The effect of labour market access on asylum-migration flows; quasi-experimental evidence from Ireland

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Master Thesis

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Abstract

Recent crises have brought asylum-migration to the forefront of political debate in Europe. There has been significant discourse in academic and policymaking spheres concerning migratory pull factors; in particular whether prospective socio-economic entitlements in destination countries are determinants of asylum applicants' destination choice. Employment rights feature prominently in these discourses, yet few studies directly examine the relationship between labour market access and asylum inflows. This paper exploits Ireland's transposition of the Recast Reception Conditions Directive in 2018, which ended a comprehensive ban on asylum applicants' access to the labour market, in order to study the effects of labour market access on the magnitude and demographic composition of asylum applications. Using difference in difference and regression discontinuity methodologies, this paper finds that the labour market access reform caused an increase in the number of asylum applications and in the proportion of working-age applicants in Ireland. These findings contribute to a small body of quasi-experimental literature on the determinants of asylum inflows to destination countries.

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1 Introduction

Since 2014, 2.5 million people have crossed the borders of the European Union (EU) through irregular means and 28 thousand people have died while attempting to do so (European Council, 2023). In the same period, more than 6.8 million people have applied for asylum in EU member states (Eurostat, 2023a). This recent influx, variously referred to as the "refugee crisis", "migration crisis" or "asylum crisis", has divided member states and exposed asylum policy deficiencies at the European level (Hatton, 2017). The influx has also provoked polarised debate at an intranational level and been linked to the rise of far-right parties across the EU (Steinmayr, 2017). State policy responses have largely been aimed at reducing or diverting inward migration, in contrast to distributional objectives at EU level, resulting in the development of restrictive regimes in many countries (Niemann & Zaun, 2018; Andersson & Jutvik, 2023). In addition to heightened border controls, countries have also sought to reduce their attractiveness as a destination for those seeking asylum by other means (Crawley & Hagen-Zanker, 2019; Mayblin, 2016). Specifically, countries have limited the socioeconomic rights of asylum applicants with respect to welfare entitlements, social services access, accommodation conditions and labour market access (Breidahl, 2022; Diop-Christensen & Diop, 2022).

Underpinning restrictive policy is the idea of a 'magnet effect', whereby comparatively generous social entitlements and/or economic rights in destination countries are theorised to result in increased migrant inflows from poorer countries (Mayblin, 2016; Larsen, 2022). This idea largely derives from the broader 'push-pull' model of migration; a contested theory which has become increasingly central to European policy narratives over the past decade (Garelli & Tazzioli, 2021). Despite this prominence in policymaking, evidence for a causal relationship between prospective socioeconomic rights and migrant destination choice is limited. Numerous quantitative studies have examined the effect of socio-economic entitlements (including, but not limited to labour market access) on asylum flows, with mixed findings (Thielemann, 2003; Neumayer, 2004; Hatton, 2009; Beenstock et al., 2015; Razin & Wahba, 2015a; Hatton, 2016; Brekke et al., 2017; Agersnap et al., 2020; Kang, 2021; Dellinger & Huber, 2021a; Diop-Christensen & Diop, 2022; Di Iasio & Wahba, 2023; Brekke et al., 2023).

While the results of some scholars (Neumayer, 2004; Hatton, 2009; Diop-Christensen & Diop, 2022; Di Iasio & Wahba, 2023) suggest that socioeconomic entitlements are largely insignificant with respect to determining asylum flows, others (Beenstock et al., 2015; Razin & Wahba, 2015a; Brekke et al., 2017; Kang, 2021) have found evidence of positive correlation. In general, the effects of individual economic or social rights and entitlements are rarely isolated in the literature (Diop-Christensen & Diop, 2022). Most studies use indexes that condense many different socioeconomic entitlements and related policies into composite variables, meaning that the effects of specific factors like labour market access on destination choice are difficult to gauge (Diop-Christensen & Diop, 2022; Hatton, 2016). Spillover effects, heterogeneity in effects between countries and the importance of country-specific factors are recurring themes, introducing further hurdles for causal inference (Keogh, 2013; D. D. Toshkov, 2014). Additionally, rapid changes in critical asylum-related information flows over the past two decades have negative implications for the general comparability of results and the development of consensus within the literature (Frouws et al., 2016).

This paper adds to the existing literature by employing quasi-experimental methods to investigate the relationship between labour market access and asylum flows. It does so by exploiting a specific instance of constitutional change and subsequent legislative reform in Ireland in 2018, culminating in Recast Reception Conditions Directive ,wherein the country ended its previously comprehensive prohibition on labour market access for asylum seekers. Unlike most literature on the topic, this paper eschews a broader international comparative model and focuses on the specific effects of labour market access policy change in one country. It is hoped that this approach may help to shed some additional light on the causal effects that are at play. The primary research question addressed by this paper is as follows:

> To what extent does labour market access influence the number and demographic composition of asylum applications in a destination country?

The empirical question posed by this paper has significance for policymakers as well as scholars. Asylum applicants remain explicitly or substantively excluded from the conventional labour market in many countries. Most countries where access is permitted (including Ireland) still subject applicants to lengthy waiting times, restrictions on working duration, and various purposive administrative barriers (Waite, 2017; Breidahl, 2022). These measures contemplate significant costs in terms of human capital, not to mention implications for the mental health and overall wellbeing of asylum applicants (Crumlish & Bracken, 2011; Brell et al., 2020). This paper aims to provide policymakers with insight into part of the evidentiary basis for such restrictions.

This paper relies on country-specific data on the number of first time asylum applications, acceptance-rate, age and sex of asylum applicants, drawn from the Eurostat migr_asy dataset (Eurostat, 2023a). The data covers a period from January 2008 to August 2023 at both monthly and quarterly intervals. Additionally, the paper employs OECD monthly unemployment and GDP data as covariates (OECD, 2023). Both Regression Discontinuity (RD) and Difference in Difference (DiD) specifications are employed to measure the causal effect in question. The results of this paper indicate that the reform had a substantial positive effect on the number of asylum applications in Ireland and also increased the proportion of working-age asylum applicants in Ireland.

The next chapter sets out the background and substantive content of the policy experiment that underlies the paper. Chapter 3 provides a brief overview of the theory and literature on the determinants of asylum-related migration. Chapter 4 describes the dataset used and includes descriptive statistics. Chapter 5 explains the empirical methodologies employed by the paper in detail and specifies the models used. Chapter 6 comprises the main results of the paper along with robustness checks. Finally, Chapter 7 consists of discussion and contextualisation of the empirical findings, along with their implications for theory and policy.

2 Asylum Policy and Labour Market Access in Ireland

2.1 Policy Experiment

The policy change underpinning the quasi-experimental approach of this paper is Ireland's transposition of the recast Reception Conditions Directive in July 2018. This legislative change gave asylum-seekers who had been waiting for a decision on the outcome of their application for a period of 9 months or more the right to apply for a work permit and thus enter the labour market. Prior to this period of reform, an absolute prohibition on work was in place. Asylum-seekers could not look for or enter employment before a final determination on their entitlement to international protection was made by the State.

2.2 Background

Asylum-related immigration was virtually non-existent in Ireland up until the mid-1990s (Mac Éinrí & White, 2008). The beginning of the 21st century coincided with a sharp increase in asylum applicants that exposed an antiquated and deficient Irish asylum system. The government policy response was an overhaul of the existing system aimed at reducing or deflecting incoming migration (McCormack-George, 2019). The Direct Provision and Dispersal system (DPD), instituted in April 2000, was designed to meet minimum obligations under international law while remaining comparatively less "attractive" than neighbouring regimes in the United Kingdom and mainland Europe (Loyal & Quilley, 2016). Applicants received accommodation and a small allowance of €19 per week, but had limited mobility and no right to seek employment while being processed. This system remained largely unchanged for the better part of two decades. By 2016, Ireland and Denmark were the only European countries remaining that did not provide asylum seekers the right to work after a specified period of time (Loyal & Quilley, 2016). The average length of stay in the DPD system at that time was 32 months (Pollak, 2018a).

2.3 Reform Period

The period of reform commenced in late May 2017, following the outcome of a constitutional case in the Irish Supreme Court. In N.V.H v Minister for Justice (2017) the Court found that asylum applicants had a constitutional right to seek employment. However, rather than immediately declaring the existing prohibition on employment to be unconstitutional, the Court gave the state a period of six months to amend the existing legislation (McCormack-George, 2019). In November 2017, the Irish government announced that it would opt in to the Recast Reception Conditions Directive, without providing a specific timeline for implementation (Department of Justice, 2018).

In February 2018 interim arrangements were instituted in order to comply with the judgment of the Court. Under the interim scheme, asylum seekers were required to pay €1000 to apply for a permit; they were required to find a job with an annual salary of over €30,000; their employer was required to demonstrate that they were unable to find an Irish or EU citizen to fill the position; they remained barred from over 60 sectors, "including hospitality, healthcare, social work, childcare, general care services, marketing, sales, administration, textiles, printing, housekeeping, food and construction"(Pollak, 2018b). Only one application to work was made (and subsequently withdrawn) over the entire duration of the 5 month scheme (Work Permits Eligibility Dáil Éireann Debate, Thursday - 14 June 2018).

2.4 Labour Market Access in Ireland

On the 6th of July 2018, the European Communities (Reception Conditions) Regulations 2018 entered the statute book. Under this legislation, asylum applicants who had been waiting for a first instance decision for 9 months or more could apply for a work permit and enter the labour market . The new system saw immediate engagement from asylum applicants. Over 500 work permits, corresponding to approximately 10 percent of individuals in the DPD system at the time, were granted within 6 weeks of implementation (Bardon, 2018). Importantly from the perspective of causal inference, the wider reception structure remained largely unchanged (Hamilton & Hennigan, 2018). Aside from labour market access, the regulation included a limited number of legislative guarantees as to reception conditions that previously had no explicit basis in Irish law; however, efforts to implement these guarantees have thus far been limited (Hamilton & Hennigan, 2018; Irish Refugee Council, 2023b).

Available evidence suggests that there has been significant utilisation of labour market access permissions in the intervening period (Polakowski & Cunniffe, 2023). The Irish government received 15,136 work applications between 30 June 2018 and 1 January 2023, of which 2731 were refused and 12,181 were granted (Irish Refugee Council, 2023a). This is substantial given that the average number of persons subject to applications being processed at any one time was 7650 in the same period and that the average waiting time for a first instance decision was nearly 2 years in 2022 (Eurostat; Polakowski & Cunniffe, 2023). Labour market integration has not been seamless and asylum seekers attempting to seek employment in Ireland still face a number of administrative, financial and socio-cultural hurdles (Irish Refugee Council, 2023a). Nevertheless, the significant number of applications and work permits granted indicate that access to the labour market is broadly attainable for asylum applicants.

3 Theory and Literature Review

This chapter will provide an overview of the theory and literature concerning the determinants of asylum-related migration flows. The first part of the chapter will be devoted to describing the major theoretical perspectives in the field, specifically focusing on the ‘push-pull’ framework. The latter portion of the chapter will focus on evaluating the body of evidence stemming from existing quantitative studies on the topic, including those pertaining to the effects of labour market access on asylum-related migration. The chapter will culminate with the formation of the primary hypothesis of the paper.

3.1 Push-Pull Framework

In the context of asylum-related migration, labour market access is often referred to as a potential ‘pull factor’ in both political-policy discourse and scholarly literature (Mayblin, 2016). This terminology derives from the push-pull model of migration, which explains migration as a function of “disparities in conditions between place of origin and place of destination” (Van Hear et al., 2018). Broadly speaking, push factors refer to place of origin characteristics that drive outward migration, while pull factors describe destination characteristics that attract inward migration.

Push-pull theory has venerable origins in the work of 19th century scholar Ernst Georg Ravenstein (EASO, 2016). Its longevity in the field of migration studies was solidified by Lee (1966), who developed a framework of four explanatory factors underlying the process of migration (Lee, 1966; EASO, 2016). These comprised conditions in area of origin (or push factors), conditions in area of destination (or pull factors), ‘intervening barriers’, and ‘personal factors’(Lee, 1966). The logic of spatial disequilibrium explicated by Lee would inform many strands of later migration scholarship; the best known of which would ground push-pull theory within a neoclassical rationale (Haas, 2011). Dominant applications of push-pull theory conceptualised migrants as rational utility-maximising actors, with a particular emphasis on income differentials and labour supply (Kang, 2021; Zimmermann, 1996). Within this frame, prospective migrants compare the price of displacement against the value associated with differential conditions (i.e., the utility associated with successfully migrating) and choose to migrate based on the resultant utility maximising cost-benefit analysis. This focus on macro-economic disparities naturally minimised the importance of non-structural factors outlined by Lee (1966). Dynamic contextual components, intervening barriers and personal factors, are largely subsumed as static costs or randomness within the neoclassical paradigm; or simply reconceptualised as structural origin or destination push-pull factors (Haas, 2011).

It follows that the limitations of push-pull models are numerous and well documented in the literature. Conventional push-pull models struggle to account for individual agency, characteristics and subjective drive (Garelli & Tazzioli, 2021; Van Hear et al., 2018). They cannot adequately incorporate factors relating to volition, a particularly important consideration in the context of asylum-oriented migration (EASO, 2016; Van Hear et al., 2018). The theory's emphasis on differential conditions and structural factors often neglects the importance of network effects and family ties, which have been consistently highlighted as vital determinants of migrant destination in both qualitative and quantitative studies (Brekke et al., 2023; Di Iasio & Wahba, 2023; Diop-Christensen & Diop, 2022; EASO, 2016; Van Hear et al., 2018). Relatedly, push-pull theory is largely blind to the reality of the imperfect information and network related distortions that asylum seekers experience (Thielemann, 2003; Frouws et al., 2016; Crawley & Hagen-Zanker, 2019). Nevertheless, the language of push and pull remains ubiquitous in the literature. Despite widespread contestation of the underlying push-pull framework, the intuitive appeal of the push-pull dichotomy is such that scholars have tended towards the expansion of the model (or at least, linguistic framework) to include social, cultural and network drivers- rather than its rejection or replacement (EASO, 2016).

Although push-pull theory does not provide a comprehensive description of migration drivers, it retains instrumental value as a descriptor of structural determinants- not to mention as an practical framework for categorising causal factors (Zimmermann, 1996). Network effects, family-ties or cultural-linguistic ties, for instance, are often casually conceptualised as pull factors, regardless of their fit with the foundational notion of spatial disequilibrium (Hatton, 2020; Brekke et al., 2023). In this respect, push-pull theory constitutes an umbrella structure for largely disjointed body of scholarship examining migration based on utility maximising models. The conception of asylum seekers as rational utility-maximisers remains the standard in quantitative literature; as such push and pull continue to endure as the most common linguistic devices used in relation to the causal factors affecting asylum-oriented migration.

3.2 Determinants of asylum-applicant destination in industrialised countries

While there is a significant body of literature examining the determinants of asylum flows and destination choice based on utility-optimising models, there is little consensus as to the major pull factors affecting asylum-applicant destinations. This is partially because scholars have consistently found push factors to be the most significant determinants of the magnitude of flows, complicating the task of causal identification based on destination characteristics (Van Hear et al., 2018). Nevertheless, a number of factors thought to affect the distribution of asylum applicants between countries have emerged within the literature.

Studies generally find modest link between GDP measures and flows, indicating that asylum seekers are somewhat sensitive to economic conditions in destination countries (Keogh, 2013; D. D. Toshkov, 2014; Neumayer, 2004; Kang, 2021). At the same time, a number of studies have found that asylum flows are either not significantly correlated or negatively correlated with GDP growth (Thielemann, 2003; Neumayer, 2004; Beenstock et al., 2015). As such, it may be that general levels of prosperity (or perceptions thereof) matter more than contemporary economic trends (Neumayer, 2004). Results concerning employment level variables are generally consistent with this line of reasoning, with scholars reporting small or statistically insignificant effects on asylum flows (Beenstock et al., 2015; Hatton, 2016; Kang, 2021); although Thielmann (2003) does report a more substantial association. Like conventional migrants, asylum seekers are sensitive to distance, spatial gravity and linguistic factors (Thielemann, 2003; Beenstock et al., 2015; Di Iasio & Wahba, 2023). Results concerning the specific effects of colonial ties vary in significance between studies, possibly due to time effects (Kang, 2021).

Studies have consistently found that existing asylum migrant stocks are strongly positively correlated with asylum flows (Neumayer, 2004; Hatton, 2009; Di Iasio & Wahba, 2023); indeed Hatton (2020) has argued that "the most powerful single variable influencing asylum-seeker flows to a country is the stock of previous migrants from the same origin stocks"(Hatton, 2020). Relatedly, family reunification policy has also been identified as a strong determinant factor in the literature (Diop-Christensen & Diop, 2022; Brekke et al., 2023). These results highlight the importance of network effects and align with the findings of qualitative studies, such as that by McAuliffe & Jayasuriya (2016). However, as pointed out by Di Iasio and Wahba (2023), such determinants are necessarily dependent on prior structural factors.

In addition to family reunification policy, a significant body of literature examines how more restrictive asylum regimes affect migrant flows. A number of studies have examined the responsiveness of asylum applications to acceptance rates and repatriation risk, generally finding that low acceptance rates and/or high repatriation risks are associated with fewer asylum applicants (Keogh, 2013; D. D. Toshkov, 2014; Bertoli et al., 2022). Toshkov (2014) additionally finds evidence that asylum application numbers exert downward pressure on acceptance rates. Hatton (2009) investigates the broader relationship between asylum-oriented policy and asylum applications by constructing a policy index including elements such as border policy, process restrictions and access restrictions. They show that more restrictive policy is associated with a moderate deterrent effect on applications (Hatton, 2009). Andersson and Jutvik (2023) also provide quasi-experimental evidence that the liberalisation of asylum policy has a positive effect on inflows, in line with previous observational studies.

An important strand of the literature on policy effects, perhaps most relevant from the perspective of this paper, is concerned with the effects of welfare policy on asylum inflows and the 'welfare magnet hypothesis'. The welfare magnet hypothesis builds on the utility maximising model and notion of differential attraction contained within the broader push-pull framework (Larsen, 2022). The hypothesis suggests that, as rational utility-maximisers, migrants will be attracted to destinations with generous social welfare benefits over those with less progressive systems (Larsen, 2022). As discussed in the introduction, this "common-sense assumption" has been a significant feature of the political and policymaking discourse on asylum policy over the past decades (Mayblin, 2016; Di Iasio & Wahba, 2023).

No strong consensus on the welfare magnet hypothesis has emerged within the literature. A number of studies have reported findings broadly inconsistent with the hypothesis. For instance, an important early study by Neumayer (2004) suggests no statistically significant link between asylum destinations and total welfare spending as a share of GDP (Neumayer, 2004). Hatton (2009) finds similar results in relation to a constructed welfare policy indicator, in contrast to their findings regarding access and process restrictions. More recently, Diop-Christensen & Diop (2022) address the welfare magnet hypothesis in a study using SAMIP and UNCHR data. They again find no statistically significant link between asylum-flows and levels of social assistance, defined as the average minimum income protection derived from cash benefits, accommodation allowances, tax credits and other benefits (Diop-Christensen & Diop, 2022).

On the other hand, there are many scholars who have found evidence consistent with the idea of a ‘magnet’. Razin & Wahba (2015) find that more generous welfare regimes are correlated with downward shifts in migrant skill composition, although their results are non-specific to asylum seekers. Kang (2021) reports a strong correlation between asylum applications and social spending (as a proportion of GDP) based on a spatial gravity model, in contrast to Neumayer (2004). While Beenstock et al. (2015) find no relationship between the overall levels of welfare generosity and asylum application rates, they do find that application levels vary with changes in generosity:

> ‘This means that more generous countries in terms of welfare benevolence do not necessarily attract more immigration. On the other hand, if a given country becomes more benevolent it attracts more immigration, and when it becomes less benevolent it deters immigration (ibid, p. 27).’

In a related vein, Brekke et al. (2017) test the relationship between welfare policy and asylum flows using a composite welfare policy indicator and find that a tightening of welfare policy is associated with a substantial deterrent effect on applications. The results of Hatton (2016) and Di Iasio & Wahba (2023), who also employ composite welfare policy indicators, fall somewhere in between the lines. They report associations between welfare policy and applicant numbers that are statistically significant, but small in magnitude when compared with other determinant factors (Hatton, 2016; Di Iasio & Wahba, 2023). The few quasi-experimental studies on the topic tend to support the magnet hypothesis (Agersnap et al., 2020; Dellinger & Huber, 2021a). Dellinger & Huber (2020) exploit benefit differentials between Austrian states to study the influence of welfare on asylum-seeker location choice, finding that the locational distribution of asylum applicants is responsive to benefit levels. Agersnap et al. (2020) examine the effect of Danish welfare policy reform at the start of the 21st century. They find that a reduction of welfare benefits for asylum seekers led to a significant reduction in inflows, with the reinstatement of these benefits having an opposite effect (Agersnap et al., 2020).

3.3 Labour Market Access

Studies specifically pertaining to labour market access are a small subset of the welfare literature. Scholars that examine the effects of labour market access usually do so as a facet of broader welfare policy, rather than as a major determinant in its own right. For instance, both Brekke et al. (2017) and Hatton (2016) include labour market access within composite indicators relating to aspects of welfare policy. In both cases, modest positive effects on application levels were reported; however, little can be inferred about the effects of labour market access in isolation. With respect to qualitative literature, studies suggest that employment prospects form at least part of the decision-making matrix of asylum-seekers (Brekke & Aarset, 2009; McAuliffe & Jayasuriya, 2016; Crawley & Hagen-Zanker, 2019). Crawley & Hagen-Zanker (2019) found that the "juxtaposition" of employment opportunities with welfare support opportunities was a recurring component in the decision-making process of asylum seekers when choosing between Germany and Sweden as destinations. However, they also note that respondents' views were "rooted more in stereotypes than policy knowledge", implying that destination choice may not be responsive to short-term policy changes (Crawley & Hagen-Zanker, 2019, p. 30).

A single, recent quantitative study (Di Iasio & Wahba, 2023) directly examines the relationship between employment rights and asylum applications. Using a variable that captures the nominal length of time before asylum seekers can apply for access to the labour market, i.e. the length of labour market 'ban', the authors find a modest, statistically significant, positive association between access and application numbers (Di Iasio & Wahba, 2023). Their results indicate that a 1 percent reduction in ban length is associated with a 0.18 percent increase in first time asylum applications. Although the authors suggest that this association is marginal when compared with social network factors and other pull factors, if taken at face value it is argued this correlation has substantial practical implications. To take a hypothetical example, the result indicates that reducing an employment ban from 12 months to 6 months would be associated with a 9 percent increase in asylum applications. These results are not causal in nature and should not be interpreted as such. Nevertheless, viewed in tandem with the larger body of literature on welfare, they are indicative of a relationship in line with the welfare magnet hypothesis and utility-maximising perspectives more generally.

3.4 Hypothesis Formation

Taking into account the limited empirical evidence regarding the causal relationship in question, this paper adopts a utility-maximising framework in line with the majority of the existing quantitative literature. It is imagined that asylum seekers will choose to seek asylum where the utility associated with seeking asylum is higher than the utility associated with remaining in their country of origin. Further, it is imagined that asylum seekers will choose the destination country that is associated with the highest utility gain. The supply of asylum seekers to a given country is thus a function of the utility associated with arriving in the destination country, the utility associated with remaining in the origin country and the utility associated with arriving in other destination countries. Given the mental-physical and pecuniary benefits associated with access to work, it is assumed that the utility associated with migrating to Ireland is higher with labour market access than it was without labour market access. As such, the expectation of this paper is that the end of the prohibition on labour market access will be associated with an increase in the number of asylum applications in Ireland.

> H1: Labour market access increased the number of asylum applications in Ireland.

This expectation lends itself to an important corollary hypothesis relating to the demographic composition of incoming asylum applicants. Given that the theorised increase is associated with labour market access, it is logical to expect that there will be a concomitant shift in demographic composition of applicants towards economically-active demographics and away from less economically-active demographics. It is not suggested that the entirety of the treatment effect will be associated with economically-active demographics; the existence of network/family effects with respect to dependent demographics must be remembered. Nevertheless, it is plausible that the end of the prohibition on labour market access be associated with a disproportionate increase in working-age applicant populations when compared to non-working age populations.

> H2: Labour market access resulted in a higher proportion of working-age asylum applicants in Ireland.

4 Data and Descriptive Statistics

4.1 Datasets

This paper primarily relies on the Eurostat (migr_asy) dataset, which contains asylum-related statistics on 34 European countries(Eurostat, 2023a, 2023b). Data on asylum is supplied by EU member states to Eurostat in accordance with Regulation (EC) No 862/2007. Eurostat adopts a number of quality assurance practices in order to ensure that the data is representative and of high quality (Eurostat, 2023d). Among other metrics, the data includes monthly data on asylum applications in each country, the number of pending applications in each country, the country of citizenship of applicants, the sex of applicants and the age category of applicants. The number and nature of first instance decisions on applications in each country is recorded at quarterly intervals. The data covers a period from January 2008 to August 2023. This corresponds to 188 monthly observations, with 126 observations in the pre-treatment period and 62 observations in the post-treatment period. This paper also employs quarterly GDP data from the Eurostat (naid_10) dataset and monthly unemployment-rate data from the OECD open statistics database for use as covariates(Eurostat, 2023c; OECD, 2023).

Monthly times series data on applications is reported across two categories, comprising 1. *First-time applications* and 2. *Subsequent applications*. Data on age is reported across seven categories. These include 1. *Less than 14 years*, 2. *From 14 to 17 years*, 3. *Less than 18 years*, 4. *From 18 to 35 years*, 5. *From 35 to 64 years*, 6. *65 years or over*, and 7. *Unknown*. Data on sex is reported across 3 categories, including 1. *Male*, 2. *Female*, and 3. *Unknown*. Quarterly time series data on first instance decisions is reported across five categories. These include 1. *Total positive decisions*, 2. *Rejected*, 3. *Geneva Convention Status*, 4. *Humanitarian Status* and 5. *Subsidiary Protection Status*. Data is aggregated by country of citizenship of applicants and by reporting entity, i.e. country of destination. The data also includes two composite metrics pertaining to the EU-27, which combine the EU-27 countries and non-EU-27 countries into representative single entities in the dataset.

4.2 Variables

The variables used derive from the dataset as described above. The main dependent variable used in this paper's difference in difference and synthetic difference in difference models, with respect hypothesis (1), is the monthly 1^{st} time applications in Ireland and a control group consisting of Netherlands, Belgium, Slovenia, Norway and Portugal. Models are also estimated is using a variable consisting of monthly 1^{st} time applications exclusive of Syrian-origin applications. A simple index variable was constructed to identify observations from each country. Separate dummy variables corresponding to each value of the index were constructed. A dummy variable taking value of 0 prior to July 2018 and 1 after was constructed to indicate the treatment period. Twelve dummies corresponding to each month (January, February, etc) were also constructed, as were two dummies indicating periods affected by the 2015 Syrian refugee crisis and the Covid-19 pandemic. Covariates were included for each country, comprising acceptance rate, unemployment rate and GDP. The acceptance rate variable was constructed by dividing the quarterly total positive decisions on applications by the total decisions on applications for each country, then interpolating the data to a monthly level. The GDP variable was also constructed by taking quarterly GDP data aggregated by country and interpolating it to a monthly level. The unemployment rate variable was taken directly from monthly data in the OECD database.

The main dependent variable used in the Regression Discontinuity model is the natural log of monthly 1st time applications in Ireland, constructed by applying a log function to the existing variable. The primary running variable used, *m*, consists of a vector of length 188 normalised to 0 at the July 2018 threshold. Similar running variables are constructed for the February 2018 and June 2017 thresholds. Covariates unique to the Regression Discontinuity model include the total number of 1st time applications in the EU-27 (excluding Ireland and Nigerian-origin), which was constructed by taking the eponymous composite metric directly from the Eurostat database and subtracting the number of 1st time Irish applications and total 1st time Nigerian origin applications (excluding Ireland) in the EU-27. The total number of 1st time Nigerian origin applications (excluding Ireland) was constructed as a separate variable. Additional covariates used in the model are identical to those described above with respect to the Difference in Difference formulation.

Separate variables were constructed in order to test hypothesis (2). Variables corresponding to the number of (1st time) applicants under 18 years of age, the number of applicants between 18-35 years of age, the number of applicants between 35-65 years of age, and the number of applicants over 65 years of age were constructed for Ireland and a control group consisting of the Netherlands, Belgium, Slovenia, Norway and Portugal. Separate variables representing the proportion of total applicants in each category were calculated by dividing each of the aforementioned variables by the total number of 1st time applicants. A variable representing the total working age applicants was calculated by summing the number of applicants between 18-35 years of age and the number of applicants between 35-65 years of age. A corresponding variable was constructed for dependent applicants (under 18, over 65.

4.3 Descriptive Statistics

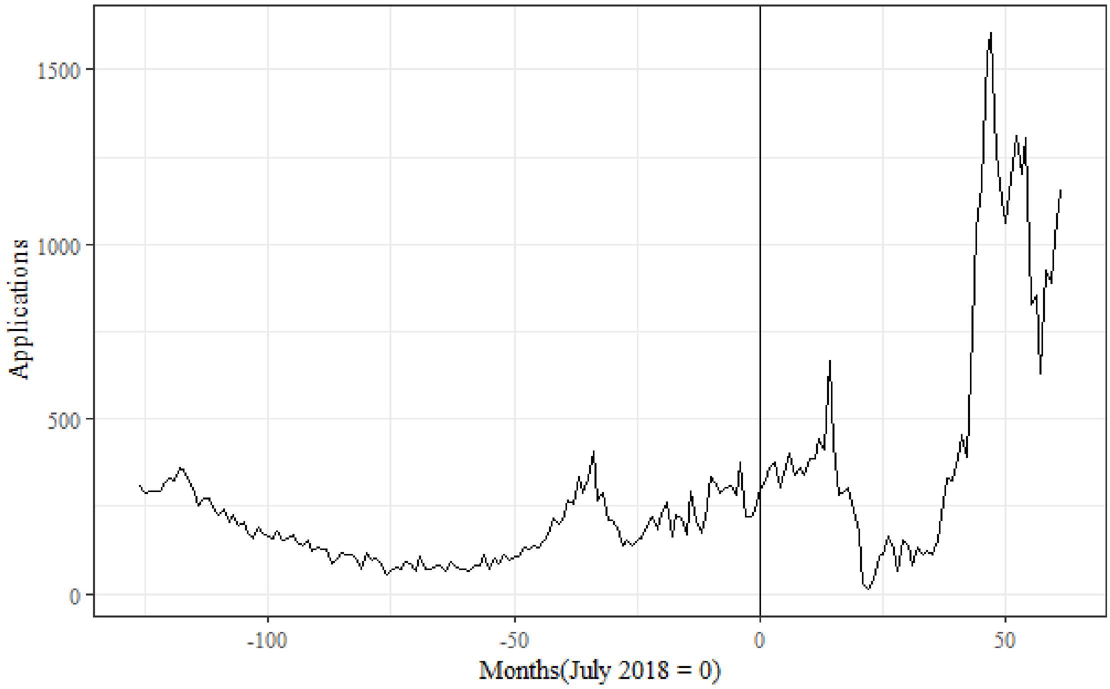

Figure 1: First time asylum applications in Ireland, 2008-2023

Figure 1 shows the monthly 1st time asylum applications in Ireland from January 2008 to August 2023. The graph suggests a concave upward trend prior to March 2020, with a small shock corresponding with the 2015 Syrian refugee crisis. The dramatic drop in March 2020 corresponds with the onset of the Covid-19 pandemic. A large-post Covid-19 shock is also apparent from the graph. The vertical line at 0 represents the threshold corresponding to the change in labour market access. **Table 1** consists of vital statistics covering the same time period. The mean number of first-time applications is ~294, which is less than 1/5th the value of the highest observation in the sample. Statistics are displayed showing the proportion of applicants across sex and age categories. The average asylum applicant in Ireland is male between the ages of 18 and 35, which is in line with the typical demographic patterns associated with irregular migration.

Table 1: First-time asylum applications in Ireland

	Mean	Standard deviation	Median
1st time applications	293.67	300.978	210
Female	0.352	0.067	0.357
Less than 18 years	0.244	0.066	0.25
18 – 34 years	0.523	0.075	0.511
35 – 64 years	0.228	0.057	0.227
Nigerian Origin	34.362	33.449	20
Georgian Origin	29.414	63.185	5*
Pakistani Origin	22.393	27.148	15
Algerian Origin	20.027	46.837	5*
Zimbabwean Origin	18.298	24.869	10
Somalian Origin	17.128	37.001	5*
Acceptance Rate	0.416	0.344	0.340
Unemployment Rate	9.308	4.048	8.2
GDP (billions)	70433.32	28522.92	76704

* Non-zero observations fewer than 5 were recorded as 5 for the purposes of anonymisation.

Statistics are displayed for the six most common countries of origin over the period. The most common country origin for asylum seekers was Nigeria with 11.7 percent of the total applicants received, followed by Georgia with 10 percent and Pakistan with 7.6 percent. Origin country flows varied significantly over the time period as evidenced by high standard deviations. **Figure 2** displays application trends for the top three origin countries. Nigerian-origin applications exhibit the most stable trend over time. Large spikes in Pakistani-origin and Georgian-origin coincide with the 2015 Syrian Refugee Crisis and the 2022 Russian invasion of Ukraine, respectively.

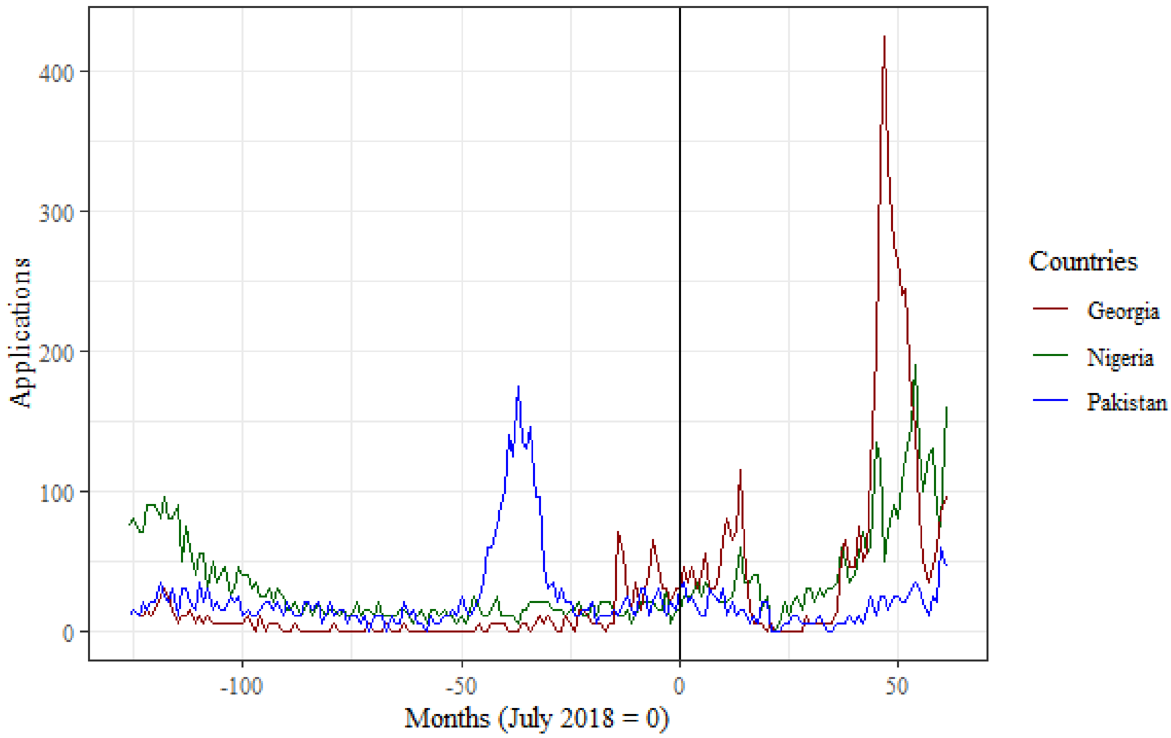

Figure 2: Applicant countries of origin 2008-2023

Figure 3 compares Irish application trends with 6 comparator countries. Comparator countries were chosen based on combination of graphical analysis of trends, applicant composition, population and economic development. The graph indicates that Ireland was relatively unaffected by the shock associated with the Syrian Refugee Crisis in 2015 when compared to larger continental countries and the UK. Sharp falls in applications coinciding with the onset of the Covid-19 pandemic are common to all countries, as is an increase coinciding with the end of the pandemic/start of the conflict in Ukraine; although it is important to note that Ukrainians are largely exempt from the traditional asylum application process within the EU-27.[1] An increase in applications is visible at the July threshold for all countries.

[1] Ukrainians are entitled to protection under the Temporary Protection Directive (2001/55 EC), activated by EU Council Decision EU 2022/382 on 4 March 2022.

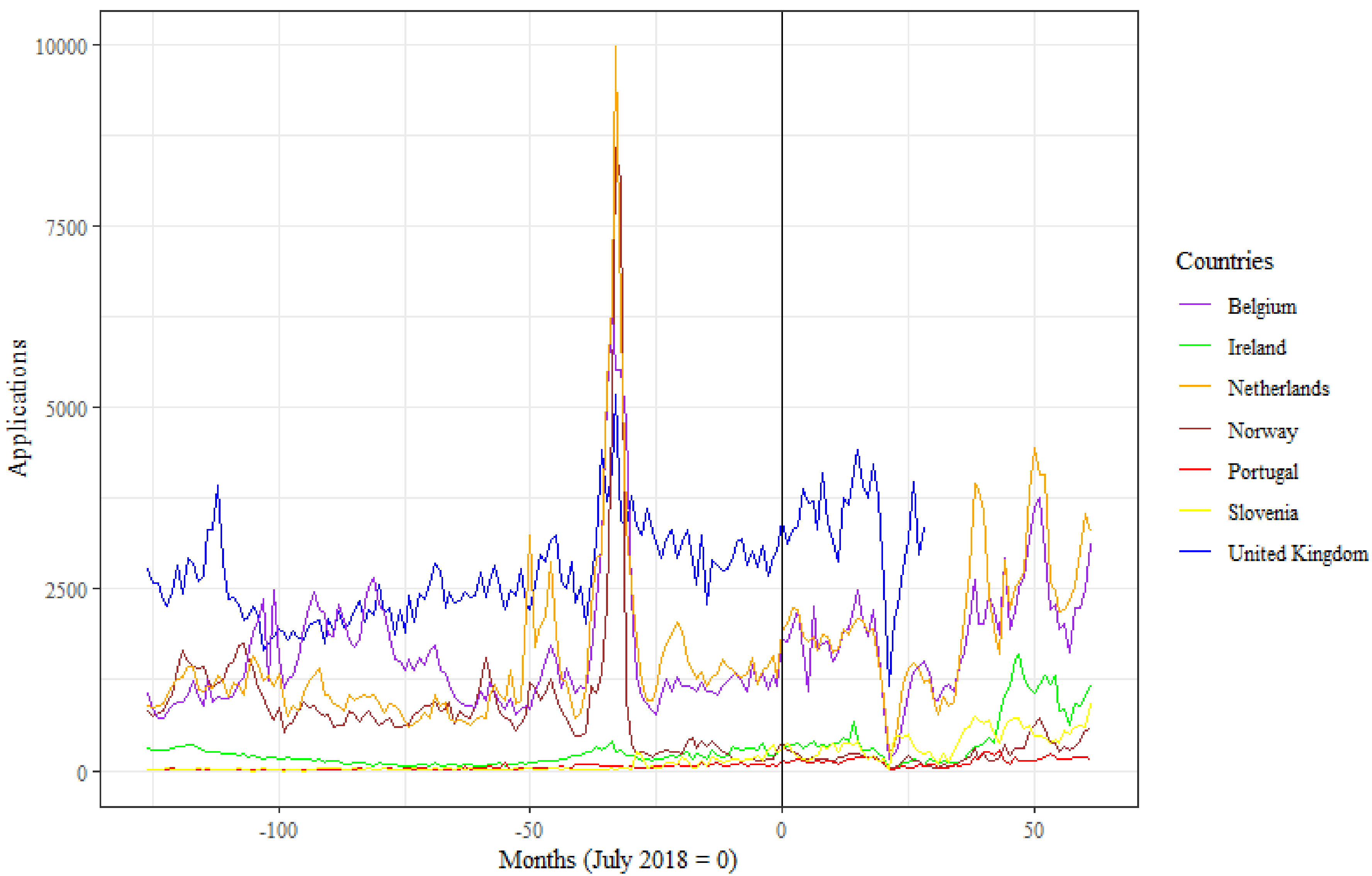

Figure 3: Application trends across countries

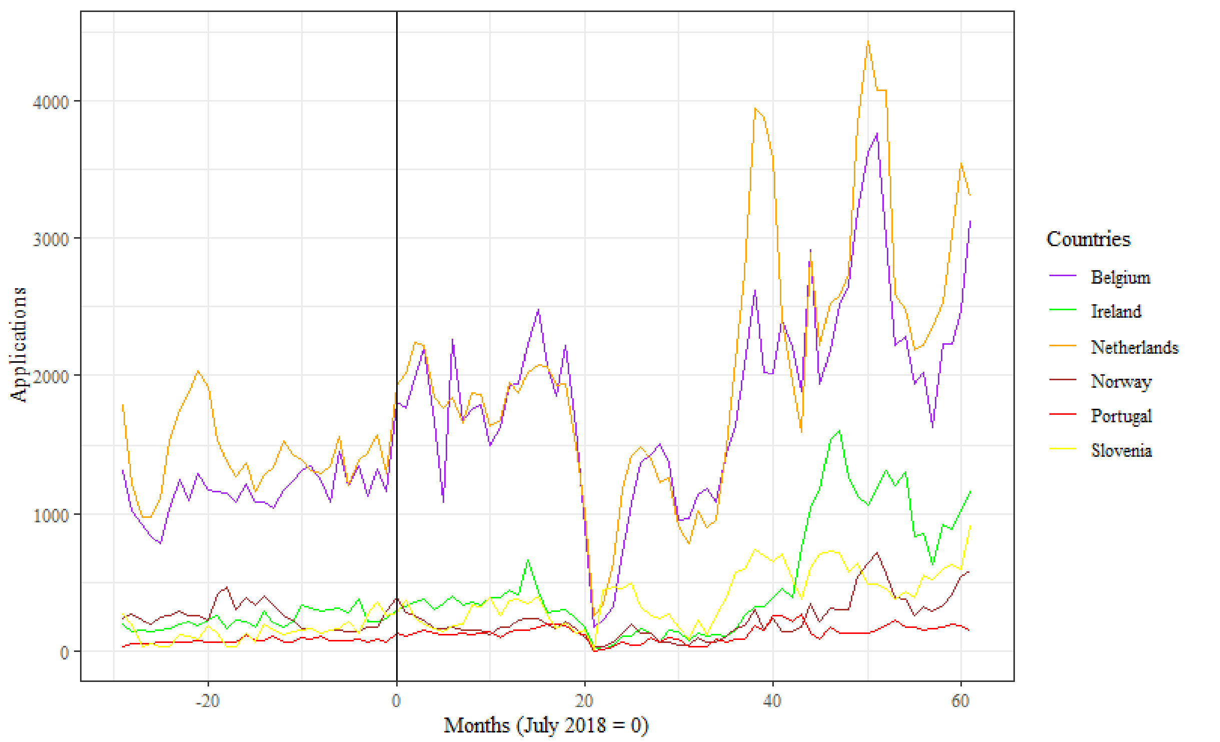

Figure 4: Application trends post-2015

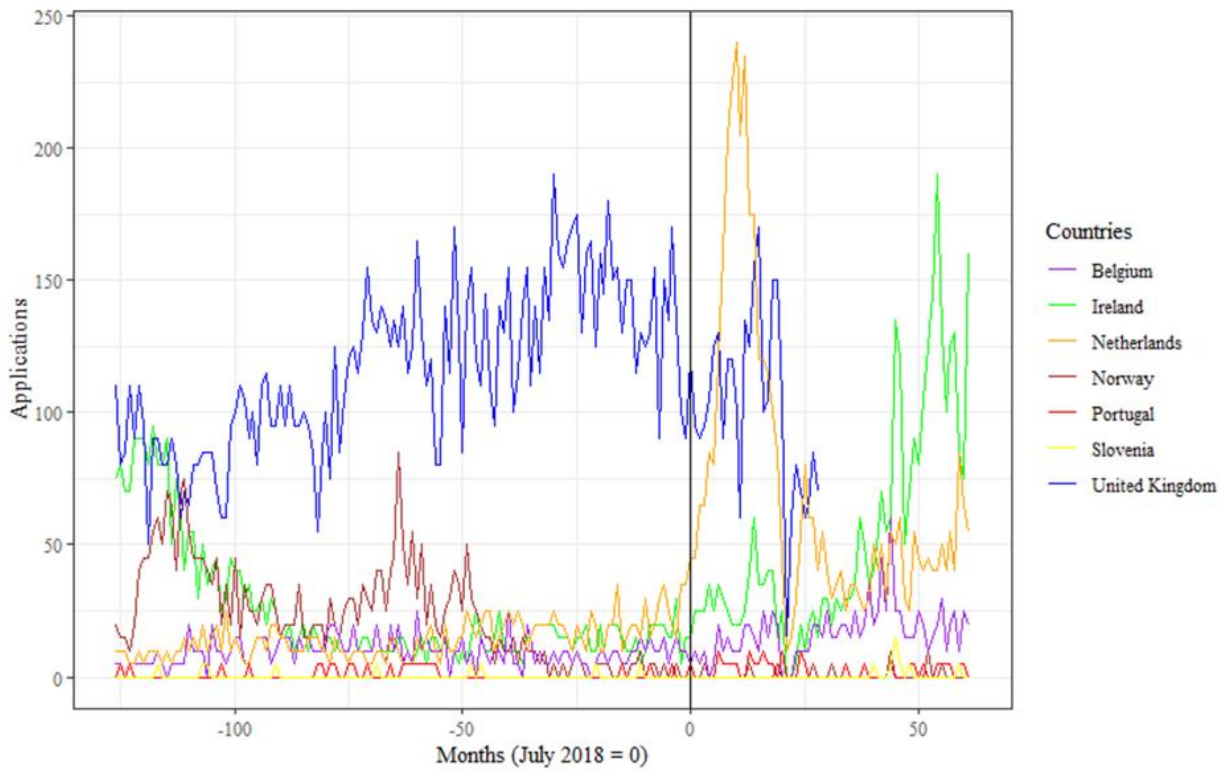

Figure 5. Nigerian-origin application trends

Figure 4 displays trends post-2015, excluding the UK. A common gradual upward trend is visible in all countries except Norway. Slovenia exhibits the greatest similarity to Ireland over time. A common jump is again visible at the July 2018 threshold. **Figure 5** displays trends for the same 6 countries, but restricts the sample to applications of Nigerian origin. Ireland's trend diverges significantly from other countries at the beginning of the pretreatment period. Interestingly, the UK and Ireland exhibit opposite trends which may be indicative of deflection effects between the two English speaking countries. A large spike around June 2019 is common to all countries, with the Netherlands experiencing by far the largest upsurge.

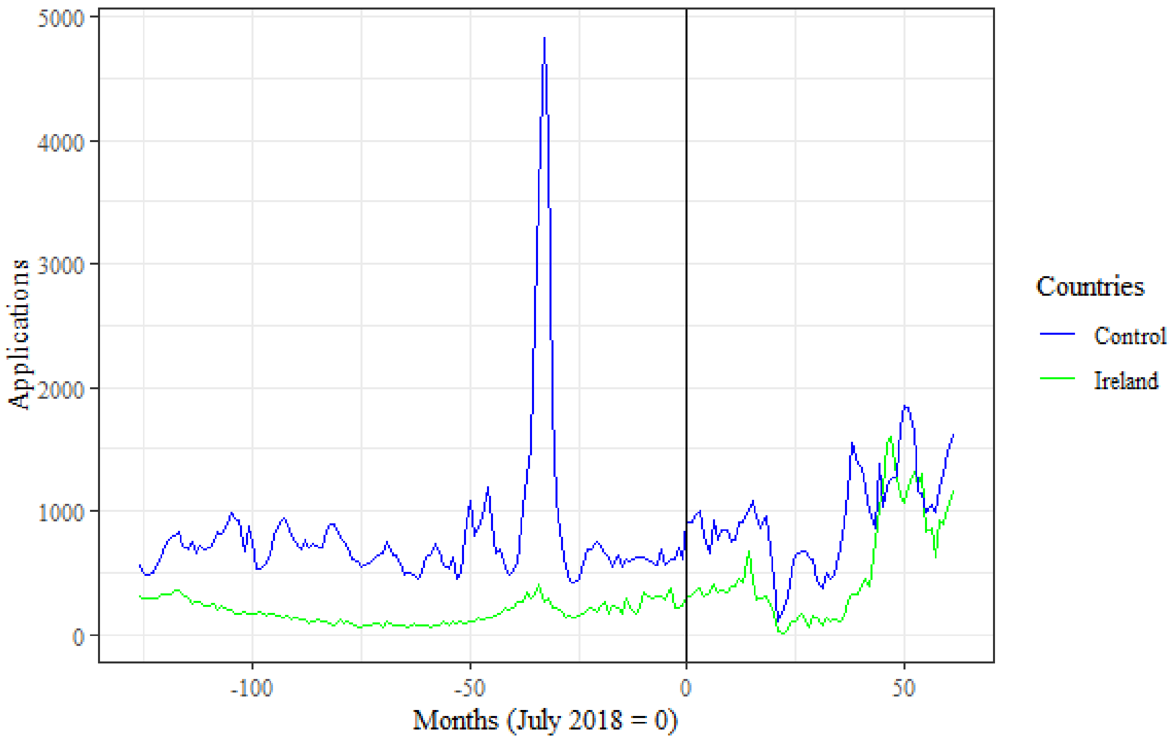

Figure 6: Control group application trends

Figure 6 displays the trend of the mean number of 1st time applications for the control group chosen with respect to this paper's Difference in Difference specification. The control group consists of five countries mentioned previously in this chapter, namely (1) the Netherlands, (2) Belgium, (3) Slovenia, (4) Norway and (5) Portugal. 1st time applications with 1st time applications in Ireland, over time. Separate statistics on each control unit are included in the appendix to this paper. The control group displays a higher mean number of 1st time applications than Ireland for the entirety of the pretreatment period, with the trends intersecting multiple times in the post-treatment period from 2022-2023. Trends largely align with the exception of a large shock corresponding to the 2015 Syrian refugee crisis. Unique geographic positioning as a small island in the Atlantic (with the UK as a buffer) likely helps to explain why Ireland did not experience this shock to the same extent as the continental control group. **Figure 7** displays 1st time applications for Ireland and the control group exclusive of Syrian applicants. Although reduced in magnitude, a noticeable divergence shock corresponding to the 2015 Syrian refugee crisis still apparent. **Figure 8** displays Syrian-origin applications for Ireland and the control group. In addition to the aforementioned shock in the pre-treatment period, significant divergences in the post-treatment period are apparent.

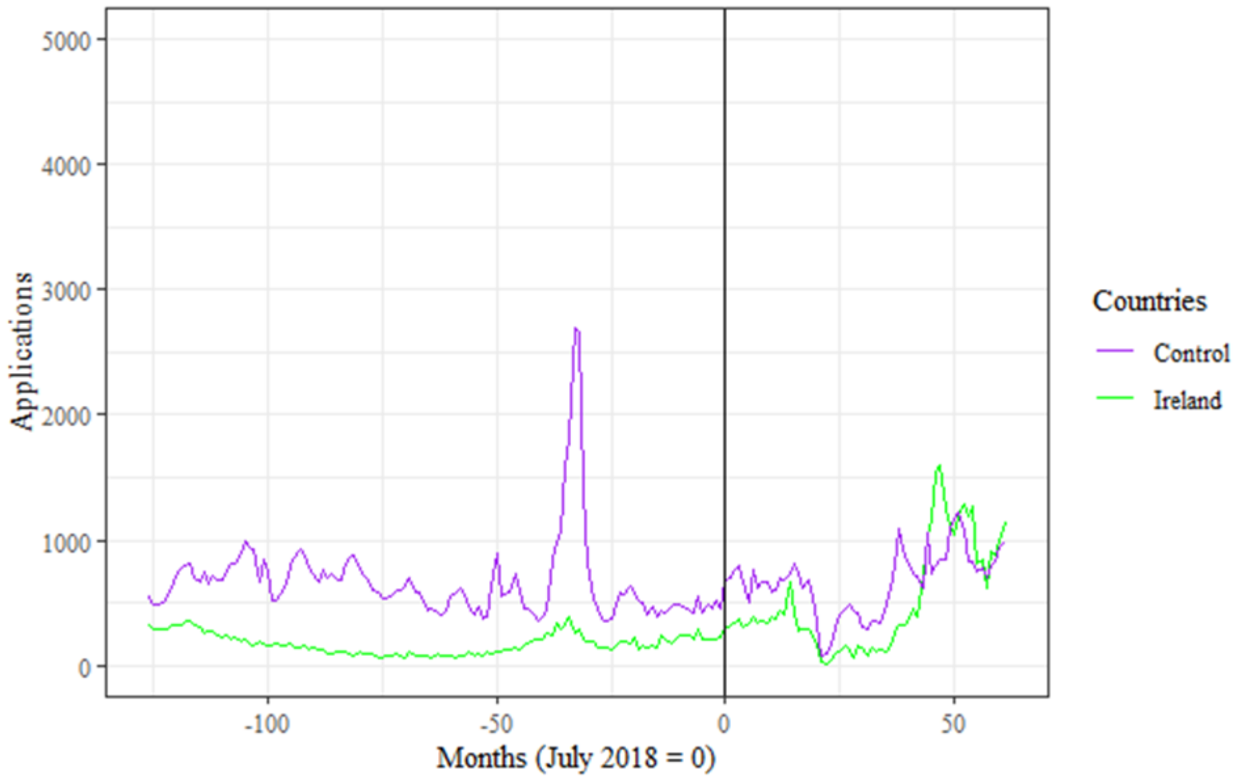

Figure 7: Control group application trends (excl. Syrian)

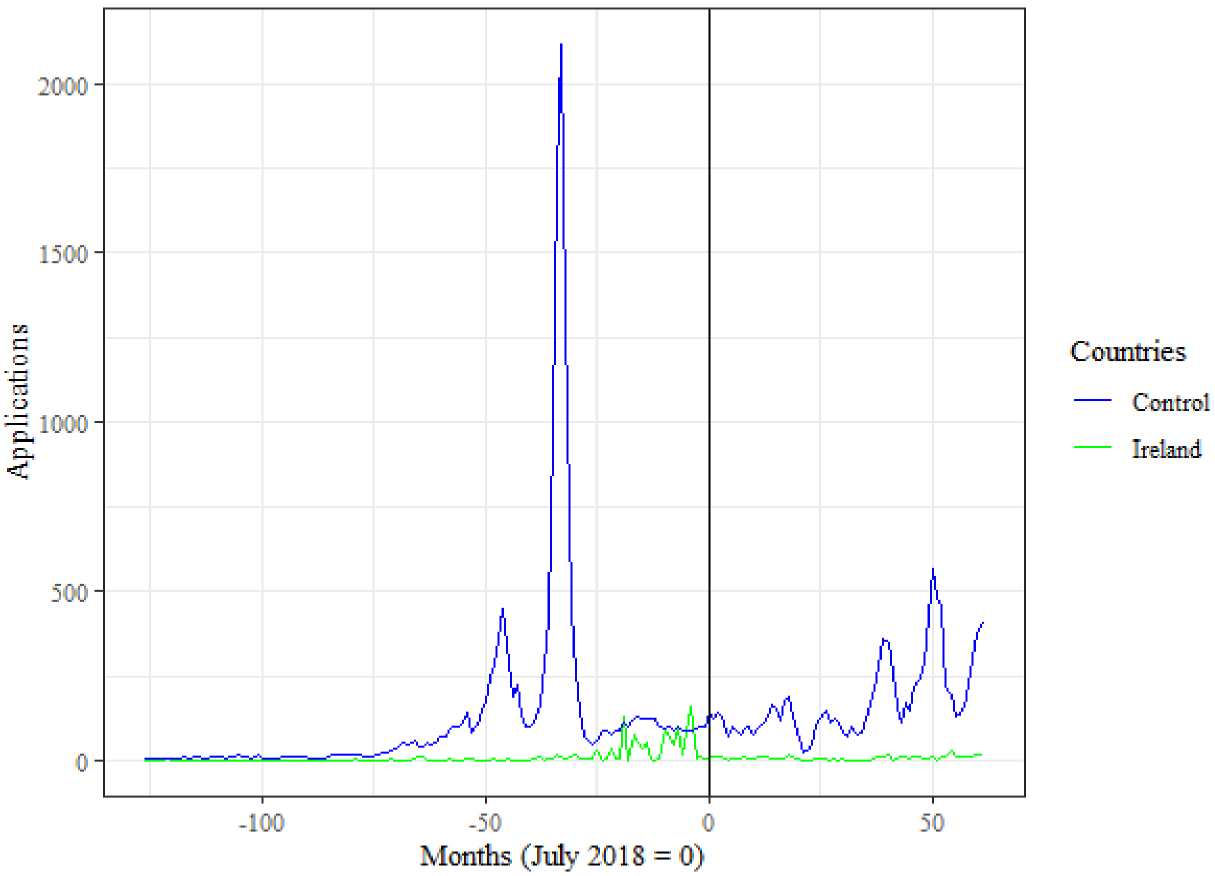

Figure 8: Control group application trends, Syrian-origin

Table 2 displays vital statistics on 1st time applications for the control group. Contrasting Table 2 with the results of Table 1, the control group displays higher mean 1st time applications than Ireland. Characteristics with respect to age category are largely similar between control and treatment group, although the table suggests that Ireland receives a higher proportion of female applicants than the control group. Again this may be due to geographic factors, as Irish applicants almost always arrive by way of air travel rather than by irregular means (Irish Refugee Council, 2023). Statistics are displayed for the same six origin countries as in Table 2 for the purposes of comparison. The largest divergence occurs with respect to Zimbabwean applicants, who are largely absent from the control sample. **Figure 9** displays trends in Irish 1st time applications across four age categories. Trends align largely align across the categories, with the exception of the over-65 category where application levels are too low to interpret a trend. A pre-treatment shock affecting the 18-35 category corresponds to the Syrian refugee crisis. Another large post-treatment, post-Covid 19 shock affecting the 18-35 category is also visible. Interestingly, there is significant divergence between the under-18 and 35-64 categories in the post-treatment, post-Covid-19 period. Up until this period, the categories display nearly identical trends. Finally, **Table 3** displays statistics corresponding to the categories featured in Figure 9.

Table 2: First-time asylum applications: Control group

	Mean	Standard deviation	Median
1st time applications	851.771	1046.54	612.5
Female	0.286	0.125	0.314
Less than 18 years	0.253	0.109	0.265
18 – 34 years	0.534	0.126	0.509
35 – 64 years	0.197	0.094	0.197
Nigerian Origin	12.72	24.391	5*
Georgian Origin	11.648	19.449	0
Pakistani Origin	13.622	23.009	5*
Algerian Origin	15.547	32.434	5*
Zimbabwean Origin	0.271	1.133	0
Somalian Origin	44.601	86.292	7.5
Acceptance Rate	0.464	0.457	0.221
Unemployment Rate	6.830	3.044	6.25
GDP (billions)	87249.56	61378.66	79451.7

* Non-zero observations fewer than 5 were recorded as 5 for the purposes of anonymisation.

Table 3: First-time asylum applications by age-category

	Mean	Standard deviation	Median
Less than 18 years	65.47	56.27	50
18 - 34 years	153.12	156.66	105
35 - 64 years	72.89	90.59	40
65+ years	1.329	2.84	0

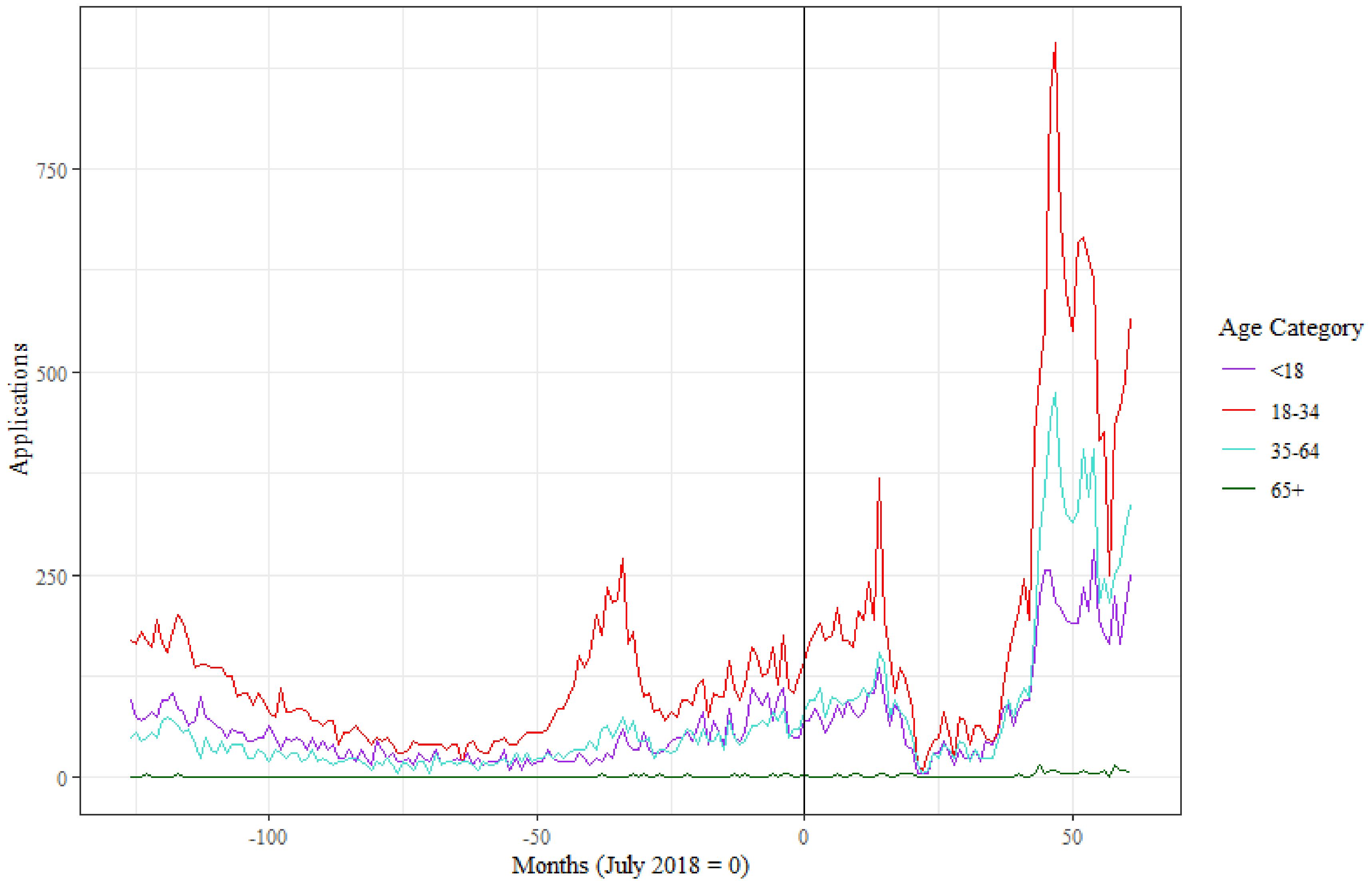

Figure 9: 1st time applications in Ireland by age-category

5 Empirical Strategy and Models

5.1 Choice of Models

This study employs three related empirical frameworks to examine the policy reform at hand; Difference in Difference (DiD), Regression Discontinuity in Time (RDiT) and Synthetic Control Difference in Difference (Synthetic DiD). Despite sharing many methodological characteristics, each framework relies on different assumptions, involves different comparisons and lends itself to different inferences. RDiT, for instance, provides easily interpretable estimates of causal effects based on observations from a single case (Angrist & Pischke, 2009). At the same time, the framework relies on the comparison of units from different time periods, which renders estimates vulnerable to bias from exogenous time-related shocks (Hausman & Rapson, 2018). This is particularly problematic when trying to estimate the effect of asylum policy change in one country, given the diverse and unpredictable push factors/deflection effects involved in the causal context of asylum-related migration. DiD, by contrast, involves the comparison of contemporaneous treatment and controls units, which helps to mitigate bias from common shocks. However, the framework relies on strong assumptions about common trends and similarity between units over time; assumptions that are often tenuous when confronted with the realities of cross-country analysis (Borusyak et al., 2023).

Synthetic DiD reduces reliance on such assumptions (to an extent) by combining multiple units to create an idealised control case. This has the potential to prove beneficial in this instance where control and treatment unit trends substantially diverge from parallel over a specific period, but entails further hurdles in terms of unit selection and interpretation (Abadie, 2021; Arkhangelsky et al., 2021; Clarke et al., 2023). The specific details of the different frameworks are described in greater detail below. In short, each framework entails strengths and weaknesses with respect to measuring the effects of the asylum-policy change at hand. By using multiple complementary designs, rather than relying on a single framework, it is hoped that the chance of drawing erroneous inferences may be minimised. Further, that a comprehensive picture of the effects of the reform may be constructed in spite of a challenging period of observations that encompasses the 2015 refugee crisis, the 2020-2021 Covid-19 pandemic and the 2022 Russian invasion of Ukraine.

5.2 Difference in Difference (DiD)

Difference in Difference is a method for causal inference that relies on the comparison of groups that differ in treatment status but are subject to common time trends (Lechner, 2011). A treatment group and non-treated group are compared both before and after treatment and a causal effect is inferred from the 'difference in the difference' between groups in the pre-treatment period and post-treatment period (Toshkov, 2016).. The primary identifying assumption underlying DiD methodology is the common trends assumption (Angrist & Pischke, 2009; Lechner, 2011). In short, it is assumed that control and treatment groups are subject to the same trends over time; both groups would have evolved in parallel but for the treatment intervention (Lechner, 2011). A secondary assumption in the context of policy evaluation is that the reform is unanticipated; the implications of this assumption for the results of this paper are discussed in section 5.4.

With respect to hypothesis (1), this paper employs a cross-country approach using monthly data. Our expectation on the basis of the hypothesis a positive and substantial treatment effect. Common trends are identified across a number European comparator countries, featured in the previous section ,which are then used to approximate the counterfactual. This paper estimates effects on 1st time applications at the July 2018 threshold using five countries as a counterfactual control group, Norway, Portugal, Slovenia, Netherlands and Belgium. These countries were chosen based on graphical analysis of trends, similarities in asylum applicant origins (particularly a high proportion of Nigerian origin applications), developmental and geographical factors (not EU border countries). A standard model is used as set out by Angrist & Pischke (2014). Covariates comprising acceptance rates, lagged unemployment rates, GDP, sex, and age category are included. Time dummies corresponding to the 2015 Syrian crisis and the 2020-2022 Covid-19 pandemic are incorporated. Month dummies are also included to account for potential seasonality. The model is specified as follows;

(1.1) $$A_{dt} = \alpha + \beta TREAT_c + \rho POST_T + \sigma(TREAT_c * POST_t) + \sum dC_{ct} + f \sum T + \varepsilon_{ct}$$

Where A_{dt} is the dependent variable, the number of first-time applications in a given month. $TREAT_c$ is a dummy variable taking values 1 for observations from the treatment country (Ireland) and 0 for observations from the control countries. The coefficient β captures the fixed differences between control and treatment units. $POST_T$ is a dummy variable taking value 1 in periods after July 2018 and value 0 for periods prior to July 2018.The coefficient ρ captures common changes over time periods. $TREAT_c * POST_t$ represents an interaction term capturing the relationship between TREAT and POST. The coefficient σ represents the causal effect of interest, i.e. the difference in the difference between treatment and control due to treatment. dC represents the specified covariates in the model and their coefficients. $f \sum T$ represents time and monthly dummies and their coefficients. Additionally, this paper also estimates the same model with Syrian-origin applications excluded from the dependent variable. This should help to mitigate divergence corresponding to 2015 Syrian crisis shock displayed in Figure 6, although Figure 7 suggests that only partial mitigation will be achieved. This approach may also help to block the impact of structural changes in migrant population composition resulting from the shock.

(1.2) $$S_{dt} = \alpha + \beta TREAT_c + \rho POST_T + \sigma(TREAT_c * POST_t) + \sum dC_{ct} + f \sum T + \varepsilon_{ct}$$

Where S_{dt} is the dependent variable, the number of first-time applications in a given month excluding Syrian applications. All other terms are identical to those described in relation to model (1.1).

With respect to hypothesis (2), this paper employs a within-country approach exploiting common trends across four applicant age-categories in Ireland. Firstly, effects are estimated on the number of 1st time applicants between the ages of 18-65(working -age) as compared to a control group made up of applicants under the age of 18 and over the age of 65 (dependent-age). A second regression is also estimated with the treatment group restricted to applicants under the age of 18 and the control group restricted to applicants between the ages of 35-64. In both cases, this paper's expectation on the basis of hypothesis (2) is a positive and substantial treatment effect. The base specification is as follows:

$$(1.3) \qquad W_{dt} = \alpha + \beta TREAT_c + \rho POST_T + \sigma(TREAT_c * POST_t) + \sum dC_{ct} + f\sum T + \varepsilon_{ct}$$

Where W_{dt} represents the number of applicants in Ireland in a given month. Covariates include acceptance rate and lagged unemployment rate. Other terms are, again, identical to those described in relation to model (1.1).

5.3 Synthetic Difference in Difference (Synthetic DiD)

Synthetic DiD is a method for causal inference that combines aspects of the traditional difference and difference framework with the synthetic control method developed by Abadie et al. (2010). The logic underlying the synthetic control method suggests that a combination of similar control units may better approximate the characteristics of the treatment unit than any single control unit alone; in particular with respect to unobservable characteristics (Abadie, 2021). Synthetic control methods seek to approximate the counterfactual for a treated unit by generating a weighting of comparable control units so as to create a single synthetic control unit that is as closely matched to the treatment unit as possible (Clarke et al., 2023). Importantly, this process reduces the need for parallel unit trends prior to treatment as a prerequisite for causal inference (Arkhangelsky et al., 2021).

Conventional synthetic control methods require the careful selection of units that are highly similar to the treatment unit. Where there are large discrepancies in the magnitude of observations between units, as is often the case of cross-country analysis, synthetic control methods will often generate sparse weights across a majority of control units (or the concentration of weights across a few units with observations of similar magnitude) even where these units share common trends with the treatment unit (Abadie, 2021). This limitation with respect to large time-invariant differences between units is a key weakness when compared to DiD methods (Clarke et al., 2023). The synthetic DiD method set out by Arkhangelsky et al. (2021) is designed to bridge the gap between the two methods. Synthetic DiD allows for control and treatment units to be trending at different levels, while also lessening the need for parallel trends across aggregated control data (Clarke et al., 2023). It is hoped that these properties may prove advantageous in terms of investigating hypothesis (1) in current case, where graphical evidence indicates that large shocks associated with the 2015 Syrian refugee crisis may contravene the strict assumptions behind the conventional DiD estimator.

This paper employs the synthetic DiD method set out by Arkhangelsky et al. (2021), as implemented by the 'synthdid' package in R, in order to construct a synthetic DiD estimator for average treatment effect associated with the policy in question (Hirshberg, 2023). As was the case with respect to the conventional DiD model, the expectation with respect to hypothesis (1) is a positive substantial treatment effect. An expanded panel of ten European countries is used, consisting of Belgium, Czechia, Denmark, Estonia, Germany, Netherlands, Portugal., Sweden, Norway and Slovenia. These countries were chosen based on geographical positioning within the EU and completeness of observations in the dataset over the time period between January 2008 and August 2023. The data was constructed as a panel matrix, with observations of the outcome variable (1st time applications) across 188 time periods, 10 control units and 1 treated unit. As with the conventional DiD model, effects are estimated with respect to both total 1st time applications and 1st time applications excluding Syrian-origin applications.

Synthdid estimates treatment effects through a four-step process. Following Arkhangelsky et al. (2021), Synthdid first computes a regularisation parameter which is then incorporated into its unit weight optimisation procedure to reduce overfit (Arkhangelsky et al., 2021; Hirshberg, 2023). Synthdid then optimises unit weights such that average outcomes in the treatment group are approximately parallel to the weighted average for control units (Arkhangelsky et al., 2021; Hirshberg, 2023). Time weights are optimised so as to hold constant the difference between control units in the pre-treatment periods and control units in the post-treatment periods (Arkhangelsky et al., 2021; Hirshberg, 2023). The estimation model used follows the optimisation problem described by Arkhangelsky et al. (2021) (p.7);

$$\left(\hat{\tau}^{\text{sdid}}, \hat{\mu}, \hat{\alpha}, \hat{\beta}\right) = \underset{\tau,\mu,\alpha,\beta}{\arg\min} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} \left(Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau\right)^2 \hat{\omega}_i^{\text{sdid}} \hat{\lambda}_t^{\text{sdid}} \right\}.$$

(Arkhangelsky et al., 2021, p.3)

Where $\hat{\tau}^{\text{sdid}}$ represents the average treatment effect, μ represents the intercept, α represents unit fixed effects and β represents time fixed effects. Y_{it} represents the outcome variable for unit i in time period t. W_{it} represents the treatment dummy, in this case taking values of 1 in periods after July 2018 for observations of the treated unit, Ireland. ω_i^{sdid} and λ_t^{sdid} represent the optimised unit and time weights, respectively. For situations where the panel consists of many control units and one treated unit, synthdid only has one implemented method for calculating standard errors, the 'placebo' method (Arkhangelsky et al., 2021; Hirshberg, 2023). As noted by Hirshberg (2023) and Arkhangelsky et al. (2021) this method can be 'untrustworthy' where strict homoskedasticity assumptions are not met, resulting in standard errors that are too large. It is unlikely that such assumptions are met in this instance, meaning reported confidence intervals should be interpreted cautiously.

5.4 Regression Discontinuity in Time (RDiT)

Regression discontinuity design is a method for causal inference predicated on the comparison of similar groups of units that are separated by an arbitrary cutoff, determined by the value of a some 'running variable'. Units on one side of the cut off are considered to be treated; units on the other side make up a quasi-control group. The effect of treatment is inferred by comparing linear trends on either side of the cutoff. Regression Discontinuity in Time (RDiT) is a type of regression discontinuity design that adapts the logic of the wider framework to instances where the treatment threshold occurs at a particular point in time (Hausman & Rapson, 2018). Despite underlying similarities, RDiT is distinct from 'regular' cross-sectional regression discontinuity designs in a number of important respects (Hausman & Rapson, 2018).

Using time as a running variable has several implications. Variation occurs in the dimension of time, meaning that units on either side of the threshold become more distant in time as the observational bandwidth increases (Hausman & Rapson, 2018). In other words, the comparability of observations decreases in proportion to the number of observations included. As a consequence, the inclusion of relevant control variables in the model is more important than in cross-sectional applications (Hausman & Rapson, 2018). Time is uniform in nature and, as such, non-random with respect to its distribution around a given threshold (Hausman & Rapson, 2018). This means that it is impossible (or illogical) to apply conventional tests for sorting effects such as the McCrary test when using RDiT (McCrary, 2008; Thoemmes et al., 2017; Hausman & Rapson, 2018). This is particularly important from a causal inference perspective. As noted by Hausman & Rapson (2018), the results of RDiT must be interpreted as compound effects that incorporate both treatment effects and unobservable anticipation or avoidance effects. Causal inference with respect to RDiT is thus highly dependent on contextual analysis.

This paper estimates effects on monthly asylum applications around three thresholds. The primary threshold examined is July 2018, which corresponds to the substantive end of the prohibition on labour market access in Ireland. The second threshold examined is February 2018, which corresponds to the beginning of interim arrangements. The third threshold examined is June 2017, which corresponds to the judgement of the Irish Supreme Court in the N.V.H case and the inception of a constitutional entitlement to seek work. Each threshold requires a separate regression and corresponding dummy variable, but uses the same base model. The model used replicates a standard sharp RD design as described by Angrist & Pischke (2009) and is similar to that employed by Gonzalez (2013). Control variables derived from previous analysis are included, comprising acceptance rates, lagged unemployment rates, total applications in the EU-27 (excluding Ireland) and a variable to account for differing numbers of days in each month (González, 2013). Acceptance rates are again calculated as the number of positive decisions on applications divided by the total number of decisions, interpolated from quarterly to monthly data. The base model is specified as follows:

(2.1) $$A_m = \alpha + \beta m + \rho D_m + \sigma(m * D) + \sum C_m + \varepsilon_m$$

Where A_i is the natural log of first time asylum applications in Ireland in month m , the running variable. D is a dummy variable dependent on m, taking a value of 1 for observations after the specified threshold and a value of 0 for observations before. C represents the specified covariates in the model. $(m * D)$ represents an interaction term, allowing for differing slopes on either side of the threshold. The coefficient of interest, ρ , represents compounded sorting and treatment effects as discussed above. A second model is estimated in order to test hypothesis (2). This time, the dependent variable used is the number of applicants in Ireland between the ages of 18-65 (i.e. working-age applicants) as a proportion of the total number of applicants in Ireland in a given month. Covariates added include the number of working age applicants in the EU-27 (excl. Ireland) as a proportion of the total EU-27 applicants, total applications in Ireland, lagged unemployment rate and acceptance rate. The specification is again otherwise identical to model (2.1).

(2.2) $$W_m = \alpha + \beta m + \rho D_m + \sigma(m * D) + \sum C_m + \varepsilon_m$$

5.5 Sorting and Anticipation effects

This policy change cannot be described as unanticipated. Nevertheless, it is argued that the nature of the sorting effects likely to be present are not incompatible with causal inference. There are a number of factors which suggest that the treatment effect is unlikely to be *overestimated* as a result of anticipation or sorting effects. Firstly, the capacity of asylum seekers to delay travel is likely to be limited. The nature of asylum-oriented migration is such that there is often an attenuated degree of volition involved on the part of asylum seekers (Van

Hear et al., 2018). It thus seems implausible that many asylum seekers would purposively delay migration for a period of months. Secondly, perhaps more importantly, asylum seekers had no logical incentive to delay travel. The directive involved specifically contemplates a waiting period prior to seeking employment and similar waiting periods are common to nearly all European asylum regimes. Furthermore, asylum seekers already face significant processing times in the DPD system and delaying travel also postpones any potential recognition of status (along with concomitant reunification rights and welfare entitlements).

Conversely, it is logical that we might see treatment effects prior to the substantive change in access. It is conceivable that potential applicants might have been exposed in the wake of publicity following the N.V.H decision, or following the public announcement and implementation of interim arrangements in February 2018. 'Early' applicants, in the sense of arrival pre-substantive labour access, would commence and conclude their waiting periods for both labour market access and recognition sooner. Given the balance of these factors, it is argued that any potential net sorting effect is likely to bias the estimated treatment effect of the RDiT or DiD model downward. This means that insignificant or negative treatment coefficients stemming from the aforementioned models must be interpreted with caution when making causal inferences. On the other hand, it is suggested that sorting effects are not a significant barrier to causal inference should the coefficient suggest a positive relationship.

6 Main Empirical Results and Robustness Checks

This chapter will first briefly describe the main results with respect to each model. The results of robustness checks will then be reported. Findings across models will be discussed and contextualised collectively, incorporating findings from the robustness checks, in the next chapter.

6.1 Initial Results

6.1.1 Difference in Difference

Table 4 displays the results for model 1.1. The dependent variable is total monthly 1^{st} time applications. The Slovenia unit dummy is removed in order to provide a reference country. Estimates for monthly dummies are not displayed for purposes of presentation, but are included in the appendix. The coefficient for the interaction term TREAT:POST represents the estimated average treatment effect associated with the labour market access reform. The estimated average treatment effect for model 1.1 is substantial and positive (with and without controls), but statistically insignificant. **Table 5** displays results for model 1.2, where Syrian applications are excluded from the dependent variable. This time, the coefficient for the interaction term suggests a substantial positive and statistically significant average treatment effect (with and without controls). The results indicate that average inflows to Ireland increased by approximately 235 applications per month in the post-treatment period, when compared to average outcomes in the control group.

Table 6 displays results for model 1.3 which tests hypothesis (2). The dependent variable is monthly first-time applicants in Ireland. As described in the previous section, two different sets of observations are used in separate regressions. The treatment unit in Group A is the number applicants between the ages of 18 and 65, with the control unit being the sum of applicants under 18 or over 65. The treatment unit in Group B is the number of applicants between the ages of 35 and 65, with the control unit consisting of the number applicants under the age of 18. Both groups show substantial positive and statistically significant average treatment effects. In the case of Group A, the average number of working-age applicants increased by 72 when compared with dependent applicants. In the case of Group B, the average number of applicants between the age of 35-65 increased by 48 when compared with the average number of applicants under the age of 18. The results for Group B are particularly striking, given the small, negative difference between treatment and control in the pre-treatment period suggested by the TREAT coefficient.

Table 4: Model (1.1) Results

	Applications	
	(Controls)	(Without Controls)
TREAT	55.795	65.123
	(133.198)	(84.788)
POST	139.973*	144.374***
	(72.994)	(50.111)
TREAT:POST	71.403	191.159
	(123.061)	(122.747)
Belgium	1,114.660***	1,494.149***
	(183.764)	(74.501)
Netherlands	539.013*	1,491.303***
	(294.452)	(74.501)
Norway	294.140**	541.569***
	(144.345)	(74.501)
Portugal	-2.491	-95.691
	(110.689)	(74.501)
Syria crisis	914.270***	
	(79.342)	
Covid-19	-464.273***	
	(73.373)	
GDP	0.007***	
	(0.002)	
Acceptance Rate	-172.806	
	(107.746)	
Unemp. Rate(t-1)	-19.680*	
	(10.132)	
Female	-1,377.381***	
	(220.738)	
Under 18	259.828	
	(215.421)	
Constant	389.154***	117.893**
	(125.631)	(55.211)
Observations	1,123	1,128
R^2	0.584	0.466
Adjusted R^2	0.575	0.463
Residual Std. Error	643.389 (df = 1097)	722.311 (df = 1120)
F Statistic	61.673*** (df = 25; 1097)	139.584*** (df = 7; 1120)
Notes:	***Significant at the 1 percent level.	
	**Significant at the 5 percent level.	
	*Significant at the 10 percent level.	

Table 5: Model (1.2) Results

	Applications (excl. Syrian)	
	(Controls)	(Without Controls)
TREAT	215.680**	42.960
	(89.617)	(56.873)
POST	127.910***	90.481***
	(49.111)	(33.613)
TREAT:POST	233.711***	247.155***
	(82.796)	(82.335)
Belgium	1,280.937***	1,313.723***
	(123.638)	(49.973)
Netherlands	843.188***	1,099.282***
	(198.110)	(49.973)
Norway	429.674***	437.952***
	(97.117)	(49.973)
Portugal	167.746**	-91.596*
	(74.473)	(49.973)
Syria crisis	514.035***	
	(53.382)	
Covid-19	-423.213***	
	(49.366)	
GDP	0.003**	
	(0.001)	
Acceptance Rate	-305.854***	
	(72.492)	
Unemp. Rate (t-1)	-32.290***	
	(6.817)	
Female	-978.415***	
	(148.515)	
Under 18	227.962	
Constant	511.579***	130.453***
	(84.526)	(37.034)
Observations	1,123	1,128
R^2	0.657	0.563
Adjusted R^2	0.649	0.560
Residual Std. Error	432.877 (df = 1097)	484.503 (df = 1120)
F Statistic	84.125*** (df = 25; 1097)	205.727*** (df = 7; 1120)
Notes:	***Significant at the 1 percent level.	
	**Significant at the 5 percent level.	
	*Significant at the 10 percent level.	

Table 6: Model (1.3) Results

	Applications (Group A)		Applications (Group B)	
	(Controls)	(Without Controls)	(Controls)	(Without Controls)
TREAT	72.040***	72.022***	-8.509	-8.509
	(8.843)	(9.109)	(7.624)	(8.030)
POST	13.351	57.942***	18.922	61.497***
	(10.493)	(7.931)	(11.776)	(9.888)
TREAT:POST	104.731***	104.757***	48.104***	48.058***
	(15.399)	(15.862)	(13.278)	(13.985)
Acceptance Rate	-2.739		1.798	
	(14.609)		(14.559)	
Unemp. Rate (t-1)	-7.607***		-6.906***	
	(1.296)		(1.291)	
Constant	114.269***	26.921**	118.944***	41.480***
	(20.527)	(11.699)	(20.662)	(12.376)
Observations	752	752	376	376
R^2	0.390	0.351	0.384	0.313
Adjusted R^2	0.377	0.339	0.356	0.286
Residual Std. Error	85.949 (df = 735)	88.534 (df = 737)	60.484 (df = 359)	63.704 (df = 361)
F Statistic	29.371*** (df = 16; 735)	28.471*** (df = 14; 737)	13.977*** (df = 16; 359)	11.730*** (df = 14; 361)

Notes: ***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

6.1.2 Synthetic Difference in Difference

Figure 10 displays results of the synthetic estimation for total 1st time applications. The estimated effect estimate is positive and substantial but statistically insignificant. The magnitude of the estimated average treatment effect (~170) is similar to the estimation of the conventional DiD model 1.1 without controls. Very wide estimated 95% confidence intervals are likely the result of heteroskedasticity across units interfering with the placebo error estimation method implemented by the synthdid package. As mentioned in the previous section, this estimation method relies on strict homoskedasticity across units and is untrustworthy where this condition is not met (Arkhangelsky et al., 2021; Hirshberg, 2023). Despite optimised weighting, a discontinuity in trends corresponding to the 2015 Syrian crisis is visible. **Figure 11** displays results of the synthetic estimation for 1st time applications excluding Syrian-origin applications. The estimated average treatment effect is positive, substantial and statistically insignificant, with very wide estimated 95% confidence intervals. The magnitude of the estimated average treatment effect (229) again closely resembles the estimation of the corresponding conventional DiD model 1.2. Although reduced in magnitude, a shock in the synthetic control trend corresponding to the 2015 Syrian crisis remains visually apparent despite the exclusion of Syrian-origin applications. **Table 7** displays control unit weightings for both synthetic specifications. **Figures 12** and **13** display unit by unit differences in differences corresponding to Figures 10 and 11, respectively.

Table 7: Units and Weights

	Unit Label	Weights Total	Weights Excl. Syrian
Belgium	1	0.114	0.111
Czechia	2	0.127	0.124
Denmark	3	0.116	0.115
Germany	4	-	-
Estonia	5	0.127	0.124
Netherlands	7	0.108	0.113
Portugal	8	0.127	0.124
Slovenia	9	0.127	0.124
Sweden	10	-	-
Norway	11	0.110	0.111

Notes:‘ – ‘ indicates that the unit did not contribute to the synthetic control unit.

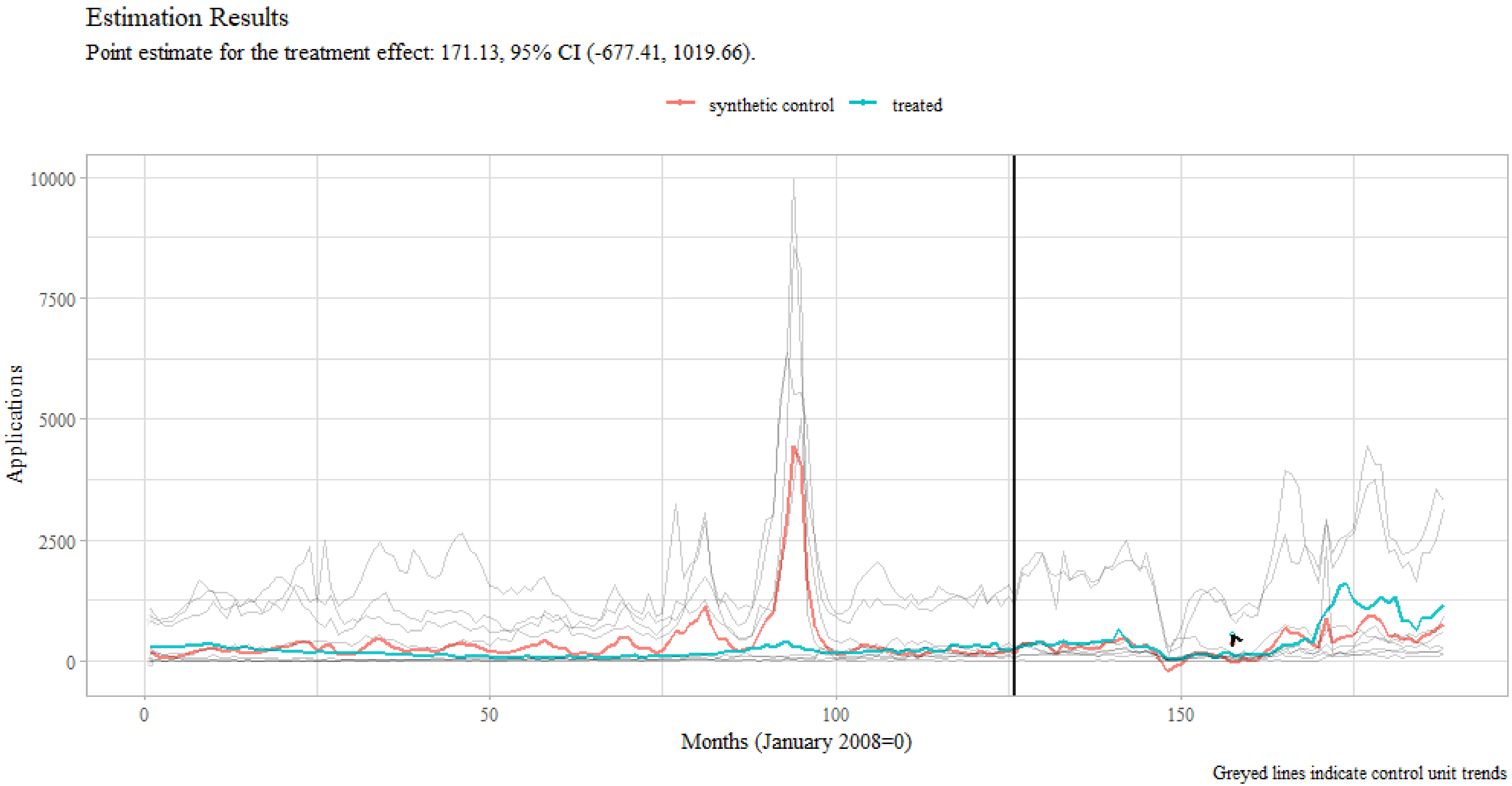

Figure 10: Synthetic DiD estimation results, total 1st time applications.

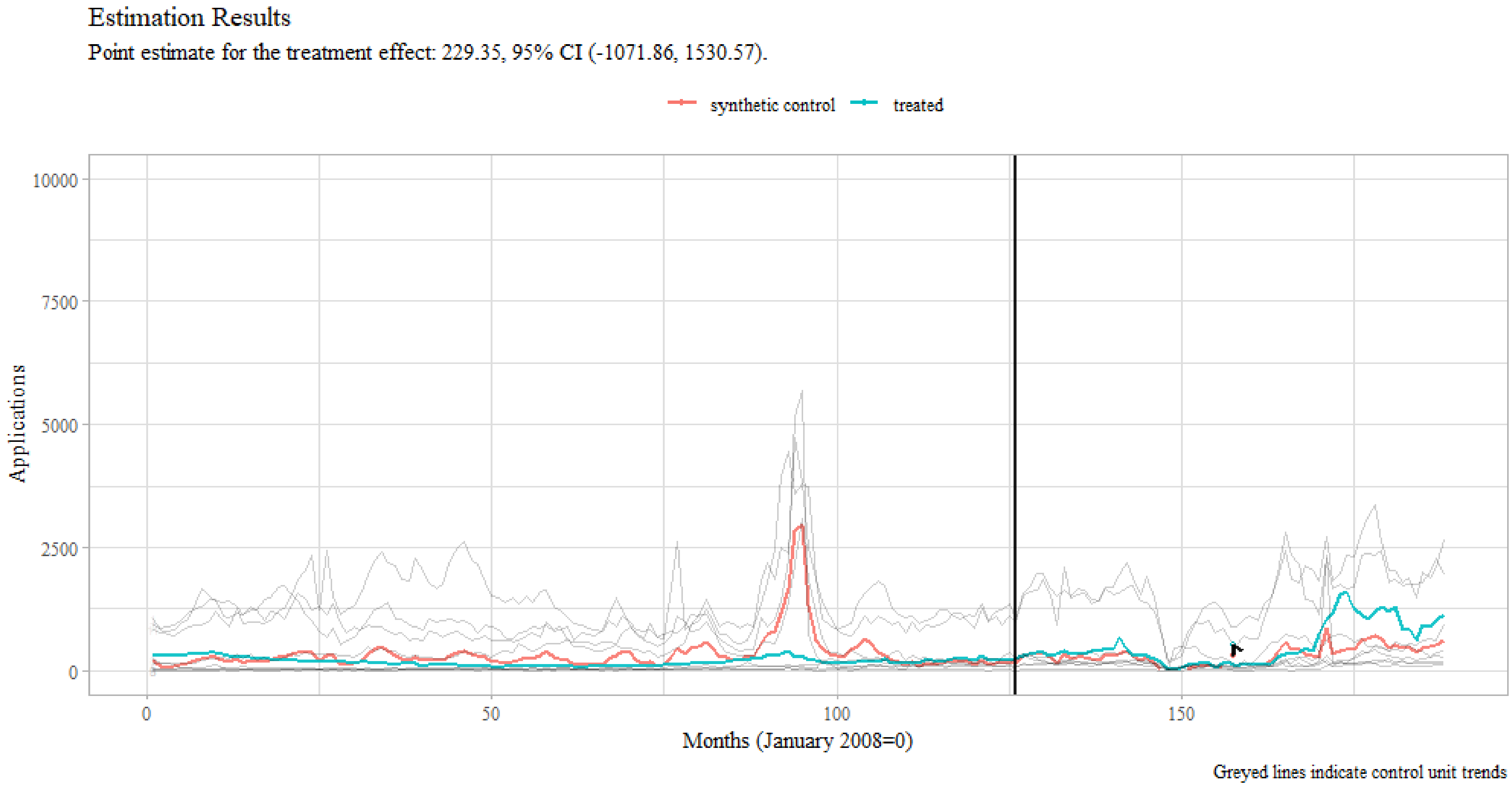

Figure 11: Synthetic DiD estimation results, 1st time applications excl. Syrian.

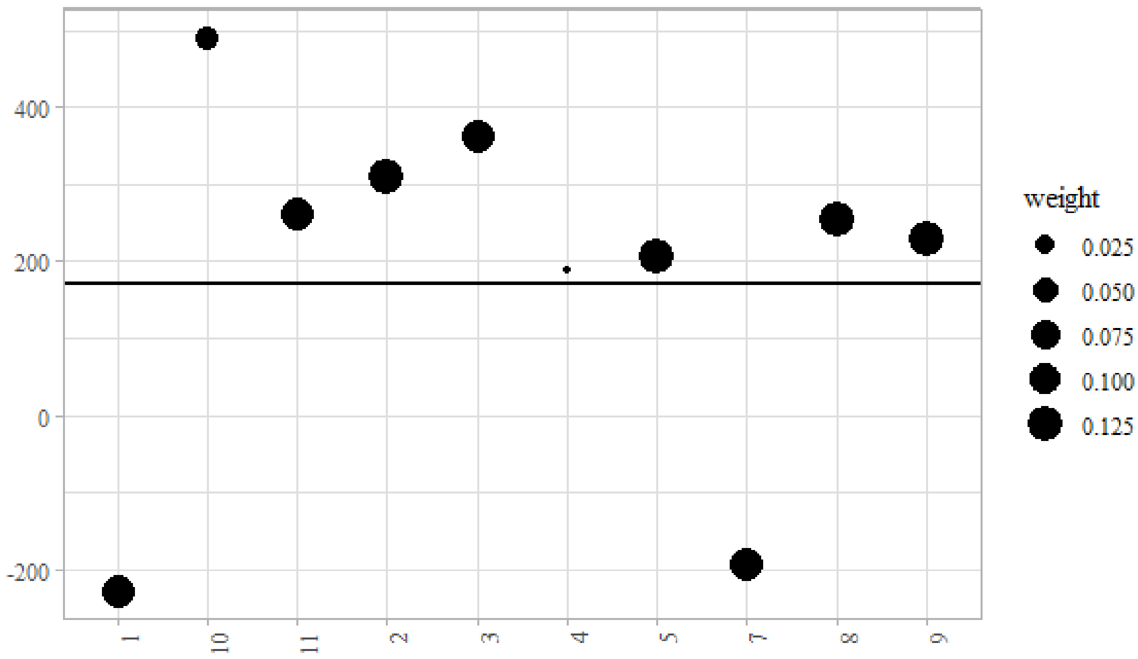

Figure 12: Unit by unit differences in differences (total 1st time applications).

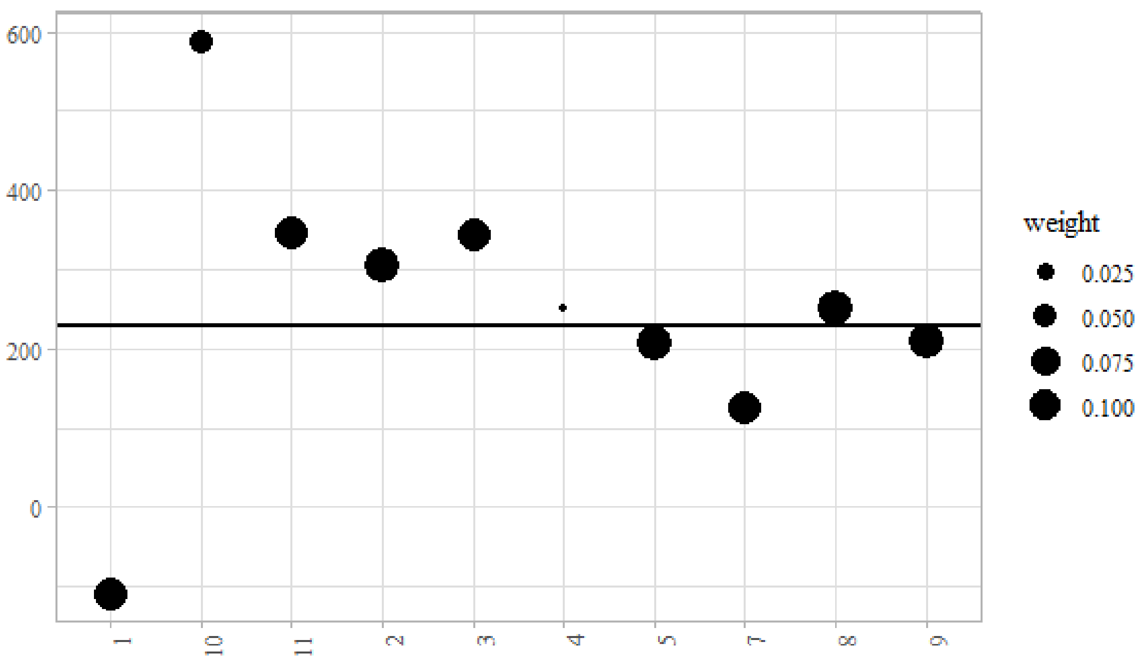

Figure 13: Unit by unit differences in differences (1st time applications excl. Syrian).

6.1.3 Regression Discontinuity

Table 8 displays results for model 2.1. Estimates are reported across three bandwidths at the July 2018 threshold and one bandwidth at the June 2017 and February 2018 thresholds. The estimated treatment effect at the July threshold is positive and significant at the 6 year, 2 year and 18 month bandwidths. The coefficients for these bandwidths suggests a 4 to 6 percent increase in monthly applications following the labour market access reform, consistent with the expectations of hypothesis (1). Effects estimated at the June 2017 and February 2018 thresholds are negative, relatively small in magnitude and statistically insignificant. These results at pre-treatment thresholds provide some evidence against the existence of significant anticipatory early treatment effects resulting from the N.V.H judgement or the implementation of interim arrangements, in addition to their utility as quasi-placebo tests. **Figure 14** visualises the results of model 2.1 at the 6 year bandwidth.

Table 9 displays results for model 2.2. The dependent variable is the proportion of working age applicants in Ireland in a given month. Estimates are reported across three bandwidths at the July 2018 threshold. While all treatment coefficients are positive, only the 6 year bandwidth estimate is statistically significant. As evidenced by **Figure 15,** the significant result at the 6 year bandwidth incorporates a number of outlying observations which coincide with the beginning of the Covid-19 pandemic. It is conceivable, but not certain, that the treatment effect observed may be a result of pandemic-related factors rather than the labour market access change. The fact that the estimated treatment effect is significantly lower (although still positive) for bandwidths that do not overlap with the pandemic lends some credence to this line of reasoning.

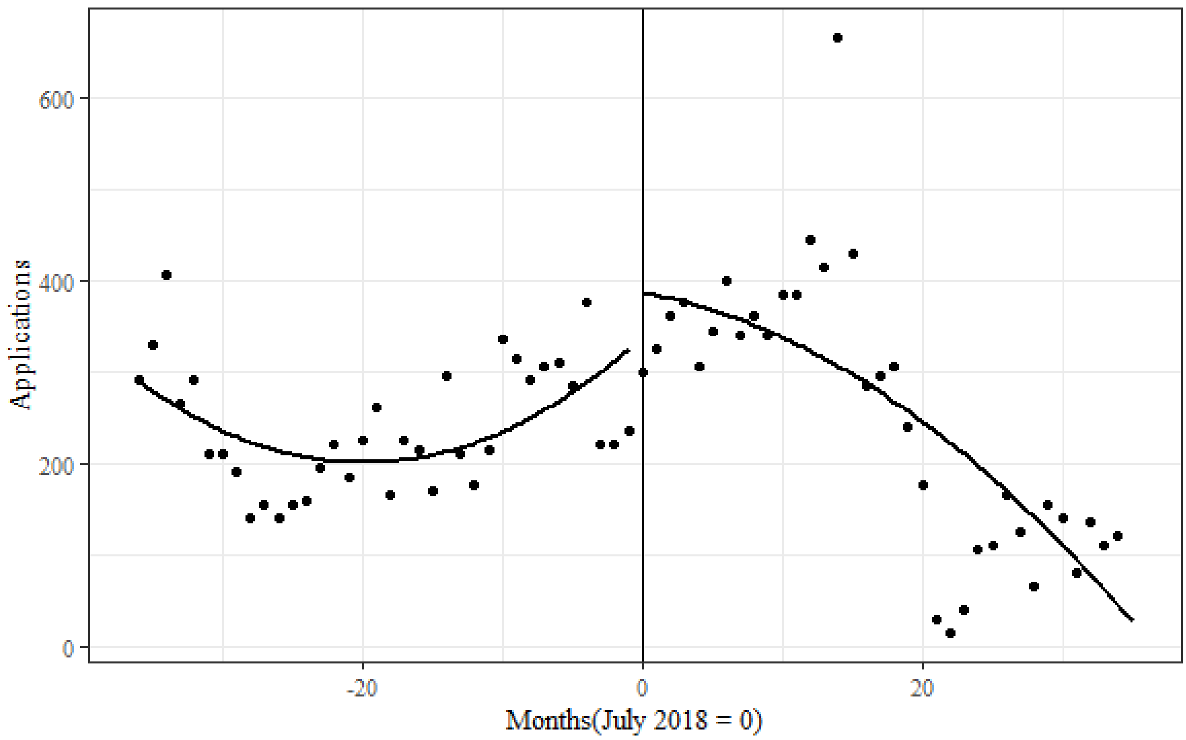

Figure 14: Results of model (2.1) at the 72 month bandwidth.

Table 8: Model (2.1) Results

	July 2018 6 years (1)	July 2018 2 years (2)	July 2018 18 months (3)	July 2018 12 months (4)	February 2018 18 months (6)	June 2017 18 months (7)
Log Applications	0.645***	0.584**	0.380**	0.484	-0.057	-0.171
	(0.274)	(0.282)	(0.164)	(0.254)	(0.329)	(0.281)
Polynomial	2	2	N	N	N	N
Bandwidth	Jul 2015-Jun 2021	Jul 2017-Jun 2020	Oct 2017-May 2019	Jan 2018-Dec 2018	May 2017-Nov 2018	Sep 2017-Mar 2018
N (months)	72	24	18	12	18	18
EU-27	0.0001***	(0.000)	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
EU-27 (Nigerian Origin)	0.000	0.000	0.000	0.0001	0.000	0.000
	(0.000)	(0.000)	(0.000)	(-0.004)	(0.000)	(0.000)
Unemployment Rate (t-3)	0.539**	0.201				
	(0.269)	(0.321)				
Acceptance Rate	1.418**	-0.217				
	(0.514)	(0.529)				
Days in Month	Y	Y	Y	Y	Y	Y

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 9: Model (2.2) Results

	July 2018 6 years (1)	July 2018 2 years (2)	July 2018 18 months (3)	July 2018 12 months (4)
Proportion Working Age	0.136***	0.125	0.034	0.023
	(0.039)	(0.073)	(0.128)	(0.088)
Polynomial	N	N	N	N
Bandwidth	Jul 2015-Jun 2021	Jul 2017-Jun 2020	Oct 2017-May 2019	Jan 2018-Dec 2018
N (months)	72	24	18	12
EU-27	-0.839	0.698	0.158	0.489
	(0.459)	(0.984)	(1.509)	(20.000)
Total Ireland	-0.0001	-0.0003	0.000	-0.000
	(0.0001)	(0.0002)	(0.000)	(0.000)
Unemployment Rate (t-3)	0.539	0.002	0.050	
	(0.269)	(0.065)	(0.118)	
Acceptance Rate	0.021	0.166	0.116	
	(0.04)	(0.173)	(0.294)	
Days in Month	Y	Y	Y	Y

***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

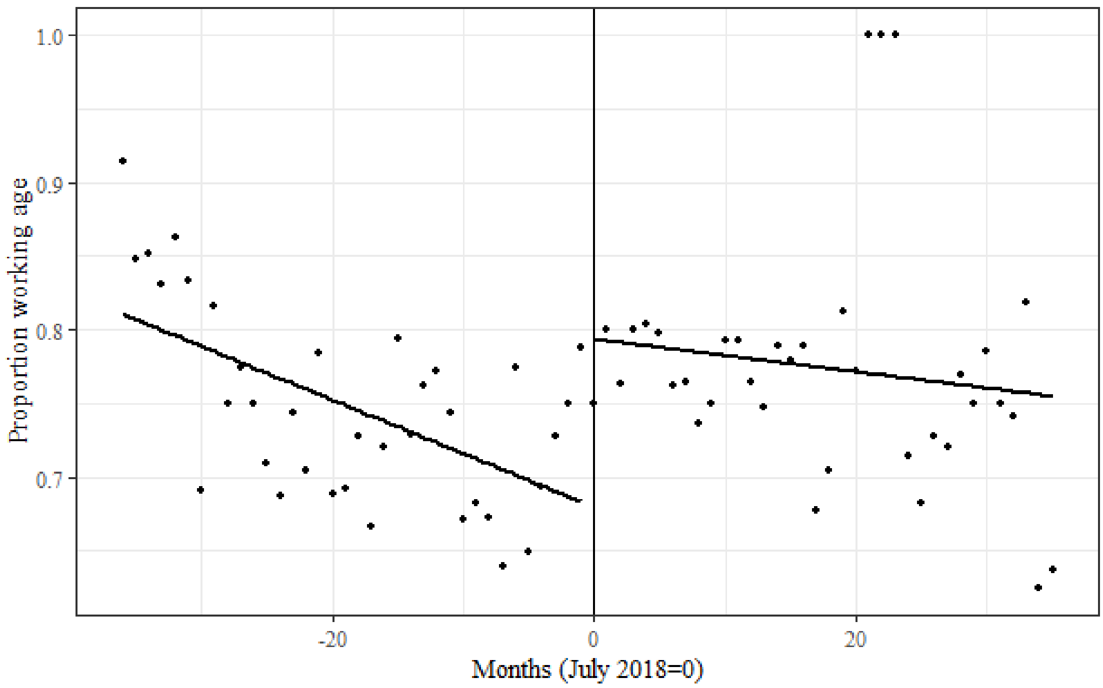

Figure 15: Results of model (2.1) at the 72 month bandwidth.

6.2 Robustness Checks

6.2.1 Difference in Difference

The existence of pre-treatment parallel trends between treatment and control groups is a core underlying assumption in the DiD methodology. With respect to models 1.1 and 1.2, Figures 6 and 7 suggest parallel trends between treatment and control group with the important exception of a large divergence corresponding to the 2015 Syrian crisis. In the case of model 1.3, Figure 9 suggests consistent parallel trends between the under 18 and 35-65 year old applicant age groupings (Group B). The 18-35 age category, however, suffers a significant incongruous spike in the pre-treatment period, again coinciding with the 2015 Syrian crisis. This category is included in the Group A specification. In order to substantiate this visual intuition, dummy variables corresponding to a number of yearly time periods and the Syrian crisis were constructed. These then were then added to each model and interacted with the treatment group dummy in a series of separate regressions.

Table 10: Robustness Check, Parallel Trends

	1.1	1.2	1.3 (Group A)	1.3 (Group B)
2008:TREAT	143.6	-68.1	92.1***	26.5
	(207.7)	(-140.1)	(27.4)	20.0
2010:TREAT	102.9	57.5	1.7	-3.9
	(202.6)	(137.2)	(27.4)	(20.0)
2012:TREAT	98.15	66.9	-44.1	-15.2
	(203.3)	(140.7)	(27.4)	(20.0)
Syria:TREAT	-1368***	-764.3***	83.71***	-19.9
	(251.1)	(145.6)	(26.6)	(19.3)
2017:TREAT	-129.5	-40.2	34.7	22.3
	(214.7)	(145.4)	(27.4)	(20.0)

***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 10 displays the results for each interaction term across all 4 specifications. The results largely correspond to the visual intuition described above. All models except for 1.3 (Group B) display statistically significant coefficients for the Syria dummy. Model 1.3 (Group A) also has a statistically significant coefficient for the 2008 dummy. As such, in every case bar model 1.3 (Group B), we see an apparent violation of the common trends assumption. This evidently has negative implications for the validity of causal inference on the basis of the models. However, it is submitted that the results of this robustness check are not necessarily fatal to inference. The overall results from Table 10 indicate, in line with the graphical evidence, that the divergence in trends due to the Syrian crisis was an isolated event. As discussed briefly in chapter 3, Ireland's divergence from common European trends at this time is explicable. The Syria crisis involved an unprecedented influx of irregular migration, with asylum seekers travelling through Eastern Europe and across the Mediterranean sea to physically traverse EU borders. Uniquely positioned as an island in the Atlantic, Ireland was naturally insulated from the impact of this influx.

Outside of this period, by contrast, the majority of asylum applicants in the EU arrived by conventional means such as air travel, particularly in non-border countries which make up the control group (European Council, 2023; Eurostat, 2023a). Ireland's geographic positioning matters much less with respect to conventional travel, which explains why control and treatment group trends are aligned before and after the crisis event. Figure 5 in chapter 3, which portrays trends post-2015, constitutes a clear graphical representation of this return to alignment. The control group is thus not necessarily an inappropriate counterfactual for Ireland, but the size of treatment estimates including the Syrian crisis shock are likely upwardly-biased due to the impact of the shock on mean outcomes across the control group. With this in mind, **Table 11** and **Table 12** display estimates for models 1.1 and 1.2, respectively, with observations from January 2015 - January 2016 (Syrian crisis shock) removed. Treatment estimates for model 1.1 remain statistically insignificant, with a negative coefficient including controls and a positive coefficient without controls. Estimates for model 1.2, excluding Syrian applications, remain statistically significant and positive (with and without controls). As predicted, the magnitude of the estimated average treatment effect for model 1.2 is considerably smaller than that estimated with all time periods included. Nevertheless, it remains substantial; a 133-197 increase in the mean number of non-Syrian asylum applications compared to the pre-treatment period.

6.2.2 Synthetic Difference in Difference

Following the logic outlined with respect to the conventional DiD, the Synthetic DiD model was re-estimated with observations from January 2015 - January 2016 removed. The model was estimated without Syrian-origin applications. The results remained positive and statistically insignificant with large confidence intervals and are included in the appendix with unit weights. Additionally, a conventional synthetic control model was estimated experimentally, again without Syrian-origin applications. The estimated treatment coefficient remained positive but statistically insignificant. Results and weights are also included in the appendix.

Table 12: Model (1.1) Robustness Check

	Applications	
	(Controls)	(Without Controls)
TREAT	-88.914	91.995
	(84.362)	(55.652)
POST	95.916*	263.786***
	(44.853)	(31.923)
TREAT:POST	-115.851	83.859
	(77.209)	(78.178)
Belgium	658.125***	1,374.130***
	(115.311)	(48.272)
Netherlands	-142.731*	1,336.800***
	(184.151)	(48.265)
Norway	-80.050**	541.569***
	(90.733)	(48.272)
Portugal	-134.549	-104.552**
	(110.689)	(48.272)
Covid-19	-431.309***	
	(73.373)	
GDP	0.007***	
	(0.002)	
Acceptance Rate	-357.632***	
	(67.985)	
Unemp. Rate(t-1)	-26.002*	
	(6.285)	
Female	-999.038***	
	(141.146)	
Under 18	151.778	
	(139.431)	
Constant	551.286***	127.904**
	(79.062)	(58.855)
Observations	1,045	1,050
R^2	0.745	0.662
Adjusted R^2	0.739	0.656
Residual Std. Error	394.062 (df = 1020)	451.480 (df = 1031)
F Statistic	123.975*** (df = 24; 1020)	112.235*** (df = 18; 1031)
Notes:	***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.	

Table 12: Model (1.2) Robustness Check

	Applications (excl. Syrian)	
	(Controls)	(Without Controls)
TREAT	146.221*	48.173
	(71.123)	(46.307)
POST	109.180**	152.393***
	(37.814)	(26.562)
TREAT:POST	133.079**	196.874***
	(65.093)	(65.051)
Belgium	1,043.332***	1,239.868***
	(97.215)	(40.166)
Netherlands	513.246***	1,025.029***
	(155.251)	(40.161)
Norway	236.862**	437.952***
	(76.494)	(40.166)
Portugal	96.615**	-91.596**
	(58.647)	(40.166)
Covid-19	-406.653***	
	(37.939)	
GDP	0.004**	
	(0.001)	
Acceptance Rate	-394.883***	
	(57.316)	
Unemp. Rate (t-1)	-35.263***	
	(5.299)	
Female	-779.507***	
	(119.001)	
Under 18	156.059	
	(117.552)	
Constant	578.825***	140.225***
	(66.654)	(48.972)
Observations	1,045	1,128
R^2	0.739	0.664
Adjusted R^2	0.733	0.658
Residual Std. Error	332.222 (df = 1020)	375.670 (df = 1031)
F Statistic	120.55*** (df = 25; 1097)	113.225*** (df = 7; 1031)

Notes: ***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

6.2.3 Regression Discontinuity

As discussed in chapter 5, similarity between treatment and control groups is an important assumption underlying the RD framework. Following Gonzalez (2013), tests for balance are conducted across observable covariates in order to make sure this assumption is satisfied. Tests examine the composition of asylum applications in Ireland with respect to country of origin (although some heterogenous treatment effects across origin countries are conceivable) and the main covariates used in models 2.1 and 2.2. Regressions are estimated at the July 2018 threshold across four bandwidths. The regressions follow a basic specification similar to model 2.1, set out below:

(3.1) $$C_m = \alpha + \beta m + \rho D_m + \sigma(m * D) + \varepsilon_m$$

Table 13 displays the treatment dummy coefficients for the tested covariates. Country of origin characteristics appear to be reasonably balanced around the July 2018 threshold. Estimates corresponding to a small reduction in the proportion of DRC-origin applications are consistent across bandwidths. **Figure 16** visualises this discontinuity. Another small, statistically significant discontinuity in the lagged unemployment rate is reported at the 24 month, 18 month and 12 month bandwidths, although not at the 6-year bandwidth. Statistically significant coefficients relating to acceptance rate variable are reported at two of four bandwidths and may be due to interpolation of the data. No statistically significant discontinuities are reported with respect to the total number of EU-27 applications or Nigerian-origin applications.

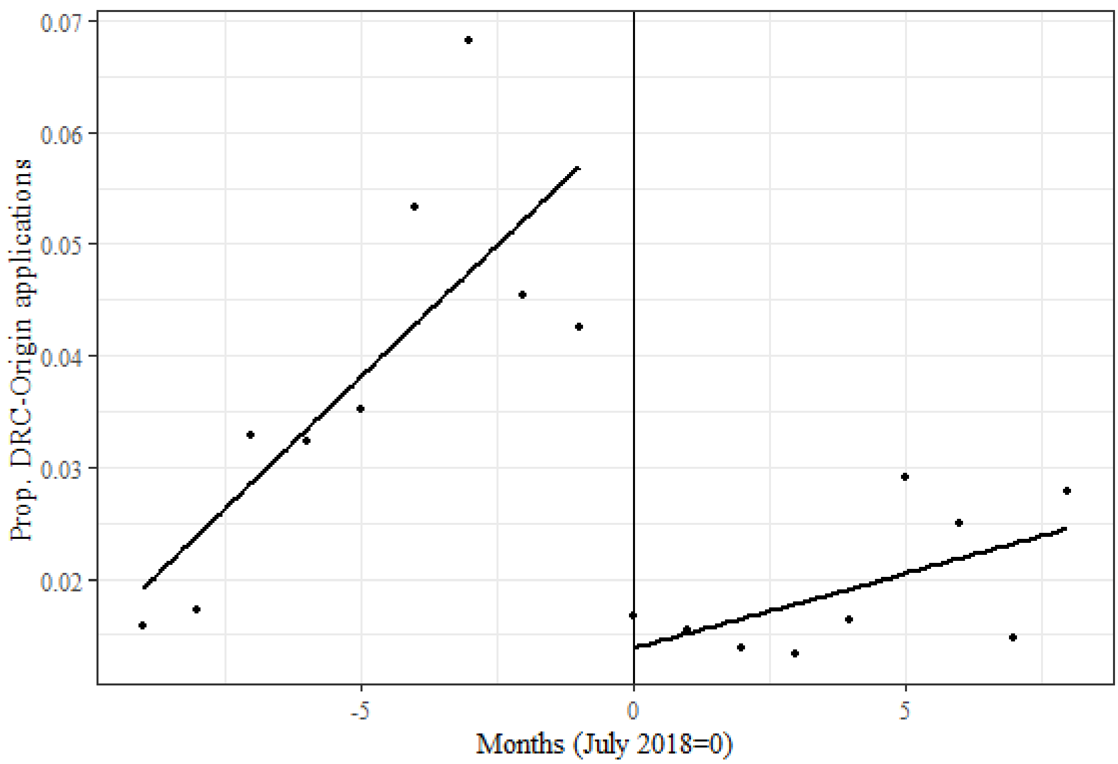

Figure 16: Proportion of DRC-origin applications in Ireland

Table 13: RD Balance Tests

	6 year bandwidth	18 month bandwidth	2 year bandwidth	12 month bandwidth
	(1)	(2)	(3)	(4)
Nigerian Origin	0.040	0.004	-0.009	-0.003
	(0.025)	(0.025)	(0.033)	(0.037)
Georgian Origin	-0.012	-0.016	0.016	0.030
	(0.038)	(0.036)	(0.049)	(0.035)
Algerian Origin	0.009	0.022**	0.012	0.020
	(0.016)	(0.009)	(0.014)	(0.012)
Pakistani Origin	-0.059**	0.024	0.012	0.038
	(0.027)	(0.027)	(0.038)	(0.022)
Somalian Origin	-0.008	0.001	0.001	0.002
	(0.023)	(0.009)	(0.011)	(0.012)
DRC Origin	-0.031**	-0.048***	-0.054***	-0.043***
	(0.013)	(0.008)	(0.017)	(0.011)
Zimbabwe Origin	0.043	0.012	-0.002	-0.003
	(0.032)	(0.025)	(0.025)	(0.025)
South African Origin	0.024	0.007	0.008	0.002
	(0.033)	(0.018)	(0.040)	(0.036)
Female	0.020	-0.003	0.018	0.021
	(0.039)	(0.026)	(0.038)	(0.032)
Applications (tot. EU-27)	8164.82	2136.38	4436.56	5507.57
	(10049.7)	(4926.64)	(4065.67)	(5191.83)
Nigerian Applic. (tot. EU-27)	642.66	-699.26	-391.75	-73.381
	(483.101)	(417.43)	(324.68)	(250.36)
Acceptance Rate	-0.016	-0.112***	-0.065	-0.152***
	(0.031)	(0.040)	(0.036)	(0.025)
Unemployment Rate(t-3)	0.548	-0.507***	-0.452***	-0.650***
	(0.584)	0.190	(0.131)	(0.139)

***Significant at the 1 percent level.**Significant at the 5 percent level. *Significant at the 10 percent level.

7 Discussion

This section will summarise, distil and contextualise the empirical findings described in the previous chapter, in order to assess the main hypotheses. The limitations of the study will also be detailed. The chapter will conclude with discussion on the implications of the results of this paper for theory and policy.

7.1 Summary of Findings

To briefly recap, the main hypotheses of this paper predict that *(1)* labour market access resulted in an increase in asylum applications to Ireland and *(2)* labour market access resulted in an increase in the proportion of working-age applicants in Ireland. With respect to hypothesis 1, the DiD models employed by this paper indicate no statistically significant treatment effect on total applications but a substantial, statistically significant, positive effect when Syrian applications are removed from the data, irrespective of whether observations from 2015 are included. One possible explanation for this, compatible with the expectations of hypothesis 1, is the effect of established migrant stock on inflows (Hatton, 2020). If the control group experienced a long-term increase in Syrian applications relative to Ireland due to the 2015 shock, this could serve to mask the treatment effect associated with the policy reform in question. Figure 8 in Chapter 4, which graphs Syrian-origin applications for the control group and Ireland, suggests that the control group did see a long-term increase in applications relative to Ireland. The results of the regression discontinuity specification, which suggest a small positive short-term treatment effect on total applications in Ireland, further substantiate this explanation. Given these factors, it is suggested that the evidence from these models meets the expectations of hypothesis 1. It is acknowledged, however, that these results alone may not provide a wholly convincing basis for causal inference with respect to the policy reform.

In this regard, the timing of the estimated treatment effect is an important point associated with the results. While the estimates from the Synthetic DiD are statistically insignificant, the model does help to illustrate *when* the bulk of treatment effect actually occurred. Figure 10 shows that the majority of divergence between treatment and control only happened after early 2022, which corresponds both to the end of the Covid-19 pandemic and to the start of the war in Ukraine.[2] As such, the attribution of treatment estimates to the effect of labour market access reform, three years distant, requires considerable scrutiny. It is submitted that there are two main elements necessary to support the idea that the treatment estimate, at least partially, reflects the causal impact of the labour market access reform. Firstly, an explanation as to why the treatment effect might be delayed; the Covid-19 pandemic, which lasted 2 years and flattened trends across the EU, goes a significant way towards explaining why the majority of the treatment effect might be observed post-2021. Secondly, contemporaneous evidence linking the treatment effect to the labour market access reform; here, factors pertaining to hypothesis 2 become relevant.

The results of model 1.3 (Group B), along with visual inspection of Figure 9 in Chapter 4, indicate that the shift in application levels relative to the control group coincided with a shift

[2] As previously mentioned, Ukrainian applicants do not feature substantially in the data due to their status under the Temporary Protection Directive.

in applicant age composition in Ireland. The numbers of applications in the under-18 and 35-65 age categories had remained highly similar, both in terms of trend and magnitude, up until this point. As indicated by the robustness checks, these groups were alone in retaining parallel trends throughout the periods of observations (particularly the 2015 Syrian crisis). The substantial, statistically significant, positive treatment estimate for the number of applicants between the ages of 35-65 is strong evidence for the causal relevance of the labour market access reform, representing a divergence between working age and non-working age categories which were previously highly similar. One (somewhat speculative) explanation for the consistent alignment pre-treatment and subsequent divergence post-treatment of these categories relates to the collective migration of family unit groups. If labour market access attracted more experienced single workers, this might disrupt highly correlated trends driven by the arrival of parent-child dyads. This may be an avenue for further research, given more detailed data on applicant characteristics. Leaving aside the precise mechanism at play, model 1.3 (Group B) is crucial both in terms of hypothesis 2 and with respect to general causal identification. Viewed in tandem with the wider body of evidence, these results provide greater confidence that the labour market access reform is responsible for the treatment effects observed across models.

In summary, the balance of evidence from the empirical findings of this paper is consistent with the main hypotheses concerning labour market access. The results indicate that labour market access increased application levels in Ireland and increased the proportion of working-age applicants in Ireland. Taking the results of model 1.3 and model 1.2 (post-robustness check) (Group B), it is estimated that the labour market access reform resulted in an average of between 45 - 133 additional applications per month in the post-treatment period.

7.2 Limitations

While many limitations have already been implicitly discussed, it is worthwhile to catalogue some of the major limitations of this paper for the purposes of clarity. DiD control and treatment groups do not meet the parallel trends assumption at first instance in most models. While the assumption may have been better met after the removal of specific time periods, this involved an additional degree of manipulation that must be kept in mind when interpreting the results of this paper. Relatedly, the period of observation studied is far from ideal. Not one, but three large crises pertinent to asylum-related migration have occurred in less than a decade; the Syrian crisis, Covid-19 and the invasion of Ukraine. As in any event study, the attribution of causality to the labour market access reform is a matter of argument. Concurrent unobserved time-variant factors are always a potential barrier to inference, particularly with respect to the RD specification. The reform studied was not unanticipated and anticipation effects may have biased results (although the results of RD model 2.1 provide some optimistic evidence in this regard). In general, more detailed cross-country analysis of dyadic origin-destination flows may provide a better basis for inference. This study involves cross-comparison of applications from many origin countries, which renders the results more vulnerable to bias from unobserved push factors.

7.3 Implications for Theory and Policy

The results of this paper suggest that labour market access is a causally relevant determinant of asylum-destination choice, or, in other words, a pull factor. This is in line with the small body of previous scholarship pertaining to the effects of labour market access on asylum inflows, in particular Di Iasio & Wahba (2023). This is also in line with the few quasi-experimental studies investigating the 'welfare magnet hypothesis' in relation to a broader set of socio-economic entitlements (Agersnap et al., 2020; Dellinger & Huber, 2021b). Additionally, the results of this paper suggest that socio-economic entitlements can affect the composition of inflows with respect to the demographic characteristics of applicants, addressing an aspect of the causal relationship between welfare and asylum destination choice that is somewhat neglected by the literature (Razin & Wahba, 2015b). The findings of this paper contribute to the wider debate regarding the impact of relative entitlement levels on asylum inflows, indicating that asylum inflows are responsive to changes in prospective socio-economic entitlements.

The implications of these results for policy should be considered with caution. As discussed in the introduction, recent approaches to the intersection of socio-economic and asylum- policy have often been explicitly or implicitly informed by the aim of deterrence. The value of any deterrent effect associated with restricting labour market access must be squared with a multitude of other factors in the asylum policy matrix. Perhaps foremost are the constraints imposed on policymakers by liberal international and European democratic norms. In an Irish context, for instance, labour market access is constitutionally guaranteed to citizens and non-citizens alike as an irreducible "part of the human personality" (N.V.H v Minister for Justice & Equality and ors, 2017). Attempts to implement restrictions on labour market access risk conflicting with fundamental humanitarian values and democratic convention (Mayblin, 2016b). In more individual terms, exclusion from work has been identified as a significant risk factor for PTSD, anxiety and chronic depression in asylum applicants (Crumlish & Bracken, 2011). Persons exposed to bans often suffer a myriad of long-term consequences, including reduced self-esteem, reduced social functioning and loss of skills (Fleay & Hartley, 2016). At a group level, labour market exclusion has been linked to stunted community development and fewer interstitial social ties (Fleay et al., 2016). In general, exclusion from work has been linked to detrimental impacts across a range of mental and physical well-being outcomes (Hess et al., 2019; Lai et al., 2022).

From a social-utilitarian perspective, access restrictions have been linked to delayed or attenuated integration outcomes for applicants, even after recognition of status. Scholars have shown that asylum applicants that are subject to lengthy bans are less likely to invest in country-specific human capital, limiting their cultural adaptability and integration prospects (Brell et al., 2020).In a similar vein, Kosyakova & Brenzel (2020) find that lengthy bans are associated with significantly delayed investment in language acquisition, a particularly problematic element with respect to the long-term social and economic integration of asylum applicants and refugees. A relatively recent quasi-experimental study by Marbach et al. (2018) found that an additional 7 month wait for labour market access upon arrival outcome was associated with a 20 percentage point reduction in the employment rate of German asylum seekers after 5 years. In a wider context, exclusion from work has been identified as a key component of marginalisation processes resulting in criminality, illegal employment and numerous other negative long-term social and economic externalities (Valenta & Thorshaug, 2013).

These factors suggest that labour market access is a costly, normatively dubious policy tool for deterrence. Nevertheless, the results of this paper do indicate that policymakers should be cognisant of how labour market access interacts with established asylum reception structures. In the Irish case, the direct provision system was originally designed with the total subsistent dependence of applicants in mind. The state maintained strict control over income and consumption, with asylum applicants relying on the system for daily meals, clothing, accommodation, education and healthcare (Loyal & Quilley, 2016). This system remained largely unchanged in the wake of the labour market access reform, despite the fact that some applicants could now enter full-time employment and earn a corresponding wage. As evidenced by the empirical findings, leaving this system unadjusted for higher levels of consumption, or income differentials between working-age and dependent applicants, may have contributed to imbalanced migration incentives.

8 Conclusion

Increases in the magnitude of asylum-inflows following the 2015 Syrian crisis have created logistical and political challenges for destination countries. As a result, the determinants of asylum-oriented migration have come under increasing scrutiny. The impact of socioeconomic entitlements on asylum inflows has been a particular area of focus for researchers and policymakers. This paper has sought to investigate how labour market access influences the magnitude of asylum flows and the demographic composition of applicants. To this end, the paper applied quasi-experimental difference in difference and regression discontinuity methods to exploit Ireland's transposition of the EU Receptions Conditions directive in 2018; a policy reform which ended the country's prohibition on labour market access for asylum applicants.

Using Eurostat data on monthly asylum applications in Ireland and a number of comparator countries, positive and substantial treatment effects were found for non-Syrian origin applications based on difference in difference methods. Positive and substantial treatment effects were found on the number of applicants between the age of 35 and 64, as compared to the number of applicants under the age of 18. Additionally, positive and substantial short-term treatment effects on total applications were found using regression discontinuity methods. These results indicate that labour market access increased the magnitude of asylum flows and led to an increased proportion of working-age applicants in Ireland.

The empirical findings of this paper add to a small body of quasi-experimental literature on the welfare magnet hypothesis and an equally small body of quantitative literature on the relationship between labour market access and asylum inflows. Although quantitative research in this area lacks a comprehensive unified theoretical framework, this paper contributes to the broader body of evidence concerning causal determinants of inflows (or pull factors). For policymakers, these results emphasise that labour market access should be acknowledged and incorporated as a constituent part of the broader reception system; simply implementing labour market access additively may result in imbalanced incentives.

The empirical validity of this research is limited by a difficult period of observation (encompassing the 2015 crisis, Covid-19 and the invasion of Ukraine) which poses challenges for the assumptions underlying the empirical methodologies used and generally complicates the task of causal identification. Validity might be improved by taking a more microanalytical approach with disaggregated origin-destination data, rather than the aggregated metrics used; which are more vulnerable to bias from unobserved time-variant factors. Validity might also be improved by conducting a more extensive examination of changes in applicant characteristics, for instance with respect to education levels and family status. These shortcomings offer interesting avenues for future research; investigation along these lines might help to shed light on the precise causal mechanisms at play and offer more detailed insights into the effects of labour market access on inflows.

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APPENDIX

Table of Contents

Additional Tables: Descriptive Statistics

The following tables display vital statistics on monthly 1st time applications and covariates for each country in the control group used with respect to this paper's conventional DiD specifications.

Table A1: Belgium Descriptive Statistics

	Mean	Standard deviation	Median
1st time applications	1659.654	890.96	1415
Female	0.346	0.041	0.352
Less than 18 years	0.307	0.045	0.305
18 – 34 years	0.481	0.041	0.479
35 – 64 years	0.196	0.019	0.198
Acceptance Rate	0.377	0.133	0.372
Unemployment Rate	7.115	1.219	7.2
GDP (millions)	107789.5	16652.922	104882

Table A2: Netherlands Descriptive Statistics

	Mean	Standard deviation	Median
1st time applications	1656.808	1148.518	1305
Female	0.309	0.068	0.294
Less than 18 years	0.266	0.053	0.254
18 – 34 years	0.507	0.050	0.513
35 – 64 years	0.211	0.037	0.210
Acceptance Rate	0.562	0.171	0.493
Unemployment Rate	5.792	1.531	5.7
GDP (millions)	184744.2	28054.98	174235

Table A3: Norway Descriptive Statistics

	Mean	Standard deviation	Median
1st time applications	707.07	960.614	567.5
Female	0.336	0.068	0.338
Less than 18 years	0.274	0.068	0.265
18 – 34 years	0.516	0.085	0.526
35 – 64 years	0.195	0.055	0.177
Acceptance Rate	0.592	0.178	0.639
Unemployment Rate	3.937	0.667	3.8
GDP (millions)	84521.4755	20529.01	79451.7

Table A4: Portugal Descriptive Statistics

	Mean	Standard deviation	Median
1st time applications	69.813	59.553	52.5
Female	0.303	0.139	0.329
Less than 18 years	0.174	0.119	0.171
18 – 34 years	0.590	0.150	0.588
35 – 64 years	0.229	0.142	0.224
Acceptance Rate	0.477	0.224	0.5
Unemployment Rate	10.652	3.659	10.35
GDP (millions)	48463.9787	6392.795	45983.1

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Table A5: Slovenia Descriptive Statistics

	Mean	Standard deviation	Median
1st time applications	165.505	208.578	40
Female	0.134	0.134	0.098
Less than 18 years	0.244	0.164	0.265
18 – 34 years	0.5738	0.194	0.535
35 – 64 years	0.153	0.128	0.134
Acceptance Rate	0.312	0.2432	0.285
Unemployment Rate	6.650	2.124	6.45
GDP (millions)	10728.5771	1939.997	9883.3

Additional Tables: Monthly Dummies

The following table displays values of monthly dummies for each DiD model. As mentioned in the results section, dummy coefficient values were not reported alongside the rest of the regression results for presentation purposes.

Table A6: Monthly Dummy Coefficients

Model	1.1	1.2	1.1 (Robustness)	1.2 (Robustness)	1.3(Group A)	1.3(Group B)
January	24.020 (94.498)	-0.663 (63.579)	20.935 (68.470)	15.732 (56.973)	-2.145 (15.379)	7.313 (15.246)
February	-22.327 (94.765)	-33.177 (63.759)	-29.633 (68.470)	-19.495 (56.973)	-5.498 (15.380)	7.173 (15.245)
March	-80.583 (94.520)	-48.992 (63.594)	-48.172 (68.468)	-28.832 (56.971)	-2.023 (15.380)	6.708 (15.249)
April	-215.968** (94.459)	-147.992** (63.553)	-133.483* (68.857)	-99.646* (57.295)	-4.347 (15.377)	4.548 (15.247)
May	-152.583 (94.441)	-98.434 (63.540)	-135.478** (68.466)	-94.181* (56.970)	3.960 (15.378)	4.793 (15.366)
June	-201.749** (94.585)	-130.796** (63.637)	-156.330** (68.466)	-107.135* (56.970)	0.663 (15.378)	1.010 (15.365)
July	-108.159 (94.494)	-52.628 (63.577)	-80.223 (68.460)	-39.406 (56.964)	-0.997 (15.377)	3.956 (15.368)
August	-53.979 (94.634)	-7.861 (63.671)	-29.849 (68.855)	-0.375 (57.293)	-0.770 (15.377)	7.137 (15.368)
September	62.903 (94.454)	65.383 (63.549)	11.867 (68.465)	37.689 (56.969)	5.137 (15.438)	6.863 (15.368)
October	83.898 (94.438)	74.750 (63.539)	11.072 (68.465)	24.507 (56.969)	2.298 (15.439)	-4.456 (15.372)
November	68.860 (94.665)	58.957 (63.692)	-1.912 (68.460)	6.782 (56.965)	2.238 (15.439)	1.835 (15.372)

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Note: December excluded as a reference month.

Additional Figures: Synthetic Control Robustness Estimates

The following section reports the results of the synthetic DiD robustness estimates described in chapter 6. Estimates exclude observations from the period 2015 and also exclude applications of Syrian origin. An additional model is also estimated using conventional synthetic control method, for exploratory purposes. Estimates remain positive, substantial and statistically insignificant in all cases. Unit weights are also reported.

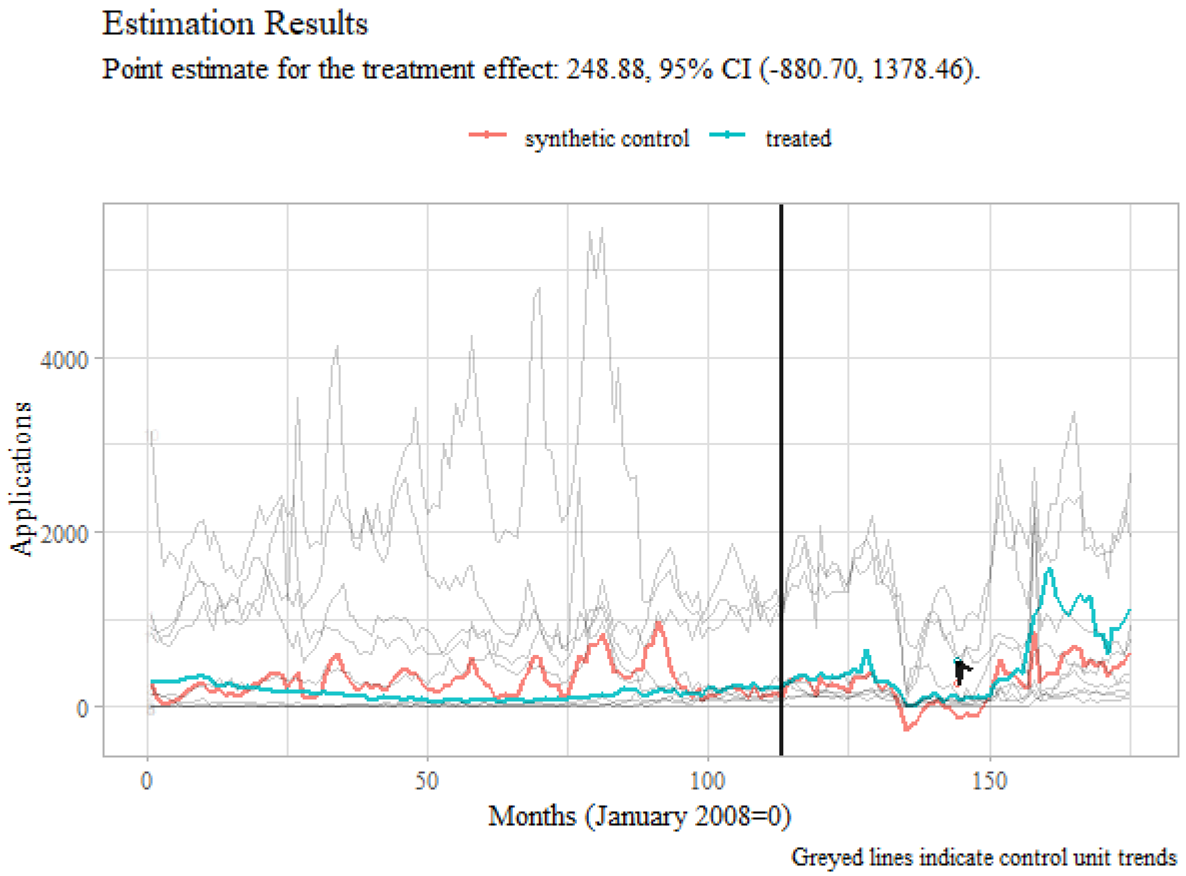

Figure A1: Synthetic DiD Estimate (excl. Syrian) (excl. 2015 period)

Estimation Results

Point estimate for the treatment effect: 270.05, 95% CI (-4099.45, 4639.55).

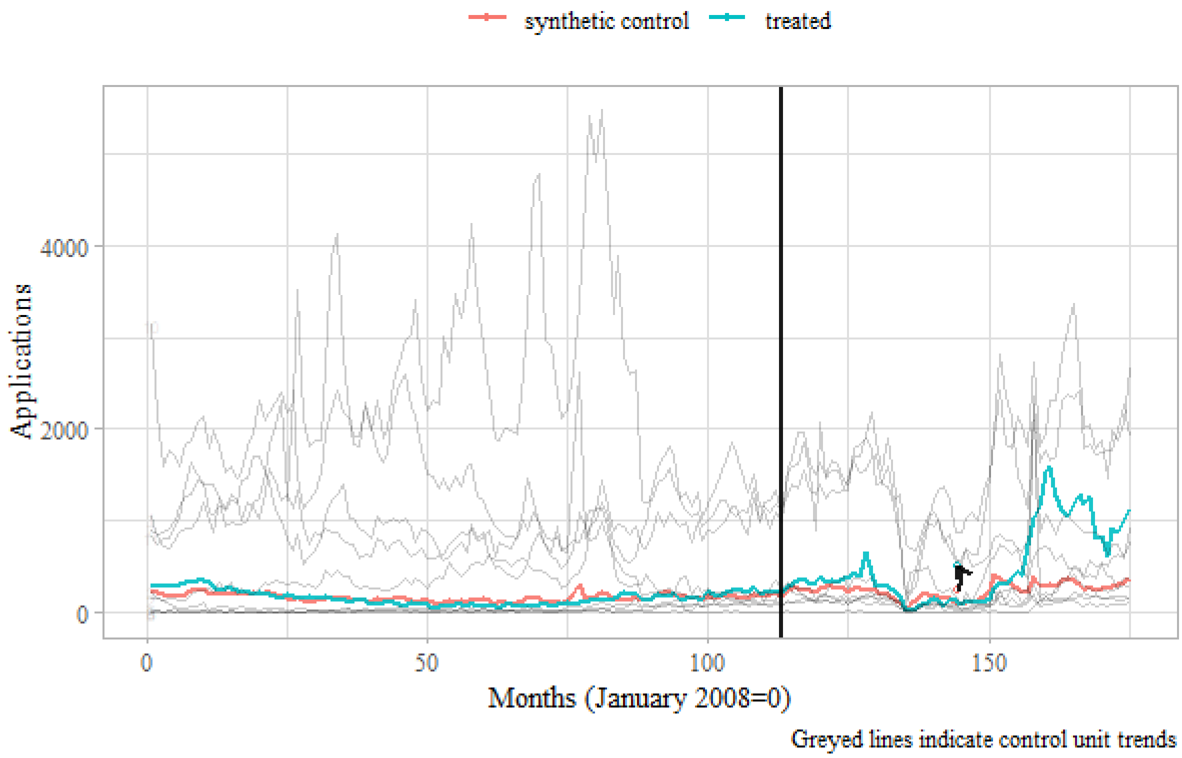

Figure A2: Synthetic Control Method Estimate (excl. Syrian) (excl. 2015 period)

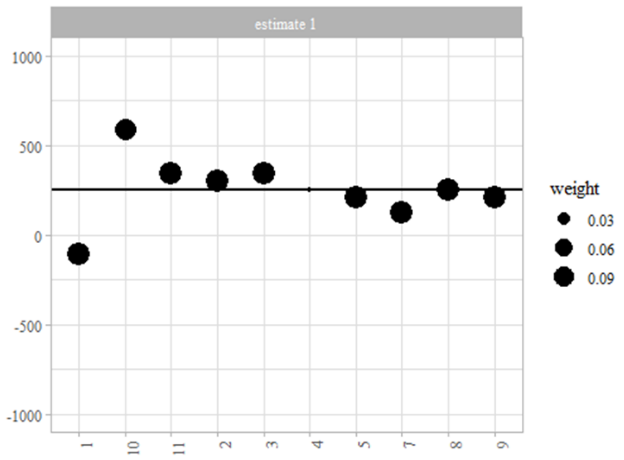

Figure A3: Unit Weights Synthetic DiD Estimate

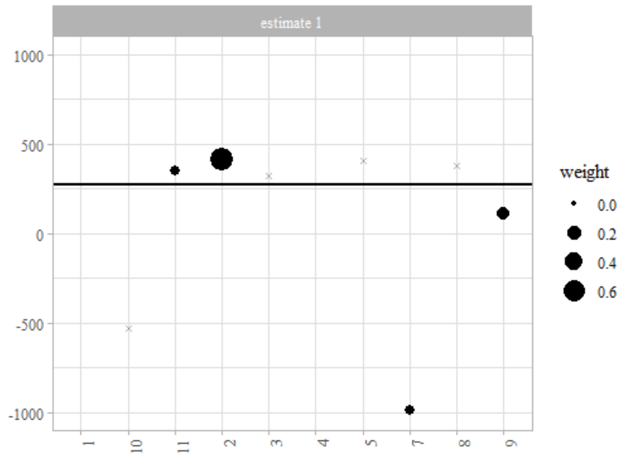

Figure A4: Unit Weights Synthetic Control Method Estimate

Additional Figures: Synthetic Control Unit Countries

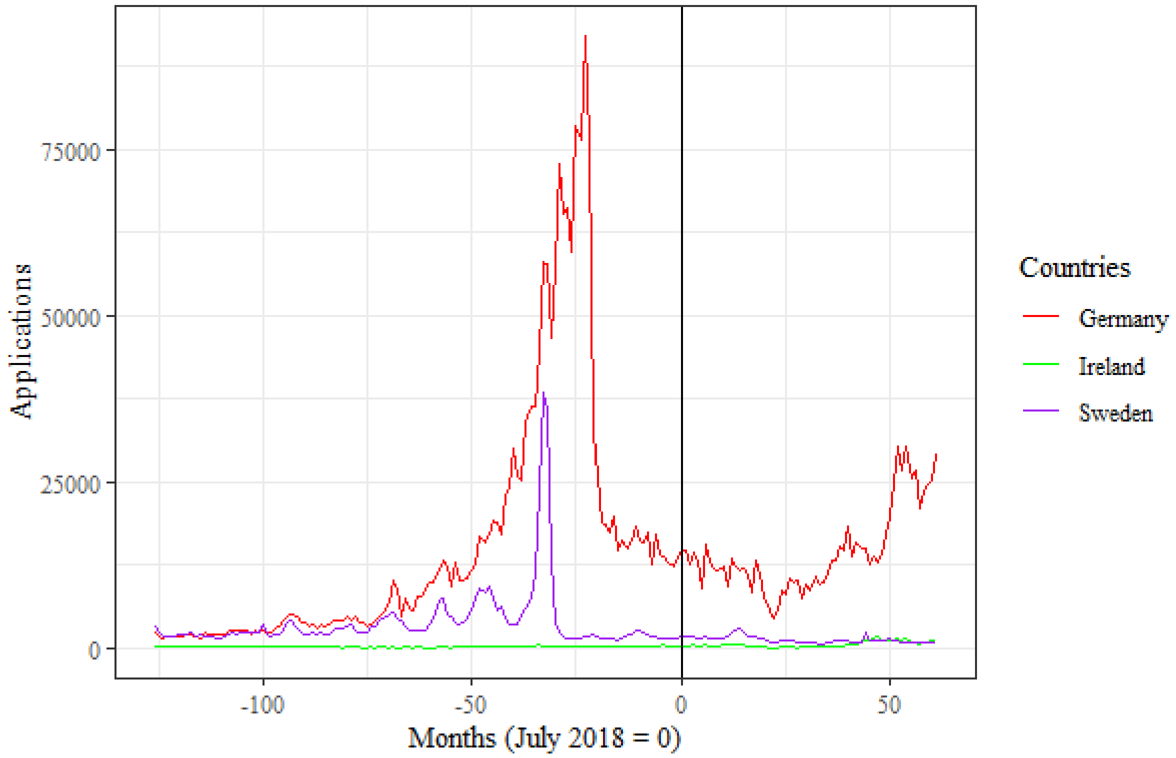

Figure 4A: First-time asylum applications

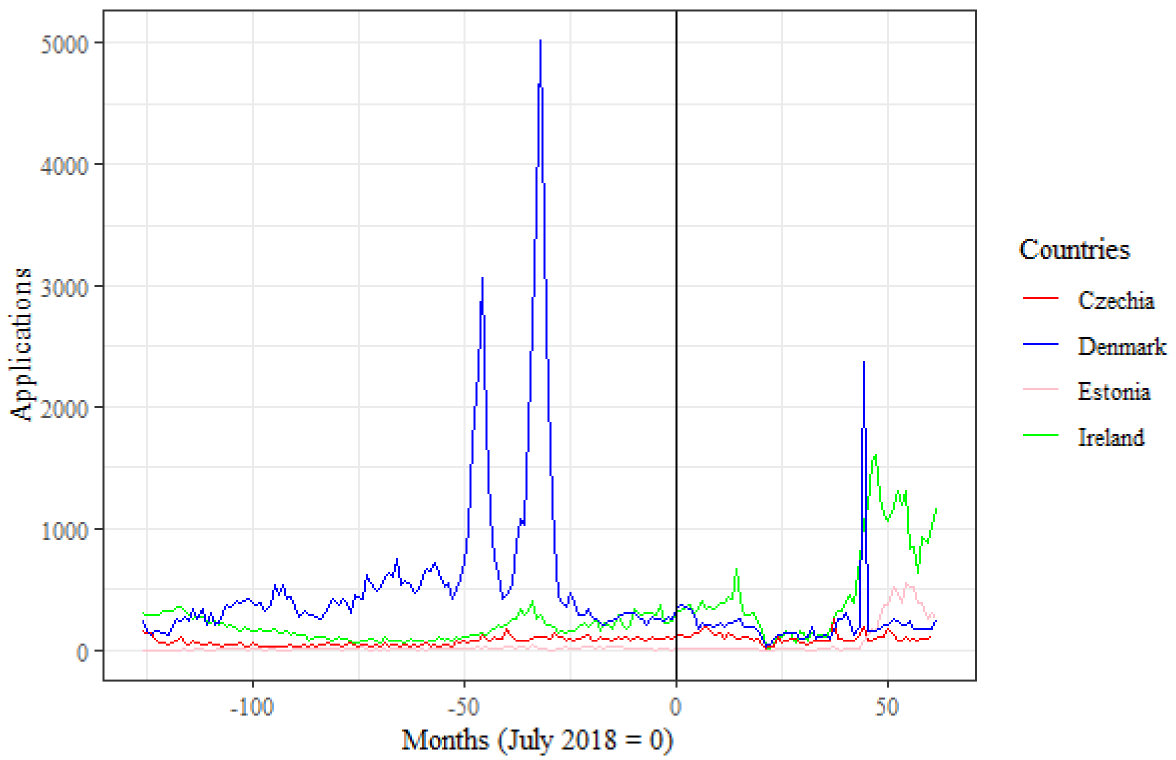

Figure 5A: First-time asylum applications