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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 changes in the level of agricultural protection of the new members, in comparison with 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. The opposite holds, however, for the 1995 and 2004 enlargements, where the incomers significantly reduced their level of assistance to agriculture, in comparison with a counterfactual scenario.

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 find this out, we study to what extent EU incoming countries significantly changed their agricultural protection level, and in particular the direction of this change.

We then 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 examines a hypothetical agricultural policy scenario under a counterfactual situation wherein each Member State implements its own agricultural policy at the national level, under the hypothesis of non-EU entrance.

Our research design exploits 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. During these enlargements, the six EU 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.[1] The European Union thus moved from 6 to 25 members. For each new EU member, we compare the level of agricultural protection in the post-enlargement period (ten years) with the level of protection of its counterfactual scenario. The latter is built by 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 CAP support. However, it is worth noting that it does not include other kinds 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, because other EU policy objectives (e.g. social and environmental ones), are disregarded.

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

Our main findings show that the EU enlargement effect on agricultural policy crucially depends on the timing of the 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 as we move towards the 1995 and 2004 EU enlargements. In the latter two cases, our results show that EU entry 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 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 also contribute to the 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 and Swinnen, 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 the study.

The remainder of the paper is organized as follows. Section 2 presents an overview of the conceptual and applied works on the CAP. 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.

Background and related literature

In this section we briefly 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.[2]

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 has attracted considerable attention is the Common Agricultural Policy (CAP). This policy given its historical importance in the EU budget[3] represents an ideal domain for a better understanding of how the EU decision-making process works in practice (see De Gorter and Swinnen, 2002; Pokrivcak et al. 2006; Greer, 2017).

Within the broad class of empirical studies on the political economy of the CAP, we can distinguish two main approaches. The first refers to the informal narrative analysis of the CAP decision-making process (e.g., Senior-Nello, 1984; Runge and Von Witzke, 1986; Moyer and Josling, 1990; Swinnen, 2001). The second embeds more quantitative (mainly in 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 of EU institutions, interest groups and national interests. Historically, the EU decision-making process has been interpreted as a game played at national and supranational inter-state bargaining levels (Olper, 1998). At least before the institutional reforms of recent decades,[4] 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.

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 design of the CAP was the custom of taking 'unanimous' decisions at Council meetings,[5] which 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.[6]

Many observers at the time argued that the EU Members had an incentive to implement more expensive policies during the annual CAP prices review - encouraging higher levels of protection - because they shared the burden of the EU agricultural support costs. This phenomenon is also known as "restaurant table effect" (Pearce, 1983; Von Witzke, 1987; Pokrivcak et al. 2001).

Initially, the analysis of the CAP through a more formal econometric approach did not explicitly consider the specific economic and political interests of individual Member States. Successively, Harvey (1982), and more formally Olper (1998), considered the economic and political interests of individual Member States more explicitly, showing

that standard political economy determinants of agricultural protection (Homna and Hyami, 1986; Gardner, 1987), were important variables in explaining variation of the CAP support.

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 and Swinnen, 2008).[7] Many of these contributions have 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 into the CAP reforms. These models consider both the players' preferences and their interaction between the rules and changes in the institutional setting. One of the first papers to apply spatial models to the CAP was Pokrivcak et al. (2006), investigating how the Member States preferences on the CAP affected the EU decision-making process. Similarly, Crombez and Swinnen (2008) analyse how changes in the EU institutional setting affected the Commission's position on pro-market CAP reforms. This analysis is particularly useful for our empirical exercise because it encompasses the four EU enlargements that occurred under different institutional settings. Because of the Single European Act of 1987, the Commission increased the set of policies toward qualified majority voting (QMV), rather than unanimous voting (UV). 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's position toward pro-market CAP reform.[8]

The analysis of Crombez and Swinnen (2008) is consistent with the view that both institutional reforms and other external constraints, such as the WTO agreement of 1994 (Pokrivcak et al. 2006), played a role in explaining the deviation from the status-quo bias of both the McSharry (1993) and the Fischler (2003) reforms of the CAP. Broadly speaking, our results reported below are not in conflict with this interpretation.

European Added Value, Fiscal federalism, and the CAP

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 national level (see Heinemann et al. 2013). 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 current 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 fiscal federalism is that, since many public policies have cross-border externalities (spill-over) and scale economies, 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. First, the concept of "race to the top" in public spending by national governments, which engage in a sort of competition on subsidies that leads to a waste of resources (Janeba, 1998). In addition, national governments tend to be prone to (short) electoral cycles, which partially preserve policy-making at EU level. Similarly, it is argued policy decisions at the EU level can be less affected by the pressure from lobbying (Alesina et al. 2005a).

However, there are also counterarguments to the above logic. For example, Vaubel (1999) finds that the effectiveness of lobbying on decision-making is lower in a decentralized system. 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 cave into pressure from lobbies. This is consistent with early stages of the CAP decision-making process. Indeed, when there were few Member States (from EU6 to EU9 until 1981) the farm lobbies were less heterogeneous, a situation that changed progressively as an effect of EU enlargements.

To the best of our knowledge, Von Cramon-Taubadel et al. (2013) is the only study that has formally tested to what extent the CAP produces EAV.[9] These authors use standard regression tools applied to a sample of (non-EU) OECD countries to build the counterfactual scenario. This regression analysis has been used to investigate how EU countries would have behaved under a 'decentralized' decision-making hypothesis. Interestingly, they find that in the recent period (2009), on average, the CAP *caps* the EU expenditure for agricultural policy (in comparison with 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 expenditure (see Heinemann et al. 2013).[10]

The study of Von Cramon-Taubadel et al. (2013) also has limitations as recognized by the authors themselves. First, the empirical results and the policy recommendations are not robust to changes in the choice of countries used to build the counterfactual.[11] 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 when investigating the impact of EU incoming members on agricultural protection (Heinemann et al. 2013). This is what we propose in the next sections.

Methodology

The Synthetic Control Method

The SCM, which is widely used in applied economics evaluation studies, was first proposed by Abadie and Gardeazabal (2003), then further refined by Abadie et al. (2010, 2015). It was originally conceived to estimate the effect of an aggregate intervention on a given outcome of interest in a comparative case study. Aggregate interventions are events that apply at an aggregate level, which nevertheless 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).[12]

The widely used regression analyses applied to estimate the effect of interventions in social science studies when dealing with large-scale rare effects often rely on time series or comparative case studies. The former are particularly valuable when considering interventions that are expected to have high-magnitude effects in the short-term (Abadie, 2021). The main problem with these methodologies is that they are not suitable for estimating long-run effects, as other shocks may also affect the result. Comparative case studies, on the other hand, where the effect of an intervention is estimated by considering the difference in the variable of interest of treated and untreated units, are widely 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 there is only a small number of available units, 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 based on a number of salient characteristics. The synthetic control is constructed by considering the pre-treatment period. In our analysis, therefore, we consider, for each country, the years *before* joining the EU. Then, by comparing the trend in the outcome variable (in our case the level of 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 the analysis we seek to establish whether the dynamic of the level of protection induced by the CAP in the treated country is significantly different from its counterfactual situation.

We believe that the properties of the SCM fit our research question better than those of other estimation methodologies, such as linear regression models. This is primarily due to the capacity of the SCM to deal with case studies where the number of observed units is limited. In the Additional Material Section A, after presenting a formal presentation of the SCM, we describe more in details the main advantages of the SCM over other more standard methodologies.

Our particular interest is in the "average" enlargement effects over time: that is to say, to what extent, on average, the four enlargements of the EU display significantly different treatment effects. This is important because the four enlargements occurred under quite different institutional settings. Thus, it may be instructive to measure the average treatment effects aggregated by the enlargement. In so doing, we follow Cavallo et al. (2013) and Olper et al. (2018). In the Additional Material Section A, we present in detail how these average effects are computed as well as how the significance of the effects is inferred.

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 occurring in the years 1973, 1986, 1995 and 2004.[13] Hence, the year of accession indicates our treatment variable.

The level of agricultural protection, which represents our outcome variable (Y_{it}), is measured by means of the Nominal Rate of Assistance to agriculture (NRA) from the 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. producer subsidy equivalent), is crucial for our research design, as it allows us to measure the level of protection in agriculture going back to 1963, hence ten years before the first EU enlargement in 1973. Only the availability of data over such a long period can allow us to base our econometric approach on the SCM.[14]

We use a vector of covariates to build the synthetic controls. This vector is based on previous cross-country studies of 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 the share of agricultural employment, 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 the fit of agricultural protection of the treated and control group in the pre-treatment period, we use 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 used to build each synthetic control, is represented by OECD countries. We decided to focus on this sample, as OECD countries are more likely to share a similar level of development as the EU, rather than less developed countries. Note moreover that countries successively joining the EU in one of the later enlargements can be part of the donor pool, if they satisfy the condition that in a specific country-experiment they are not yet EU member at the time of the pre-treatment and post-treatment period. For each country-case study, we set the pre-treatment period as 10 years prior to the treatment. Analogously, the post-treatment is set 10 years after accession.[16]

It is worth noting that a peculiar characteristic of our donor pools is the lack of countries located at the border with the treated units. As a result, the stable unit treatment value assumption (SUTVA) should 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 effects with close neighbours. However, the structure of our database allows us to discard the possibility of such effects.

Results

This section summarizes the results obtained from our 13 SCM experiments, in which we explore the effect of different EU enlargements on the agricultural protection of new members, measured as *NRA*. We first present the results of our analysis by country, looking at the significance and direction of the effects, as well as to the quality of the

matching. Next, we present the results by aggregating the estimated effects by EU enlargements.

Quality of the Synthetic Controls and Country level results

One of the main advantages of the SCM is that it allows us to assess 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.

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 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 shows that all country case studies, with 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 to the treated countries in the pre-treatment period.

On a more formal level, 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 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 it higher than 0.10, though still quite low.

In brief, the preliminary assessment based on the criterion of the RMSPE is encouraging and supports the view that the SCM experiments are reliable and sufficiently 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 period considered. Overall, the results provide a clear picture.[17]

On the one hand, the countries at the top of Table 2, namely those that joined the EU during the first two enlargements 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 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, that joined the EU in 1986, show a marked 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, albeit still positive, declined for both Spain and Portugal at time T_0+10.

On the other hand, all the 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 amounts to −68.6% and −59.1%, respectively. All these countries show a similar path in the evolution of the nominal rate of protection compared to their counterfactuals, with a marked reduction in the short-run, and a continued decline thereafter. However, it is important to note that, due to the lack of data from 2012 onward, the last post-treatment year for countries joining in 2004 is set at the year 2011, namely 7 years after the treatment.

Turning our attention to the significance of the 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 Additional Section A from their placebo test) lower or equal to 0.10, which then we may consider as statistically significant. As explained in the Additional Material Section A, the *p-value* is computed for each country case study starting from placebo tests, which are fake experiments where the treatment (in our case the EU accession) is assigned to all the countries in the donor pool. In brief, the treatment effect is considered significant (i.e. *p-value*<0.10) if the trajectory of the actual treated country outperforms the one of most (or all) the placebo tests (see Additional Section A for a detailed and more formal explanation). 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 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. Visual inspection tends to confirm the results in Table 2, and specifically with 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 period of the majority of the grey lines of the 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 compared to the counterfactual, and the third and four enlargements where, instead, the *NRA* of the treated countries substantially decreases compared to the counterfactual synthetic control.

Aggregated results across enlargement episodes

We further test the robustness of these findings at the country-level by aggregating the estimated effects within each EU enlargement. Thus, this analysis allows us to test to what extent the timing of the enlargement matters with regard to 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 Additional Material Section A.

Overall, the dynamics of the ATE are qualitatively similar to those from the single-country case studies, though now the level of significance is higher. This is simply because the aggregation process leads to a substantial increase in the degree of freedom to measure the placebo test through permutations (see equations 6 and 7 in Additional Material Section A). 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-value* = 0.04) and the long-run at T_0+10 (*p-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 1995 EU enlargement 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-value* < 0.01) and T_0+10 (*p-value* = 0.04). Finally, the effect of the 2004 EU enlargement on agricultural protection is negative and statistically significant with a *p-value* < 0.01, in both periods T_0+5 and T_0+7.

To sum up, 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 effects of EU enlargement on the level of protection in agriculture turn out to be negative and statistically significant, during the 1995 and 2004 EU enlargements.

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 us to test the main results obtained through the SCM with a difference-in-difference estimator.

As is common with many research designs 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 likely existence of an anticipation effect of the policy is due to the obvious reason that the accession process is long and gradual and may last for many years. According to the EU legal treaty texts, long 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 an anticipatory effect of the policy, the SCM allows us to backdate the intervention to a prior period, so that our estimation can capture the full extent of the treatment effect. We ran, therefore, the analysis country by country backdating the treatment period three years in advance and then aggregated the results by enlargement episode.[21]

The results of this test are presented in Additional Material Section B, Figure B.1. The graph for the average effect of the 1973 enlargement, where 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 larger at the time of the (anticipated) treatment (i.e. 1983), and became more evident after T+2 (i.e. 1985). However, the results of this robustness check should be treated with caution, due to the poor fit of countries involved and synthetic control in the pre-treatment.

Finally, the results of this test for the final two enlargements (i.e. 1995 and 2004) clearly exclude any anticipation effect. The average level of agricultural protection declines 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 fail to suggest that possible anticipation effects due to pre-accession policy are driving our results. This conclusion holds true for all the investigated enlargments, apart from 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 this, we follow Cerulli and Ventura (2019), by using a difference-in-differences (DID) estimator that can be applied to the case of binary time-varying treatments with pre- and post-intervention periods. The methodology used and the discussion of the results are reported in the Additional Material Section C. Overall, the DID estimations present effects in line with those estimated with the SCM. However, the DID estimations present some differences in the significance of the effect, which is hardly surprising given that the DID estimate an average effect that is common to treated countries and that the estimated effect is non-time varying.

Discussion and conclusions

Our study analyses to what extent incoming countries to the EU 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 meaningful, 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 in agricultural protection during the 1973 and 1986 EU enlargements. However, this effect is reversed during the 1995 and 2004 enlargements, where incoming countries significantly reduced their level of agricultural protection compared to a counterfactual scenario. The last result goes in the same

direction as the only study that explicitly tests a similar hypothesis using standard regression tools for the year 2009 (see Von Cramon-Taubadel et al., 2013). In addition, our results provide some support for the idea that the standard view on 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 induced a "restaurant table effect" only in the early stage of 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 and Swinnen (2008), as well as the insights from the political economy of fiscal federalism. In this respect, our study supports the idea that institutional changes affect the CAP decision-making process. 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 from 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 to15 (and then 25) members, the heterogeneity of the EU farm lobby significantly increases, rendering lobbying 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 the pro-market shift of the Commission, and to an increase of its power in the EU decision-making process (Crombez and Swinnen, 2008). Thus, there is considerable 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 future CAP reforms. Consider for example the Climate Change Action plan, which should be focused on two core objectives: *a*) adaptation to minimize the impact of climate change on agriculture; *b*) mitigation 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 on the farming system under consideration (see Van Passel et al. 2017; Olper et al. 2021). From this perspective, any policy targeted at minimizing the impact of climate change, would be more effective if considered at the country or, better still, 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 another perspective, it might again be a country-specific problem, rather than an overall EU one, as partially recognised by the current CAP Reform package. Thus, the EU is free to target an ambitious GHG emission reduction plan, as 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 agricultural 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 not so obvious *a priori*. Interestingly, the case of Brexit, could turn out to be instructive.[22] Indeed, as 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's reform for the CAP post-2020. In a nutshell, in this specific "climate" context a decentralized decision-making process appears at the end of the day not to be so bad.

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 compared with standard regression approach, as clearly shown by the DID estimator in this paper, it has also some drawbacks. Indeed, the results of the SCM are heavily dependent on 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 focused only on economic distortions mainly related to trade policy, disregarding other dimensions of the CAP. Thus, expanding the analysis to consider other relevant policy outcome variables, could be an interesting avenue for future research.

1 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

[2] 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.

[3] 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).

[4] 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 and Swinnen (2008) for an in-depth discussion of these institutional reforms and their implications for the CAP decision-making process.

[5] 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.

[6] 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.

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

8 This clearly holds under the assumption that the European Parliament is more reform minded that the pivotal countries (see Crombez and Swinnen, 2008, for details).

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

10 Though this statement probably encountered the agreement of the majority of economists, to properly evaluate to what extent farm money is 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.

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

12 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).

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

14 Note that OECD agricultural support data start in 1986, thus preventing us from estimating 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.

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.

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, Eastern European 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).

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 us 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.

18 Note, for 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 form 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.

20 We thank an anonymous referee for bringing 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.

22 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

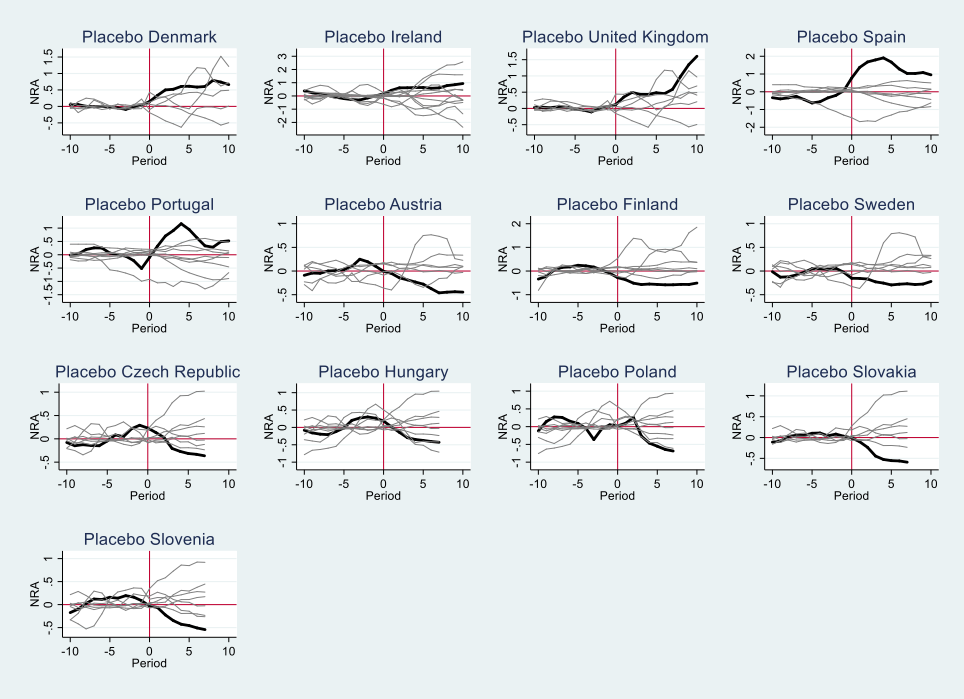
Note: See text for variables description

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

#	**Country**	Year of EU Accession (***T₀***)	*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

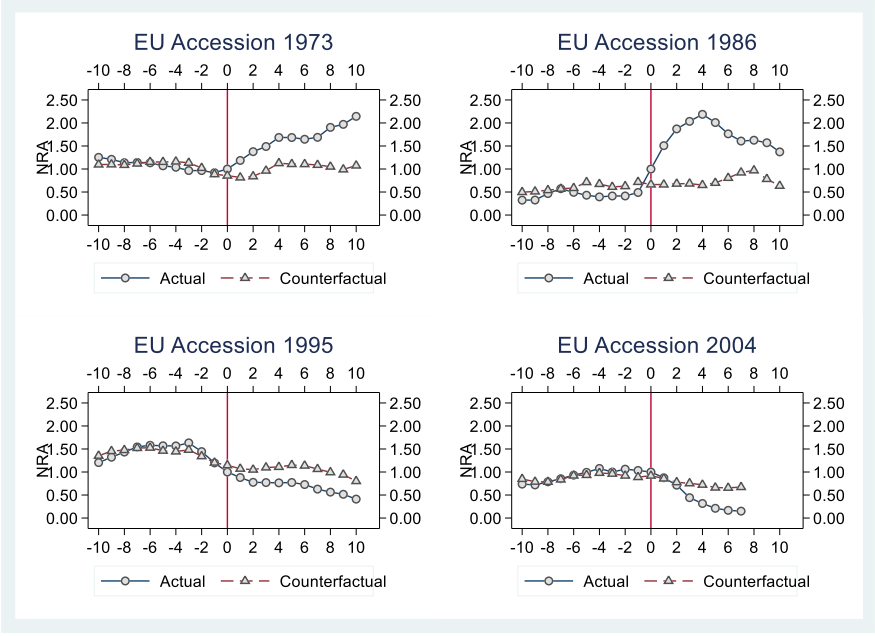
Note: The table summarizes the effect of the EU accession on countries' level of agricultural protection. Countries are ranked from the highest increase in the nominal rate of protection at T+10 to the lowest (i.e. highest reduction). The magnitude of effect of the EU accession on the evolution of the 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**

Treated country	**Countries in the Synthetic Control**
Denmark (1973)	Japan (0.352), New Zealand (0.538), Sweden (0.109)
United Kingdom (1973)	Canada (0.630), Sweden (0.370);
Ireland (1973)	Finland (0.503), New Zealand (0.379), South Korea (0.118)
Spain (1986)	Austria (0.283), Mexico (0.356), USA (0.361)
Portugal (1986)	Japan (0.033), Mexico (0.501), Norway (0.039), New Zealand (0.427)
Austria (1995)	South Korea (0.294), Mexico (0.182), USA (0.525)
Finland (1995)	Switzerland (0.350), South Korea (0.103), Mexico (0.189), Turkey (0.358)
Sweden (1995)	Switzerland (0.124), Mexico (0.248), Norway (0.165), USA (0.463)
Czech republic (2004)	South Korea (0.047), New Zealand (0.262), Turkey (0.214), USA (0.477)
Hungary (2004)	South Korea (0.066), New Zealand (0.691), Turkey (0.204), USA (0.04)
Poland (2004)	Norway (0.008), New Zealand (0.205), Turkey (0.470), USA (0.316)
Slovakia (2004)	South Korea (0.055), Mexico (0.201), Turkey (0.291), USA (0.453)
Slovenia (2004)	Norway (0.29), Turkey (0.203), USA (0.507)

Note: The table reports for each country case studies (Treated countries) the composition of the synthetic control and the relative countries' weight in parenthesis.