural lobbies. On the other hand, the influence of the lobby has implications for each Member State, as the Council of Ministers represented, and still represents, a key actor of the CAP decision-making process.

Before the enforcement of the Single European Act in 1987 and the Maastricht Treaty in 1999, which significantly increased the use of qualified majority rule, a key element in the

³ Five major treaty reforms since the 1980s have affected the EU decision-making process. First, the Single European Act, entered into force in 1987; Second, the Maastricht Treaty, starting from November 1993; Third, the Amsterdam Treaty, which was enforced in May 1999; Fourth, The Nice Treaty that was implemented starting from February 2003; Finally, The Treaty of Lisbon of October 2007, which entered into force in 2009. See Crombez (2008) for an in-depth discussion of these institutional reforms and their implications for the CAP decision-making process.

design of the CAP was the custom of taking ‘unanimous’ decisions at Council meetings.⁴ This favoured a ‘fair’ distribution of the benefits (and costs) among member states (Runge and Von Witzke, 1987). Thus, in the first period of the CAP the EU Member States were able to pursue their own agricultural policy objectives despite the existence of a common supranational policy.⁵

Many observers at the time argued that the EU Members have had an incentive to implement more expensive policies during the annual CAP prices review - encouraging higher level of protection - because they share the burden of the EU agricultural support costs. This phenomenon is also known as “restaurant table effect” (Pearce, 1983; Von Witzke, 1987). However, other authors are critical to the idea that the supranational structure of the CAP systematically generates higher protection rate (see Pokrivcak et al. 2001).

Initially, the analysis of the CAP through a more formal econometric approach have not explicitly considered the specific economic and political interests of individual Member States. For example, studies such as Burton (1985) and Gallagher (1988), focused on a Council preference function approach or a bargaining framework between the (EU) producers and public interests (consumers and taxpayers), respectively. Differently, Harvey (1982), and more formally Olper (1998), considered more explicitly the economic and political interests of individual Member States, showing that standard political economy determinants of agricultural protection (Homna and Hyami, 1986; Gardner, 1987), such as

⁴ The recourse to the unanimity rule was a result of the ‘Luxembourg Compromise’ (1966), which enabled each member state to exert its veto power whenever it felt its national interests were threatened. In 1987, with the ratification of the Single European Act, a number of institutional reforms were introduced – by the consultation and co-operation procedure – that conferred new powers on Parliament and the Commission, favoring an increasing use of the vote by a qualified majority. In 1991, with the Treaty of Maastricht and the creation of the European Union (EU), the qualified majority rule was extended to new areas.

⁵ This was also the result of a specific institutional setup of the CAP, called the “principle of financial solidarity” that implies to what extent the costs and benefits of the CAP were shared among member states. Indeed, while each country contributed to the EU budget with a fixed amount based on their economic size (e.g., GDP), countries with large agricultural sectors (and low GDP) tended to be advantaged, because they obtained disproportional gains, while bearing only a small fraction of the budget costs.

the level income and the number of farmers, were relevant variables in explaining variation of the CAP support level.

More recently, the political economy literature on the CAP has been enriched by more formal theory (e.g., Henning, 2004; Pokrivcak et al. 2006; Crombez, 2008).⁶ Many of these contributions applied spatial political economy models to the functioning of EU institutions (see Tsebelis, 1994; Tsebelis and Garret, 1996; Crombez, 1996, 1997, 2000), which provide interesting insights to better understand the CAP policy reforms. These models consider both the players' preferences and their interaction between the decision rules and changes in the EU institutional setting. One of the first paper that applied spatial models to the CAP has been Pokrivcak et al. (2006), by investigating how the Member States preferences on the CAP have affected the EU decision-making process. Similarly, Crombez (2008) analyses how changes in the EU institutional setting have affected the Commission's position about pro-market CAP reforms. This analysis is particularly useful for our empirical exercise because it encompasses four EU enlargements that occurred under different institutional setting. Because of the Single European Act of 1987, the Commission increased the set of available polices from unanimous voting (UV) toward qualify majority voting (QMV). Both the expansion of the Commission choice set, and the new co-decision rule of the European Parliament introduced by the Maastricht Treaty (1993), shifted the Commission position toward pro-market CAP reform.⁷

The analysis of Crombez (2008) is consistent with the view that both institutional reforms and other external constraints, such as the WTO agreement of 1994 (see Pokrivcak et al. 2006), played an important role in explaining the deviation from the status-quo bias

⁶ Other formal approaches to the EU decision-making process has been based on the public choice literature and the power indices of Shapley and Banzhaf (see, e.g., Winkler, 1998; Widgren, 1994; Hosli, 1996; Baldwin, 2001).

⁷ This clearly holds under the assumption that the European Parliament is more reformed minded than the pivotal countries (see Crombez, 2008, for details).

of both the McSharry (1993) and the Fischler (2003) reforms of the CAP. Broadly speaking, our empirical results reported below are not in contradiction with this interpretation.

2.2 *European Added Value, Fiscal federalism, and the CAP*

Commentato [DC1]: Si può tagliare un po' in questa sezione?

The European Added Value (EAV) can be defined as the difference in net benefits between a policy action taken at the EU level with respect to the one implemented at the national level (see Heinemann et al. 2013, for a review of the concept). For our discussion, it is therefore appropriate to investigate the reasons why a policy implemented at the EU level should provide added value vis-à-vis a national one.

The existing literature on EAV is based on two main theories: the standard theory of fiscal federalism (Oates, 1972, 2005) and its extension enriched by political economy arguments (Persson et al. 1996; Alesina et al. 2005a; Alesina et al. 2005b; Heinemann et al. 2013).

Broadly speaking, the main idea behind the fiscal federalism is that many public policies have cross-border externalities (spill-over) and scale economies. In this respect, these policies cannot be efficiently absorbed by a (decentralized) national decision-making process.

The political economy logic applied to fiscal federalism results in different, and sometimes contradictory, arguments about the pros and cons of a supranational decision-making process vis-à-vis a domestic one. First, the concept of “race to the top” in public spending by national governments, which engage in a sort of subsidies competition that leads to a waste of resources (Janeba, 1998). In addition, national governments tend to be prone to (short) electoral cycle, which partially preserve policy making taken at the EU level. Similarly, policy decisions at the EU level can be less affected by the pressure from lobbying activities (Alesina et al. 2005a).

However, there are also counterarguments to the above logic. For example, Vaubel (1999) finds that the effectiveness of lobbying on the decision-making is lower with decentralized politics. Similarly, Persson et al. (1997) and Tabellini and Wyplosz (2006) suggest that when lobby preferences are less heterogeneous, a centralized decision-making process is more prone to satisfy the lobbies' pressure. This is consistent with early stages of the CAP decision-making process. Indeed, the few Member States involved at that time (from EU6 to EU9 until 1981) were associated with a lower level of heterogeneity of the farm lobbies preferences, a situation that changed progressively as an effect of the EU enlargements.

To the best of our knowledge, Von Cramon-Taubadel et al. (2013) is the only study that formally tested to what extent the CAP produces EAV.⁸ These authors use standard regressions tools applied to a sample of (non-EU) OECD countries to build the counterfactual scenario. This regression analysis has been used to investigate the behaviour that EU countries would have had under the hypothesis of a 'decentralized' decision making process. Interestingly, they find that in the recent period (2009), on average, the CAP caps the EU expenditure for agricultural policy (in comparison to the counterfactual). Under this approach, a low amount of public spending in agriculture at the EU level implies European Added Value, based on the assumption that agricultural spending is harmful, or it is at least less beneficial, than other types of public expenditures (see Heinemann et al. 2013).⁹

However, there are also limitations in the study of Von Cramon-Taubadel et al. (2013), as recognized by the same authors. First, the empirical results and the policy

⁸ See Heinemann et al. (2018) for a recent contribution on the EAV of the Commission proposal of CAP reform beyond 2020.

⁹ Though this statement probably encountered the agreement of the majority of economists, to properly evaluate to what extent farm money are spent "efficiently" or not, it is also necessary to carefully investigate which type of policy instruments are currently used to redistribute money. For example, moving from a distortive coupled farm policy (e.g. price support), toward a less-distortive fully decoupled farm payments (e.g. single farm payments), could make a big difference.

recommendations are not robust to changes in the choice of countries used to build the counterfactual.¹⁰ Second, their analysis refers only to the last period of the CAP (2009), without accounting for the changes in both the EU decision-making structure and the international context. Third, this study does not allow for the so called ‘before-after’ comparison, which is particularly relevant to investigate the impact of EU incoming memberships on agricultural protection. This type of empirical comparison must be at the centre of any serious attempts to quantify added value (Heinemann et al. 2013), and this is what we propose in the next sections.

3. Methodology

3.1 The Synthetic Control Method

The SCM is widely used in several applied economics evaluation studies. It has been firstly proposed by Abadie and Gardeazabal (2003) and then further refined by Abadie et al. (2010, 2015). In this section, we focus especially on the main advantages that this method presents with respect to other widely used methodologies in social science studies. A detailed and formal presentation of the SCM is provided in Appendix A of the Additional Material Section.

The SCM has been originally conceived to estimate the effect of an aggregate intervention on a given outcome of interest in a comparative case-study environment. Aggregate interventions are events that apply at an aggregate level, which however affect only one or a small number of units, such as a tax policy change or an educational program affecting a specific country, region or city (Abadie 2021).¹¹

¹⁰ For instance, subsidies are lower in Australia and New Zealand but much higher in Switzerland. These countries are all used to build the counterfactual in the work by Von Cramon-Taubadel et al. (2013).

¹¹ More recently, some works in the literature have applied the SCM to scenarios where the number of treated units was very large (e.g. Acemoglu et al., 2016).

The widely used regression analyses applied to estimate the effect of interventions in social science studies when dealing with large-scale and rare effects often rely on time series analyses or comparative case studies. Time series analyses are particularly indicated when considering interventions that are expected to have short-term and high-magnitude effects (Abadie, 2021). The main problem of these methodologies is that they are not suitable to estimate long-run effects, as other shocks may affect the estimated effect. Comparative case studies, where the effect of an intervention is estimated considering the difference in the outcome variable of interest of treated and untreated units, are largely used in social science. One of the main problems in this case is that the selection of the counterfactual is not formalized and frequently based on an informal statement of affinity between treated and control units (Abadie, 2021). This issue is reinforced when the available units are in a small number, as a single unit cannot represent a valuable counterfactual.

The SCM is based on the idea that when a few aggregated entities are available, a combination of untreated units represents a better counterfactual than a single unit. The SCM selects a weighted combination of (untreated) control countries, called the synthetic control, with the aim of minimizing the differences between the treated and the untreated countries according to some salient characteristics. The construction of the synthetic control is done considering the pre-treatment period. In our analysis, we thus consider for each country the years *before* they joined the EU. Then, by comparing the trend in the outcome variable (in our case the level agricultural protection) between the synthetic control and the treated country in the years after the treatment (in our case *after* joining the EU), we can establish to what extent the treated unit behaves differently from its counterfactual. In our analysis we seek to establish whether the dynamic of the level of protection induced by the CAP in the treated country is significantly different compared to its counterfactual situation.

We believe that the properties of the SCM fit better our research question than those of other estimation methodologies, such as linear regression models. This is firstly due to the capacity of the SCM to deal better with case studies where the number of observed units is limited.

The construction of the counterfactual is a core advantage of the SCM. The SCM precludes extrapolation, as the weights built are nonnegative and sum up to one. In contrast, weights in regressions can assume negative values, and thus allowing extrapolation out of the support of the data (Abadie et al., 2021). Moreover, the linear regression models exploit the extrapolation to allow the fit between treated and control units, even in the case these are totally different (Abadie et al., 2021). In contrast, the SCM makes this evident and transparent, and in case of poor fit of the synthetic control with the treated unit, it is possible to disregard the result of the experiment.

The construction of the counterfactual in the SCM is transparent. This is important as it is possible to assess the discrepancy of the counterfactual with respect to the treated unit. Moreover, as the contribution of the units in the donor pool is explicit, expert knowledge can interpret the estimate counterfactual and assess the validity of the synthetic control. The SCM is therefore a flexible estimator, because the set of potential controls, the so-called donor pool in the language of Abadie et al. (2010), can be restricted to make underlying country comparisons more appropriate. The units in the donor pool should be therefore chose judiciously, as the inclusion of units that may be unsuitable for a comparison with the treated units could lead to bias the final results.

Another important advantage of the SCM is the possibility to assess the estimated treatment effect over time (short-run vs. long-run effect). This is a fundamental aspect, as policy interventions may take same time to show their effect, or they could even dissipate over time. Finally, differently from the more standard differences-in-differences estimator,

a crucial property of the SCM is its ability to control for time-varying omitted variables bias.

However, the SCM is not without drawbacks. First, as it is typical in the program evaluation literature, our estimator does not distinguish between direct and indirect causal effects of joining the EU on agricultural protection outcomes. As a result, we are just able to estimate the average overall causal effect, although this shortcoming does not seem a relevant limitation in our specific context.

The second shortcoming is given by the small number of observations involved in such comparative case study approach. This renders unfeasible the use of standard inferential techniques to assess the significance of the results. To overcome this limitation, Abadie et al. (2010) suggest the use of in-space placebo tests. The latter stem from a comparison of the magnitude of the estimated effect on the treated country, with the size of those obtained by assigning the treatment randomly to any (untreated) country of the donor pool. If the magnitude of the effect of the treated country is larger than most of the placebo tests, then we can conclude that the result is robust. In what follow, we also apply an extension of these placebo tests.

In our study we are particularly interested in the “average” enlargement effects over time. That is, to what extent, on average, the four enlargements of the EU display significantly different treatment effects. This is relevant because the four enlargements occurred under quite different EU institutional setting. Thus, it may be of some interest to measure the average treatment effects aggregated by enlargement event. In doing that, we follow Cavallo et al. (2013) and Olper et al. (2018). In Appendix A of the Additional Material Section, we present in detail how these average effects are computed as well as how the significance of the effects is inferred.

3.2 Data, Measures and Donor Pool Selection

To study the effect of the adoption of the CAP on the level of countries' agricultural protection, we exploit 4 different EU enlargement episodes, namely those occurred in the years 1973, 1986, 1995 and 2004.¹² During these enlargement processes, the six founding countries (Belgium, France, Germany, Italy, Luxembourg and the Netherlands) were joined by Denmark, Ireland and the United Kingdom in 1973, Spain and Portugal in 1986, Austria, Finland and Sweden in 1995 and, finally, Czech Republic, Estonia, Cyprus, Latvia, Lithuania, Hungary, Malta, Poland, Slovakia and Slovenia in 2004.¹³ The European Union thus moved from 6 to 25 members. Hence, the year of countries' EU accession indicates our treatment variable, while the result of the full adoption of the CAP rules, as reflected in the agricultural level of protection, is the outcome variable.

The level of agricultural protection, which represents our outcome variable (Y_{it}), is measured using the Nominal Rate of Assistance to agriculture (NRA) from Anderson (2009) and Anderson and Nelgen (2013) dataset. The NRA is an indicator of the extent of subsidization (positive NRA) or taxation (negative NRA) of the agricultural sector through government policies (largely border trade policies, such as tariffs and non-tariff barriers, and distortive coupled subsidies). The use of this variable and data source, rather than other indicators from OECD data (e.g. producers subsidy equivalent), is crucial for our research design. Indeed, it allows us to measure the level of protection in agriculture that goes back to 1963, hence ten years before the first EU enlargement of 1973. Only the availability of data on such a long period can allow us to base our econometric approach on the SCM.¹⁴

¹² We cannot consider the 1981 enlargement to Greece, due to lack of data on the outcome variable (agricultural protection) for that country.

¹³ Note that concerning the 2004 enlargement, our analysis does not consider the following countries due to the lack of data on NRA in the pre-treatment period (i.e. before 2004): Cyprus, Malta, Estonia, Latvia and Lithuania

¹⁴ Note that OECD agricultural support data start in 1986, thus precluding us to estimate not only the effect of the first EU enlargement of 1973, but also the one of 1986 to Spain and Portugal. This is because, our SCM approach needs a sufficient time span (e.g. ten years) before the treatment, to build the (synthetic) counterfactual.

We use a vector of covariates to build the synthetic controls. This vector is chosen according to previous cross-country studies on the determinants of agricultural protection (e.g. Honma and Hayami, 1986; Swinnen et al. 2000; Olper, 1998; Olper, 2007). The first two variables used in our analysis are the real per capita GDP and the Polity2 index, sourced from Penn World Table and Polity IV datasets, respectively. The latter provides information on the quality of democracy (see Marshall and Jaggers, 2009).¹⁵ Moreover, we also include in the set of exogenous variables the agricultural employment share, sourced from FAOSTAT, and the agricultural net export share. The latter is a variable of agricultural trade orientation, measured as the difference between export and import in agriculture, divided by the domestic production value. Finally, to increase the quality of synthetic controls, namely to what extent it actually mimics the behaviour of agricultural protection of the treated country in the pre-treatment period, we used as additional controls lagged levels of NRA, and in particular NRA values at 10 and 5 years before the treatment, as well as the NRA level in the treatment year.

The donor pool, namely the set of countries that are used to build each synthetic control, is represented by OECD countries. We decided to focus on this sample, as countries in this sample are more likely to share a similar level of development with the EU one, rather than least development countries. Note moreover that countries successively joining the EU in one of the different enlargement episodes can be part of the donor pool, if they satisfy the condition that in a specific country-experiment they are not yet EU member in any time of the pre-treatment and post-treatment period. In this perspective, for each

¹⁵ The Polity2 index assigns a value ranging from -10 to +10 to each country and year, with higher values associated with better democracies. We code a country as democratic (= 1, 0 otherwise) in each year that the Polity2 index is strictly positive. A political reform into democracy occurs in a country-year when the democracy indicator switches from 0 to 1. See Giavazzi and Tabellini (2005) and Olper et al. (2014) for details.

country-case study, we set the pre-treatment period as 10 years before the treatment. Analogously, the post-treatment is set 10 years after the EU accession occurred.¹⁶

It is worth noting that a peculiar characteristic of our donor pools is the lack of countries that are located at the border with the treated units. As a result, the stable unit treatment value assumption (SUTVA) holds. According to the SUTVA assumption, the presence in a donor pool of a country at the border with the treated one, may bias the results, as the treatment may have spill over effect with closest neighbourhoods. However, the structure of our database allows us to discard the possibility of similar spill over effects.

4. Results

This section summarizes the results obtained from our 13 SCM experiments, where we explore the effect of different EU enlargements on countries' agricultural protection, measured as *NRA*. We first present the results of our analysis by country, looking at the magnitude and significance of the effects, as well as to the quality of the matching between treated countries and their synthetic control. Next, we present the results by aggregating the estimated effects by EU enlargement episodes.

4.1 Quality of the Synthetic Controls and Country level results

In our empirical analysis, we estimate the effect of countries' EU accession on their level of agricultural protection using the SCM, considering 4 different EU enlargement episodes. One of the main advantages of the SCM is that it allows assessing the reliability of each country case study analysis, by measuring to what extent the synthetic control fits the characteristics of the treated unit in the pre-treatment period.

¹⁶ For instance, Portugal and Spain can enter the donor pool of Denmark, Ireland and UK (EU accession 1973), as they enter the EU in 1986 (13 years after the EU accession of Denmark, Ireland and UK). In contrast, east Europe countries like Czech Republic or Hungary cannot be considered in the donor pool of Austria, Finland and Sweden (EU accession 1995) as they enter the EU in 2004 (9 years after the EU accession of Austria, Finland and Sweden).

Table 1 presents the average values of each treated unit and the relative synthetic control, for those variables employed for the construction of each counterfactual. The discrepancy between values for treated units and their synthetic control provides information about the accuracy of the balance in the set of exogenous variables. The balance plays a crucial role in the ability of the synthetic control to mimic the behaviour of the treated country in the pre-treatment period. The higher is the balance between the covariates in the pre-treatment period, the higher will be the quality of matching and hence the reliability of the SCM experiment.

[Table 1 about here].

Table 1 shows that all country case studies, with the exceptions in some covariates, provide a reasonably good balance between the treated unit and the respective synthetic control. Note that this is especially true when the lagged *NRA* are considered at T_0-10 , T_0-5 , and T_0 , suggesting that our synthetic countries behave quite similarly with respect to the treated countries in the pre-treatment period.

More formally, the ability of the synthetic controls to fit the (pre-treatment) evolution of the outcome variable (i.e., *NRA*) of the treated units is revealed by the root mean square prediction error (RMSPE), which is reported in the first column of the Table 1. This represents an indicator of the discrepancies between the level of protection in agriculture of the treated units and the associated synthetic controls in the pre-treatment period. Overall, the RMSPE values indicate a good fit in our SCM experiments, with RMSPE values lower than 0.10 in 11 out of 13 cases, while in only two cases (i.e., Spain and Finland) is higher than 0.10, but still quite low.

In summary, the preliminary assessment based on the criterion of the RMSPE is encouraging and supports the view that our SCM experiments are reliable and sufficient accurate. Countries and the relative weights constituting the synthetic control for each SCM experiment are described in detail in Appendix 1.

Table 2 reports the average treatment effect (ATE) of the EU accession on the level of incoming countries' agricultural protection. Countries are ranked from the highest increase in the nominal rate of protection at T_0+10 to the lowest (i.e., highest reduction in the NRA). The last three columns show the estimated *p-value* for each country-case study experiment, at T_0+5 , and T_0+10 years after the treatment, as well as the average over the considered period. Overall, the results provide a clear picture.¹⁷

[Table 2 about here].

On the one hand, countries at the top of Table 2 are those that joined the EU during the first two considered enlargements and show an increase in the level of agricultural protection vis-à-vis the respective synthetic counterfactual. Thus, all countries that joined the EU in 1973 (i.e., United Kingdom, Ireland and Denmark) show a steady increase in the nominal rate of assistance value compared to the counterfactual over the ten years of post-treatment period. Denmark shows a slight increase in the nominal rate of assistance between T_0+5 , and T_0+10 , while the UK registers the highest increase (+160% at T_0+10). On the contrary, Spain and Portugal, which joined the EU in 1986, show a high increase in their level of agricultural protection especially in the short-run (from T_0 to T_0+5). However, the EU enlargement treatment effect, though still positive, become smaller for both Spain and Portugal at time T_0+10 .

On the other hand, all countries that joined the EU in 1995 and 2004 show a *reduction* in their level of agricultural protection compared to their counterfactuals. At the end of the post-treatment period, Poland and Slovakia registered the highest reduction, which amount to -68.6% and -59.1%, respectively. All these countries show a similar path in the evolution

¹⁷ The results obtained through the SCM country by country for each enlargement episode are also shown in the Additional Material Section A, from Figure A.1 to Figure A.4. These figures allow to show more clearly both to what extent the different synthetic controls mimic the NRA trajectory of the treated units in the pre-treatment period and the different evolution of the NRA in the treated and the synthetic control after each EU enlargement episode.

of the nominal rate of protection compared to their counterfactuals, with a strong reduction in the short-run period, which declines in the long-run. However, it is important to notice that due to the lack of data from 2012 onward, the last post-treatment year for countries making their accession in 2004 is set at the year 2011, namely 7 years after the treatment.

Turning the attention to the significance of our SCM experiments, Table 2 shows that 9 out of 13 country-case studies present an average *p-value* over the period (computed as shown in section 3 from their placebo test) lower or equal to 0.10, which then we may consider as statistically significant. Considering the short-run effect at year T_0+5 , all the estimates but two (Ireland and Hungary) have a *p-value*<0.10, and in eight countries out of thirteen we have a *p-value*<0.01. Similarly, in the long-run (at year T_0+10), ten estimates out of thirteen are statistically significant with a *p-value*<0.01.

[Figure 1 about here]

Figure 1 plots the placebo test for each treated country.¹⁸ The bold line in the different graphs reports the *NRA* difference between each treated unit and the respective synthetic control. The grey lines report the outcome differences between each (fake) treated country from the donor pool and their synthetic control in the placebo tests. The visual inspection tends to confirm the results from Table 2, and specifically no evidence of a significant EU enlargement treatment effect for Ireland, Poland, and Hungary, and the presence of non-satisfactory placebo tests for Denmark.¹⁹ However, in all the SCM experiments the bold line of the treated country tends to be positioned well above (or below) in the post-treatment

¹⁸ Note, to have a reliable *p-value* estimation, some fake experiments have been excluded from each country-case studies placebo tests, due to the poor fit in the pre-treatment period. Therefore, some placebo test may present a low number of fake experiments.

¹⁹ The case of Poland deserves some attention. Indeed, Poland shows in the long-run a significant reduction of agricultural protection that clearly exceeds those of other fake experiments. In this case, the insignificant average estimated *p-value* of 0.2 (see the last column of Table 2), is due to the fact that the reduction of the *NRA* started two years after Poland EU accession, as it is clear from Figure 2. From this perspective, we may argue that also Poland shows a significant reduction in the level of agricultural protection, but only in the long-run.

period of the majority of the grey lines of the in-space placebo tests. These results reinforce our conclusion about the direction of the EU enlargement treatment effects in the first two enlargements, where the *NRA* of treated units increases in comparison to the counterfactual, and the third and four enlargements where, instead, the *NRA* of the treated countries substantially decreases in comparison to the counterfactual synthetic control.

4.2 Aggregated results across enlargement episodes

The results showed in the previous section are clear-cut and provide empirical evidence of two opposite effects of countries' EU accession on the level of agricultural protection. Earlier EU enlargements show a significant increase in the agricultural protection for incoming countries. In contrast, for more recent enlargement episodes (i.e. from 1995 onwards), the incoming countries experienced a sharp reduction in the level of protection in agriculture. We further test the robustness of these findings at the country-level, by aggregating the estimated effects within each EU enlargement episode. Thus, this analysis allows testing to what extent the timing of the enlargement matters in order to affect the level of agricultural protection.

Figure 2 plots the dynamic ATE of the EU accessions on the nominal rate of assistance, when countries are aggregated in accordance with the timing of the EU enlargement episodes, using equation (5) in Appendix A.

[Figure 2 about here]

Overall, the dynamics of the ATE are qualitatively similar to those resulting from the single-country case studies, though now the level of significance is higher. This is simply because the aggregation process leads the degree of freedom to measure the placebo test through permutations (see equations 6 and 7 in Appendix A) to increase substantially. Specifically, our empirical evidence shows that the effect of the EU enlargements on the nominal rate of assistance in 1973 is positive and statistically significant at 5% level, in

both the short-run at T_0+5 ($p\text{-value} = 0.04$) and in the long-run at T_0+10 ($p\text{-value} = 0.02$). Similarly, we find a positive impact of the 1986 EU enlargement on the *NRA*, which is statistically significant at 1% in both the periods T_0+5 and T_0+10 . When considering the effect of the EU enlargement of 1995 on the reduction of the nominal rate of assistance, our results suggest that this effect is statistically significant in both the periods T_0+5 ($p\text{-value} < 0.01$) and T_0+10 ($p\text{-value} = 0.04$). Finally, the effect of the 2004 EU enlargement on agricultural protection is negative and statistically significant with a $p\text{-value} < 0.01$, in both periods T_0+5 and T_0+7 .

In summary, the analysis of the effect of the EU enlargement on the level of agricultural protection of the incoming countries provides empirical evidence that countries entering the EU during the 1973 and 1986 EU (at the time CE) enlargements, experienced a statistically significant *rise* in the level of agricultural protection. Conversely, the EU enlargement treatment effects on the level of protection in agriculture turns out to be negative and statistically significant, during the 1995 and 2004 EU enlargements.

4.3 Robustness checks

This section aims at presenting some robustness checks of the empirical analysis presented in the previous section. We focus in particular on two main aspects that may affect our findings. The first one aims at capturing a potential anticipation effect in the application of the policy. The second one is methodological and allows testing the main results obtained through the SCM with a difference-in-difference estimator.

As it is common to many research designs aimed at analysing the effect of a policy intervention on a given outcome variable over time, the SCM may show biased results if forward-looking economic agents anticipate their reaction to the policy intervention (Abadie 2021). In our case, the potential existence of an anticipation effect of the policy is due to the well-known reason that accession countries do not join the EU abruptly, but they

follow a preparation process that is a long and gradual and may last for many years. According to the EU legal treaty texts, long enough before the actual EU enlargement, the Candidate Country signs a European Union Association Agreement. Association Agreements (AA) offers the Candidate Countries tariff-free access to some or all EU markets (industrial goods, agricultural products, etc.). In some cases, AA include a Free Trade Agreement (FTA) with the EU. Already at this stage, tariff-free trade with the EU implies a progressive harmonisation of domestic protection for instance in industrial goods or agricultural products.²⁰ To test the existence of a potential anticipatory effect of the policy, the SCM allows backdating the intervention to a period before, so that our estimation can capture the full extent of the treatment effect. We have therefore run the analysis country by country backdating the treatment period three years in advance and we have then aggregated the results by enlargement episode.²¹

The results of this test are presented in Appendix B, Figure B.1. The graph for the average effect of the 1973 enlargement, where thus the treatment year has been shifted back to 1970, shows a higher level of agricultural protection in the treated countries with respect to the synthetic control starting from T+2 (i.e. 1972), which however become stronger starting from T+3 (i.e. 1973) and T+4 (i.e. 1974). This finding confirms our main results, although we cannot exclude the existence of an anticipation effect in the year before the actual EU accession (i.e. 1972). When considering the results for the 1986 enlargement, the average effect of treated countries was already higher at the time of the (anticipated) treatment (i.e. 1983), and become more evident since T+2 (i.e. 1985). However, the results of this robustness check should be taken with caution, due to the poor fit of treated countries and synthetic control in the pre-treatment.

²⁰ We thank an anonymous referee for pointing this issue to our attention.

²¹ It is worth noting that even in presence of any anticipation effect, our main results would be not undermined. This is because, if any, an anticipation effect of the policy would only potentially lead to a downward bias of our main findings.

Finally, the results of this test for the last two enlargements (i.e. 1995 and 2004) clearly exclude any anticipation effect. The average level of agricultural protection gets lower with respect to the synthetic control starting from T+3 (i.e. 1995) when anticipating the enlargement at 1992, and from T+6 (i.e. 2007) when anticipating the effect to 2001. Overall, the results of this robustness check suggest that possible anticipation effects due to pre-accession policy are not driving our results. This conclusion holds true for all the investigated enlargements, but the 1986 one, where, however, the reliability of the result is quite weak due to the poor fit of the synthetic control with respect to the treated countries in the pre-treatment period.

We then test whether our results obtained with the SCM are robust to the use of an alternative estimator. To do that, following Cerulli and Ventura (2019), we use a difference-in-differences (DID) estimator that can be applied to the case of binary time-varying treatments with pre- and post-intervention periods, which also allows testing the parallel trend assumption of the DID method.²² Before presenting the main results of this test, it is worth mentioning that the comparability of the results between the SCM and a the DID estimator is limited, due to the differences described in the methodological section. Moreover, in our specific case, the constraints we impose to the SCM in the selection of the counterfactual (e.g. EU incoming countries during the 10 years post-treatment period cannot enter in the counterfactual) are not actually applicable to the DID case. Therefore, a comparison of the results of the SCM with those of the DID should be taken with caution. Bearing these caveats in mind, we believe that testing the robustness of our results with another estimator may be of some interest.

Following Cerulli and Ventura (2019), we use a DID model that can be applied to the case of binary time-varying treatment with pre- and post- intervention periods and it allows

²² The methodology is explained in details in Appendix C of the Additional Material Section.

for two distinct tests for the parallel-trend assumption.²³ Table C.1 of the Additional Material Section show the results obtained with the proposed DID approach. The results in column 1 concern the 1973 enlargement, and confirm that EU incoming countries significantly increase their level of agricultural protection with respect to the other countries. The estimation also includes three leads terms, which allow testing the parallel trend assumption by analysing whether any anticipation effect was observed during that period. In this respect, we are unable to reject the null hypothesis of parallel trend assumption. The results in column 2 suggest that the 1986 enlargement led to an increase in the level of agricultural protection, which is however not statistically significant. The results in columns 3 and 4, concerning the 1995 and 2004 enlargements, show in both cases a reduction of the level of agricultural protection of EU incoming countries. However, the effect is significant only for the 1995 enlargement. Moreover, we can reasonably expect the parallel-trend assumption to hold during the 2004 EU enlargement. However, during the 1986 and 1995 EU enlargements, the results based on parallel-trend tests are mixed. On the one hand, the Granger causality tests corroborate the absence of any anticipatory effects. On the other hand, the time-trend approach rejects the null hypothesis at 1% level, consistent with the view that some anticipatory effects took place before the 1986 and 1995 treatment periods. However, it is worth recalling that, the robustness exercise based on the SCM to backdate the intervention in the dataset to a period before is consistent with the lack of any anticipation effects in the treated countries.

Overall, the DID estimations present effects that go in the same direction of those estimated with the SCM. However, the DID estimations present some differences in the significance of the effect, which however are not surprising given that the DID estimate an

²³ A work by Cerulli and Ventura (2019) provides two different tests. The first test is in the spirit of Granger (1969), while the second test relies on the inclusion of a time-trend variable. In this context, the null hypothesis of the test for the parallel-trend assumption is the absence of any anticipatory effects.

average effect that is common to treated countries and that the estimated effect is non-time varying. Taking into account the caveats indicated above on the comparability of the two methodologies, we believe that the scarce ability of the DID of allowing an interpretation of the results reinforce one more time the choice of using the SCM in context of comparative case-study, as developed in the present paper.

5. Discussion and conclusions

Our study analyses to what extent EU incoming countries change their level of protection in agriculture in comparison to a domestic policy implementation scenario. In one sense, this means comparing the economic and political “efficiency” of a centralized decision-making process, with a decentralized domestic one. To make this comparison meaningfully, we exploit the properties of the synthetic control method to build “ideal” counterfactual scenarios, in a comparative case-study setting.

Our results suggest a large positive increase of the agricultural protection during the 1973 and 1986 EU enlargements. However, this effect is strongly reversed during the 1995 and 2004 enlargements, where incoming countries significantly reduced their agricultural protection level compared to a counterfactual scenario. The last result goes in the same direction with the only study that explicitly test a similar hypothesis using standard regression tools for the year 2009 (see Von Cramon-Taubadel et al., 2013). In addition, our results give some support to the idea that the standard view about the functioning of the CAP decision-making process as a sort of “restaurant table effect” (Runge and Von Witzke 1987), is actually more complex and probably linked to the evolution of the EU decision-making institutions, as argued by Pokrivcak et al. (2001). Indeed, our results show that the supranational nature of the CAP has induced a “restaurant table effect” only in the early stage of the EU development (first two enlargements), while the drastic changes imposed

by the institutional reforms of the 90s' apparently led to the delivery of more efficient policy outcomes, at least from an economic point of view.

More in general our results appear consistent with a political economy interpretation of the functioning of European institutions, particularly with the predictions of the spatial model of Crombez (2008), as well as some insights from the political economy of fiscal federalism. In this respect, our study supports the idea that institutional changes affect the EU decision-making process of the CAP. Before the Single European Act of 1987, the interpretation of a positive agricultural protection effect of the first two enlargements is twofold. On the one hand, it considers the common pool problem over the EU budget. On the other hand, it refers to a low level of heterogeneity in the farm lobby with national interests in a "small" Union. Under this setting, a centralized decision-making process tends to be more prone to the pressure group activities of the farm lobby (Tabellini and Wyplosz, 2006).

Conversely, the negative protection effect of the 1995 and 2004 enlargements to the East, appears in line with the growing complexity of the EU and the institutional changes of the 90s'. Indeed, on the one hand, by moving from 6 to 15 (and then 25) members, the heterogeneity of the EU farm lobby significantly increases, rendering lobbying activity in Brussels less effective. On the other hand, the important changes in the EU decision-making rules (UV vs. QMV rule) and the new role of the parliament in the EU institutional setting, contributed to a pro-market position shift of the Commission, and to an increase of its power in the EU decision-making process (Crombez, 2008). Thus, there are evidence that progressive changes in the heterogeneity of EU farm preferences and the new institutional setting that added constraints to the EU decision-making process, contributed to delivering more "efficient" policy outcomes.

Given this interpretation, it might be of some interest to speculate on the potential implications of our results for the future CAP reforms. Consider for example the Climate Change Action plan, which should be focused on two core objectives: *a*) adaptation policy to minimize the impact of climate change on agriculture; *b*) mitigation policy to reduce the Green House Gases (GHG) emissions related to agriculture. The former – adaptation to the impact of climate change – is extremely context specific, because the impact of climate change may shift from negative to positive when moving from the South towards the North of the EU, and it changes dramatically depending by the farming system under consideration (see Van Passel et al. 2017; Olper et al. 2021). From this perspective, any policy targeted to minimize the impact of climate change, would be more effective if thought at the country or, better, regional level. The second objective – mitigation of GHG emissions from agricultural activities and other environmental damages – should be implemented within a clear and unified framework developed at the EU level and based on the IPCC and EEA guidelines (see IPCC, 2019). However, from a certain perspective, it might be again a country-specific problem, rather than an overall EU issue, as partially recognised by the same current CAP Reform package. Thus, the EU is free to target an ambitious GHG emission reduction plan, as already decided in 2017 and reinforced recently in the so called “European Green Deal” through the Farm-to-Fork Strategy. However, how each country plans to pursue its own target, and to what extent agriculture GHG reduction will contribute to that, should be decided individually by each Member Country, rather than imposed by the EU.

Thus, whether the above challenge would be better addressed by policy decisions taken from a centralized vs a decentralized decision-making process is, a-priory, not so obvious.

Interestingly, what happened with the *Brexit*, could be eventually somehow instructive.²⁴ Indeed, as it is well known, the ‘public money for public goods’ approach to future farm support proposed by the UK Government goes significantly beyond to the Commission reform for the CAP post-2020. In a nutshell, in this specific “climate” context a decentralized decision-making process appears at the end not so bad.

Commentato [DC2]: Forse toglierei

Finally, it is also important to bear in mind the limitations of the present study. First, concerning the method used, the SCM, though it presents advantages when compared with standard regression approach, such as the DID estimator, as clearly showed in this paper, it has also some drawbacks. Indeed, the results of the SCM at the end are largely dependent by our ability to find reliable counterfactual units for comparison, and this can be problematic in a comparative study setting. Second, our paper exploits the EU enlargement episodes to study their effect on agricultural protection, as measured by NRA. However, this is an aggregated measure that focused only on economic distortions mainly related to trade policy, disregarding other dimensions of the CAP. Thus, expanding the analysis considering other relevant policy outcome variables, could be an interesting development for future research.

²⁴ See Matthew and Roederer-Rynning (2020), for an in-depth discussion of the influence of Brexit on the current CAP reform.

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Table 1: Pre-treatment fit, and balance of variables used to build the Synthetic Control

Country	RMSPE	NRA T ₀ - 10	NRA T ₀ -5	NRA T ₀	GDP per- capita	Employment Share	Polity 2	Net export share
Denmark (1973)	0.03	0.416	0.430	0.447	15,419	13%	10.0	0.512
Synthetic Denmark		0.394	0.450	0.386	12,033	16%	10.0	0.096
United Kingdom (1973)	0.09	0.730	0.470	0.426	12,572	3%	10.0	-0.888
Synthetic United Kingdom		0.558	0.540	0.332	13,400	9%	10.0	-0.066
Ireland (1973)	0.04	0.622	0.637	0.557	6,708	29%	10.0	0.405
Synthetic Ireland		0.606	0.651	0.489	9,046	21%	8.9	0.045
Spain (1986)	0.13	-0.08	-0.11	0.290	8,729	20%	5.6	0.003
Synthetic Spain		0.018	0.070	0.066	13,550	16%	5.1	0.146
Portugal (1986)	0.05	0.206	0.280	0.224	5,990	26%	7.6	-0.330
Synthetic Portugal		0.209	0.265	0.246	8,633	24%	3.1	0.205
Austria (1995)	0.07	0.440	0.683	0.628	19,250	8%	10.0	-0.279
Synthetic Austria		0.497	0.664	0.632	17,458	12%	6.3	-0.032
Finland (1995)	0.17	0.983	1.611	0.854	18,593	8%	10.0	-0.276
Synthetic Finland		1.272	1.403	1.093	13,496	28%	6.7	-0.174
Sweden (1995)	0.05	1.008	0.991	0.571	22,527	4%	10.0	-0.531
Synthetic Sweden		1.016	0.963	0.659	22,680	10%	7.5	-0.053
Czech Republic (2004)	0.04	0.150	0.180	0.254	5,489	8%	10.0	-0.167
Synthetic Czech Republic		0.170	0.191	0.195	20,421	14%	9.3	0.238
Hungary (2004)	0.05	0.161	0.179	0.247	4,537	11%	10.0	0.309
Synthetic Hungary		0.181	0.183	0.201	11,950	17%	9.3	0.543
Poland (2004)	0.03	0.096	0.175	0.176	4,166	22%	9.2	0.007
Synthetic Poland		0.117	0.159	0.172	14,855	25%	8.7	0.217
Slovakia (2004)	0.02	0.193	0.265	0.233	3,743	9%	8.3	-0.204
Synthetic Slovakia		0.218	0.241	0.235	17,509	20%	8.3	0.026
Slovenia (2004)	0.07	0.580	0.758	0.542	9,250	2%	10.0	-0.605
Synthetic Slovenia		0.674	0.672	0.558	27,813	12%	9.4	0.016

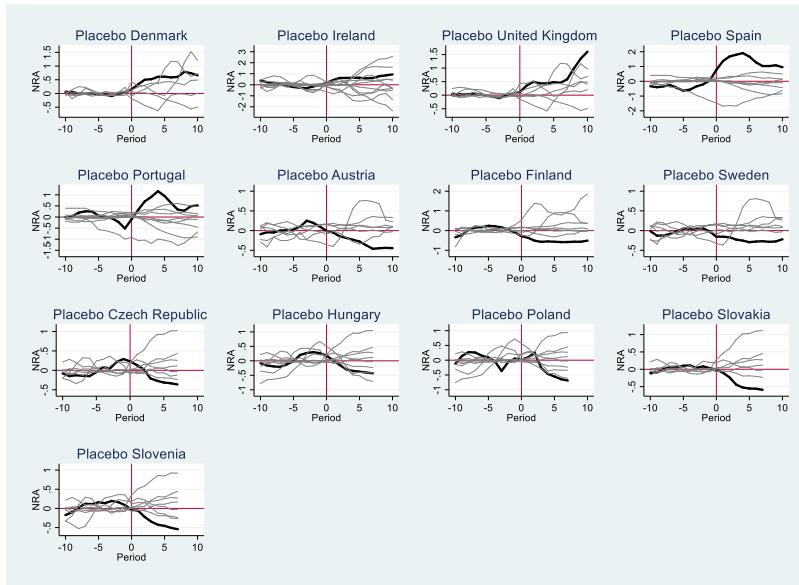
Note: See text for variables description

Table 2: Enlargement treatment effect on agricultural policy: SCM case-study results

#	Country	Year of EU Accession (T_0)	Average Treatment Effect		p-value $T_0 + 5$	p-value $T_0 + 10$	p-value Period Average
			$T_0 + 5$ (%)	$T_0 + 10$ (%)			
1	United Kingdom	1973	48.3%	160.5%	0.08	0.16	0.04
2	Spain	1986	167.1%	95.3%	0.00	0.00	0.00
3	Ireland	1973	63.6%	93.8%	0.17	0.17	0.13
4	Denmark	1973	61.3%	66.3%	0.08	0.17	0.07
5	Portugal	1986	94.9%	52.2%	0.00	0.00	0.03
6	Sweden	1995	-29.4%	-22.0%	0.00	0.00	0.02
7	Czech Republic	2004	-31.4%	-36.4%	0.00	0.00	0.11
8	Hungary	2004	-37.5%	-42.7%	0.20	0.10	0.14
9	Austria	1995	-27.7%	-44.5%	0.10	0.00	0.04
10	Finland	1995	-56.6%	-50.7%	0.00	0.00	0.00
11	Slovenia	2004	-45.6%	-54.3%	0.00	0.00	0.01
12	Slovakia	2004	-55.2%	-59.4%	0.00	0.00	0.00
13	Poland	2004	-55.1%	-68.6%	0.00	0.00	0.19

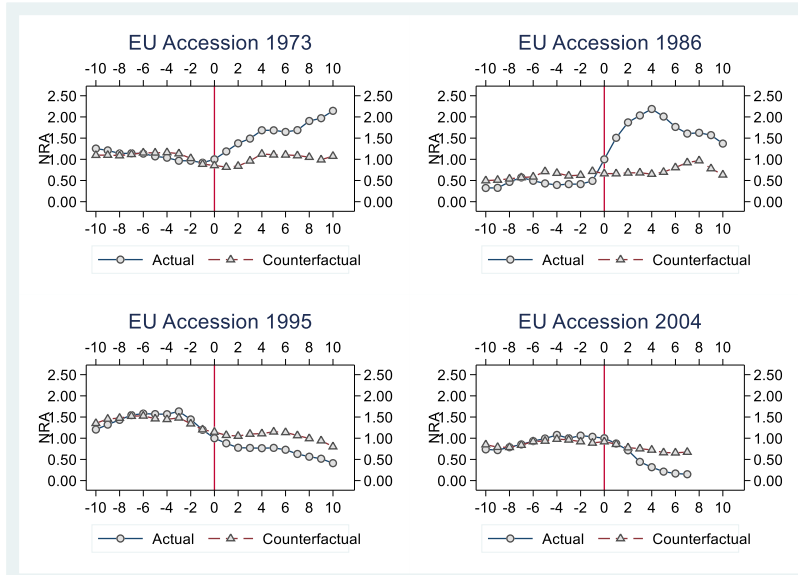
Note: The table summarizes the effect of the EU accession on countries' level of agricultural protection. Countries are ranked from the highest increase in the nominal rate of protection at T+10 to the lowest (i.e. highest reduction). The magnitude of effect of the EU accession on the evolution of the NRA is measured as the % deviation of the treated country with respect to the synthetic control. Mid-post treatment and End-post treatment periods are set, respectively, at T+5 and T+10. Due to data availability, the post treatment period for Czech Republic, Hungary, Poland, Slovakia and Slovenia ends at T+7.

Figure 1. In-space Placebo tests



Note: The figure shows for each treated country the in-space placebo test, where the treatment is assigned to countries belonging to the donor pool. The bold-black line represents the actual treated country, while the grey lines represent the fake experiments. See text for further details.

Figure 2. Enlargement treatment effect on agricultural policy: SCM results aggregated by Accession period



Note: The figure reports the dynamic average treatment effect of the EU accession on the nominal rate of assistance, when aggregating country by the timing of the EU enlargement episode.

Appendix 1:

Countries and weights of Synthtic Control for each country-case study

<p>Synthetic Denmark (1973): Japan (0.352), New Zealand (0.538), Sweden (0.109); Synthetic United Kingdom (1973): Canada (0.630), Sweden (0.370); Synthetic Ireland (1973): Finland (0.503), New Zealand (0.379), South Korea (0.118); Synthetic Spain (1986): Austria (0.283), Mexico (0.356), USA (0.361); Synthetic Portugal (1986): Japan (0.033), Mexico (0.501), Norway (0.039), New Zealand (0.427); Synthetic Austria (1995): South Korea (0.294), Mexico (0.182), USA (0.525); Synthetic Finland (1995): Switzerland (0.350), South Korea (0.103), Mexico (0.189), Turkey (0.358); Synthetic Sweden (1995): Switzerland (0.124), Mexico (0.248), Norway (0.165), USA (0.463); Synthetic Czech republic (2004): South Korea (0.047), New Zealand (0.262), Turkey (0.214), USA (0.477); Synthetic Hungary (2004): South Korea (0.066), New Zealand (0.691), Turkey (0.204), USA (0.04); Synthetic Poland (2004): Norway (0.008), New Zealand (0.205), Turkey (0.470), USA (0.316); Synthetic Slovakia (2004): South Korea (0.055), Mexico (0.201), Turkey (0.291), USA (0.453); Synthetic Slovenia (2004): Norway (0.29), Turkey (0.203), USA (0.507).</p>
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Note: The table reports for each country case studies the (untreated country) composition of the synthetic control and the relative countries' weight