

# **The Political Consequences of Demographic Change: Empirical Evidence from Migration, Naturalisation and Pension Reform in Britain**



THE LONDON SCHOOL  
OF ECONOMICS AND  
POLITICAL SCIENCE ■

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December 23, 2021

To my parents

## **Declaration**

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

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### **Statement of co-authored work**

I confirm that Chapter 3 was jointly co-authored with Dr Joachim Wehner and I contributed 50% of this work.

Elena C. Pupaza  
December 23, 2021

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## **Abstract**

This thesis monitors the evolution of voting behaviour and public opinion as a function of the socio-economic composition of the electorate. While much research focuses either on public opinion data or electoral outcome data, I bring the two together, yielding a detailed analysis of behavioural and preference changes produced by some of the most fundamental policy decisions implemented over the past three decades in the United Kingdom (EU enlargement, Brexit referendum and pension reforms). Empirically, the thesis combines high quality sources of data from secure panel studies, population censuses, Freedom of Information requests, electoral results and other administrative databases, and applies cutting edge causal inference methodology to reveal how demographic changes in the country have shaped the political sphere. The first paper, “Pension Reform: Electoral Accountability with Time Lags”, finds that voters become and remain disillusioned with politics when policies are implemented, and information is disseminated with a considerable time lag. In the second paper, “Migration: Low-Cost Flights and Far-Right Votes”, Joachim Wehner and I develop an instrumental variable approach and find that the spatial predictability of migrant settlement linked to pre-existing transport infrastructure has a large positive effect on changes in support for far-right anti-immigrant parties. In the last paper, “Naturalisation: Brexit and the Making of New Citizens”, I explore how the Brexit referendum could shape future electoral turnout and outcomes by changing the profile and incentives of the average naturalized citizen. The thesis makes three main contributions. Firstly, it challenges the assumption that voters react immediately when presented with new policies, especially those that have long implementation lags. Secondly, it provides an interdisciplinary approach to understanding how diverse actors respond to demographic changes. Lastly, it encourages the concomitant use of public opinion and observational data to study policy implications.

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# Chapter 1

## Introduction

Many OECD countries are implementing policies aimed at responding to labour market shortages produced by demographic changes. Longer life expectancy and low fertility rates have led to an ageing population crisis across advanced democracies. Two policies have dominated the global response to the crisis: (1) encouraging postponement of retirement, and (2) allowing foreign workers to help the shrinking workforce in the country. This thesis focuses on the social and electoral consequences of policies implemented in response to Britain's aging society. I investigate how the population and the new-comers evaluate these policies and I monitor the evolution of voting behaviour and public opinion as a function of the socio-economic composition of the electorate. While much research focuses either on public opinion data or electoral outcome data, I bring the two together, yielding a detailed analysis of behavioural and preference changes produced by some of the most iconic policy decisions implemented over the past three decades in the United Kingdom (EU enlargement, Brexit referendum and pension reforms). Empirically, the thesis combines high quality sources of data from secure panel studies, population censuses, Freedom of Information requests, electoral results and other administrative data-bases, and applies

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cutting edge causal inference methodology to reveal how demographic changes in the country have shaped the political sphere.

Interventions aimed at creating sustainable socio-economic structures are demanding for voters, incumbents and challenging parties alike. Whether we talk about immigration policies or retirement interventions, their effectiveness and impact on the economy and society can only be measured long after the changes are implemented. Often times, further intervention is required in response to sustainability analyses. In this dynamic, yet slow paced framework, voters are effectively asked to decide between immediate personal sacrifices and better tomorrows for future generations. Simultaneously, policy makers are forced to chose between short term electoral gains and electorally costly policies which will help future electorates. Challenger parties also look at a choice between openly supporting or opposing these policies. These three-dimensional choices, covering both demand and supply issues, are rarely modelled together in existing accountability and policy feedback models. It is no surprise then that the literature is dominated by so many conflicting findings.

In this thesis, I tackle some of the complexities described above, I raise awareness to the gaps in the literature, and reflect on the broader research question of "How does political accountability change when the demographic composition of the population changes?". I answer this question through the lenses of three seminal policy interventions implemented in the United Kingdom over the past three decades. Firstly, I explore how pension reforms implemented to tackle unsustainable policy frameworks related to ageing populations have changed the political preferences of those affected. Secondly, I study how

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the native population has responded politically to immigration policies aimed at supporting the shrinking labour force in the country. Finally, I speculate over the motivations and triggers of political engagement across different migrant groups who join the electorate through naturalisation. The changing society is not only characterised by a restructure in the demographic characteristics of the population, but also by a restructure in our collective expectations for evaluating policy makers (Heller 2003). Each paper draws theoretically driven conclusions about the future of electoral accountability in a changing Britain. Overall, the three papers at the core of this thesis provide answers to the following sub-questions:

1. Can voters hold politicians accountable when policies are implemented and information is disseminated with a considerable time lag?
2. How did “hypermobile” migration affect electoral support for far-right parties?
3. How did the Brexit referendum affect the composition of the foreign-born population with voting rights in the United Kingdom?

Furthermore, the arguments made throughout the manuscript advance our understanding of three broad themes which dominate the narrative. After describing the themes, the remaining paragraphs will put forward a case for the importance of studying the political consequences of demographic changes, followed by a road-map of the entire thesis.

## **1.1 Theme 1: Demographic changes in Britain**

The ageing of the British population is rapidly accelerating, although the trends are mitigated by migration rates. In 2014, the average age in the country exceeded 40 for the first time (Harper 2016). Long before this milestone was reached, policy makers realised

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that the growing life expectancy coupled with low fertility rates will eventually lead to major labour market shortages if a non-interventionist approach is to be pursued. The following decades were dominated by a series of policies meant to design a socio-economic structure that can cope with the rapidly unfolding consequences of an ageing population. The government enabled and incentivised individuals to work for longer in life in order to attenuate the effect of growing numbers of dependents. Furthermore, the so called "Eastern enlargement" of the European Union in 2004 and 2007 provided an opportunity for migrants from these member states to help the UK labour market cope with the demands of increasingly long retirements. At the same time, the liberalization of transportation markets in Europe gave these individuals unprecedented levels of mobility between the country of birth and the UK at affordable rates. This level of mobility sparked debate about the relationship between economic and political integration, as migrants had the opportunity to work in the UK and spend their money in their home countries with their family and friends. Fast forward a decade and these migrants experience a major threat to their freedom of movement: the Brexit referendum. Prior to the referendum on UK's membership of the European Union, many EU migrants never applied for British citizenship due to the high costs and the fact that they were already enjoying similar rights as British-born residents. After the 2016 referendum, many EU migrants decided to buy, together with a British passport, their right to vote in Parliamentary elections. The unprecedented number of applications for citizenship have changed the demographics of the eligible electorate and quite possibly the political behaviour of the new median voter.

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## 1.2 Theme 2: Delayed materialisation of policy outcomes

Retrospective evaluations of policies are often believed to be most applicable soon after implementation because policy feedback models used in political science often assume voter short-sightedness. We are accustomed to definitions of accountability which reflect the public's response in the context of the next election. My work challenges this perception by studying policies which take a long time to filter down through the economy to individual residents. A common basis across the three studies is the delayed materialisation of the policies' outcomes. Each policy, I argue, has an information lag (the time period between its enactment and the notification of those affected) and an implementation lag (the period of time between enactment and the policy coming into force). I use this scheme to observe when people are more vocal about a policy and when, if ever, they are more likely to assign political credit or blame for its (un)successful implementation.

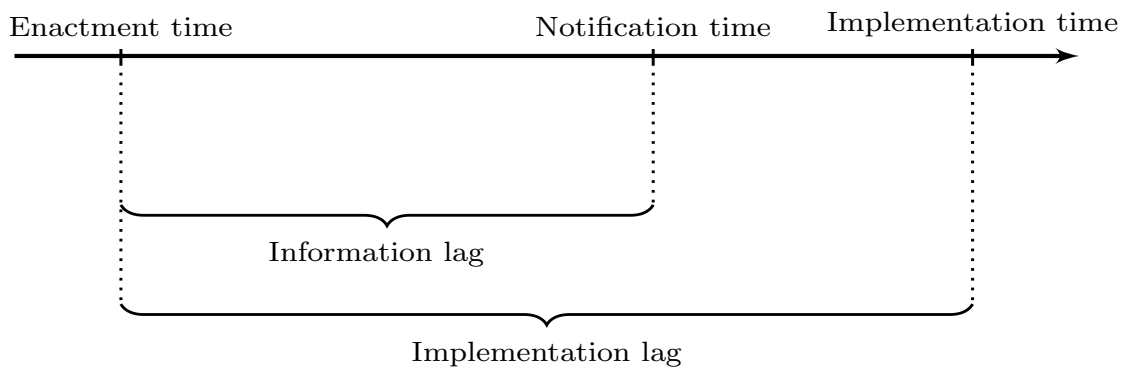


Figure 1.1: Time lags - conceptual model

The thesis encourages greater attention to policy responses across time. More and more policies implemented today target long-range goals. They are slow-paced and their implications only materialise with a considerable time lag, both in the eyes of the public and for the



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economy at large. Measuring the public's political responses to these policies may require accountability models which look beyond the performance of a single administration.

### **1.3 Theme 3: Data linkage and empirical advancements**

Lastly, the thesis encourages the concomitant use of public opinion and administrative data to study policy implications. Understanding the electoral consequences of demographic changes can be challenging if we only look at election outcomes or at individual level evaluations of policies. To have an accurate picture of electoral responses to such policies, public opinion and administrative data need to be merged. While administrative data often misses valuable attitudinal information, survey data can be accused of relying too much on self-reported statistics. By combining the two sources of data, one can gain a more robust understanding of how individual voters respond attitudinally and behaviourally to policy reforms.

The linkage of different data sources provides new research opportunities and a much wider scope for robust and relevant policy evaluations than it would be possible if these data-sets were used in isolation. Data linkage is the act of connecting separate sources of data by identifying and matching the same entity in each source and then bringing the different sources of information together into a single dataset. The linkage can refer to matching records held about an individual or a geographic area in two separate places. This technique will be used across the three papers to explore new research avenues and revise old research with new data and tools. Working with previously inaccessible data such as Freedom of Information data from the Home Office, commissioned Census data

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from the Office for National Statistics, secure level panel survey data from the UK Data Service has enabled the discovery of new behavioural patterns in an ageing Britain.

## **1.4 Why study the political consequences of demographic change?**

Two reasons drive the importance of studying political consequences of demographic changes. Firstly, policies addressing changing demographics have become more prevalent both in the United Kingdom and in other countries around the world. Given the growing salience of the topic, political behaviour scholars have not contributed to the debate as much as other fields have. This is partially driven by the fact that voters are still seen in the literature as short-sighted and incapable of evaluating policies on an ongoing and long-term basis. Therefore, if no meaningful responses to a policy are reported in surveys or reflected in the electoral outcomes at the first election after enactment, the long-term effects are rarely revisited later on. This leads to the second reason why studying demographic shifts is crucial. Policy areas such as this one, whose consequences take a long time to unfold are overlooked in the political behaviour literature. It is therefore recommended that scholars and policy makers ponder the role of time lags in responses to demographic policy changes. Furthermore, changing demographics stand for other long-ranging and slow-paced changes that impact our societies nowadays. To name just a few, they can provide insights into public opinion on climate change, natural disaster preparatory measures, national security or cyber security. Therefore, the findings can inspire other policy areas which function under similar premises and with equally long time horizons.

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Changes in the composition of the electorate (or the composition of the wider population) have major implications for political elites who become accountable to a new citizenry, and for existing members of society who need to readjust to a different social composition of their neighbourhoods. Citizens will observe how policy makers handle the social changes and will sooner or later hold them accountable. Accountability is a central concept in the political behaviour field and political scientists care deeply about understanding it. However, it is often seen as a process which manifests itself in the voting booth, in the context of the next election. When studying political responses to demographic changes, this approach may be misleading. Since societal changes are slow in nature, so can the accountability process be. For this reason, this collection of essays focuses on political accountability first and foremost to show that it sometimes is a slow process. The three papers encourage scholars to explore longer time horizons of retrospective voting, as voters are not always myopic or short-sighted.

Furthermore, demographic changes are present in most developed democracies and pose incredible challenges to the sustainability of welfare states. The accelerated rate at which the ageing population problems advance is alarming for policy makers, scholars and ordinary citizens alike. While the efficiency and sustainability of the reforms implemented are often discussed, the political behaviour field did not contribute significantly to the debate so far. Understanding how policies aimed at addressing aging population issues affect voters' preferences is essential for developing better policies, improving the communication channels with the citizenry, and learning how to manage their unintended consequences. These challenges translate to other policy areas, allowing the development of new accountability models which involve long-term or slowly evolving solutions.

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## 1.5 Road map

The next three chapters correspond to three papers assessing the effect of these policies on ordinary voters, and how these policies have shaped the social and political scene in the country. The first paper focuses on the electoral consequences of increasing the minimum state pension age - the earliest age at which a pension can be claimed from the state. Since the introduction of the first state pension in 1909, six amendments have been made to the minimum retirement age for men and women (in 1925, 1940, 1995, 2007, 2011 and 2014). The frequency of the interventions increased in recent years, being mainly driven by the accelerated rate at which the ageing population problem has been advancing. Chapter 2 focuses on the 1995 Pension Act, the first time in the post-war era when the state pension age was increased. The reform targeted women born after 5 April 1950 and was the first stage of a 36 years project meant to gradually raise retirement age to 68 for everyone by 2046. A few studies have emerged indicating that the interventions have had a positive impact on the economy by increasing the employment rate among the older population (Cribb, Emmerson and Tetlow 2013), although there is no evidence to suggest that it had any impact on affected women's political attitudes and, more broadly, on the accountability mechanisms. In spite of changes in State Pension age being perceived as unpopular among those directly affected (Macnicol 2015), so far, no obvious electoral cost has been associated with these changes (Vickerstaff and Loretto 2017). My research contributes to this debate by providing a new explanation for the lack of electoral costs - the role of time lags. These reforms have been implemented by both Labour and Conservative led administration. Furthermore, their consequences only materialise long after the party leaders leave office. The time lags make the cognitive process of assigning credit or blame challenging even for the most informed voters. Setting expectations for policy feedback

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mechanisms under these circumstances, as discussed in Chapter 2, is a real gap in the political behaviour literature (Jacobs and Matthews 2012).

The second paper focuses on the electoral consequences of changes in the composition and settlement patterns of Eastern European residents across 620 wards in London. It examines patterns of migration and far-right voting in London following the Eastern enlargement of the European Union. Czech Republic, Estonia, Poland, Hungary, Latvia, Lithuania, Slovakia and Slovenia joined the European Union in 2004, followed by Romania and Bulgaria in 2007. We show that the distribution of migrants from this region is linked to pre-existing transport infrastructure providing access to low-cost flights back home. In turn, the settlement patterns have shaped the electoral geography of support for far-right parties. Combining ward-level election and census data with geo-referenced information on bus stops serving relevant airports, our instrumental variable approach addresses immigrant sorting and reveals a large positive effect on changes in support for far-right anti-immigrant parties between the 2004 and 2012 London elections. The findings challenge claims of low-cost airlines “bringing the EU closer together” and show how mobility affected the demographic composition and political preference of communities that are left behind. Chapter 3 discusses the unintended political side-effects of low-cost travel and expands the methodological toolkit for studying the effects of migration.

The third paper discusses how the Brexit referendum has pushed unprecedented numbers of EU-born residents to apply for British citizenship. The referendum campaign painted an unflattering and often misguided image of EU-born residents living in the UK. In reality, EU citizens are more likely to move to the UK for work than non-EU citizens and

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have higher employment rates (81%) than both UK born citizens (75%) and non-EU born migrants(57%) (Vargas-Silva and Fernandez-Reino 2018). Migrants from the European Economic Area (EEA) have constantly made positive fiscal contributions, even during times of budget deficits when both natives and non-EEA migrants were making negative contributions (Dustmann and Frattini 2014). Yet, they have been accused during the campaign of being a burden for the existing infrastructure like schooling, housing and health care. Their rights and liberties were uncertain after the transition period, which led to a mass naturalisation. With citizenship comes not only freedom of movement, but also the right to have a say in national politics. If the new citizens become active voters, the next elections could look very different for many political parties who have never targeted this previously unenfranchised group. Studying the political socialisation trajectories of individuals who join the electorate by naturalisation is as important studying young, first-time voters if we are to avoid losing touch with the new generation of voters (Wüst 2004). Chapter 4 will identify the most prevalent socio-economic characteristics of successful applicants for British citizenship and connect them with the corresponding voting behaviour literature in order to develop hypothesis about their participation in future elections and their political preferences.

Chapter 5 will conclude the thesis by discussing the broader implications of the findings, and outlining areas for future research.

## **Chapter 2**

### **Pension Reform: Electoral**

### **Accountability with Time Lags**

The voting behaviour literature suggests that, where policies have short term costs, voters already have a built-in accountability mechanism for punishing policy makers (Downs 1973). That mechanism is usually the ballot box. If voters do not like policies pursued by the incumbent, they can retaliate at the polls (Fiorina 1981; Wlezien 1995). Less clear is what happens if there is a considerable time lag between the enactment of a policy and its consequences. In a framework where policy effects filter down to the electorate long after the incumbent who implemented the law leaves office, the paper sets forth to get empirical purchase on a question with broad theoretical and practical implications - How do voters react when the costs of a policy materialise under delayed circumstances, particularly when information about the policy is scarce?

The 1995 State Pension reform act is used as a case study to answer this question. The reform increased the minimum state pension age for certain women from 60 to 65. Al-

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though the policy was enacted in 1995, the actual effects did not materialise until more than 15 years later. The study advances and tests three different hypotheses to report voter evaluation of the policy at different points in time. The aim of the paper is therefore to identify whether affected voters are more likely than unaffected individuals to change their political preferences towards (1) the incumbent who enacted the law (incumbent effect); (2) the incumbent who notifies them about the change in policy (incumbent effect); or (3) become disengaged with politics in general (vote abstention). Separate theoretical expectations are set out for the mechanisms behind when and why we would expect reforms to government spending programs to affect voters.

To test these hypotheses, the study uses longitudinal household panel survey data from the British Household Panel Survey and Understanding Society. The paper draws on highly sensitive individual level data to offer a unique window into the partisan attitudes of individuals affected by the reform. The assignment to treatment is calculated based on legal and administrative data released by the government. I use a difference in differences design which exploits the variation in political preferences before and after (a) the implementation of the pension policy (1995) and (b) the time when voters receive written notification (2009-2011) of the policy change affecting their ability to claim State Pension. The paper finds mixed support for the incumbent effect. Interestingly however, the effects are specific to the enactment period and do not persist across time. Affected voters' opinions of the incumbent do not change even upon receipt of a personalised letter sent 15 years on, detailing the policy change that will take effect in a year's time. The only exception is the strong support for the abstention hypothesis, which persists even decades after the implementation of the policy. Affected women are significantly more



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likely to declare they would not support any party in an upcoming election. The effects are large and significant both in the immediate aftermath of the reform and at notification time. Further analysis suggests that this is an individual and highly personal treatment, as no evidence of household level spillover effects are found.

The paper makes three main contributions. Firstly, it addresses how political systems deal with long-term policy challenges such as demographic changes. Understanding how policy changes are best communicated to the electorate without alienating them and how voters respond to the trade off between relatively small immediate losses and long-term gains for future generation is one of the biggest challenges of our time. This paper adds to the yet scarce contribution of the political behaviour field to this ongoing debate. Secondly, it analyses if, when and why substantial time lags affect voters' ability to assign credit or blame. In doing so, it proposes a revision of the existing accountability models, such that they become more suitable to operate in a framework of long-spanning and slow-moving policy changes. Lastly, from a policy perspective, one implication of the research findings is that communication with the public needs to be revised. In an era where unsustainable policy frameworks need reforming (population ageing, climate change and technological changes), policy makers need to be better equipped to balance short-term electoral gains and long-term economic benefits. The paper exposes dissemination mistakes made by several administrations in the United Kingdom in order to invite better communication practices. It challenges the misconception that voters are myopic and they will "punish the bearer of bad news".

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I start with an overview of the established theoretical links between time and accountability and continue with a background description of the pension reform, followed by a presentation of the data sets and empirical strategy. The results and discussion will then conclude the work.

## **2.1 Time lags and accountability**

Long-ranging and slow-moving policies have dominated the political agenda of recent governments worldwide. Climate change, resource scarcity, natural-disaster preparedness, ageing populations and pension burdens are just a few areas in which policy makers were forced to implement such long-spanning policies. Some of the economic implications of these reforms are felt immediately by voters, while others are more subtle, and their social and economic impact takes decades to fully materialise. The electorate will undoubtedly have different political responses to different implementation and materialisation approaches. Yet, the electoral behaviour literature has only recently begun to examine the role of time in voters' responses to these diverse policies.

Whenever electorally damaging policies have to be implemented and they are expected to produce significant changes in voting behaviour, politicians can respond strategically. Some of the most commonly cited strategies are: providing other types of information to counteract the negative reputation (a type of "transparency intervention"), increasing campaigning efforts, or offering compensation packages (Pierson 1994). This paper acknowledges the existence of these other tactics and addresses the potential endogeneity problems they pose on the current research design. However, the main aim of the research is to explain the role of another technique in the accountability process - time lags. I

argue that implementation lags and information lags have been the main diversion tactics employed by politicians to counteract the potentially damaging electoral consequences of the 1995 pension reform.

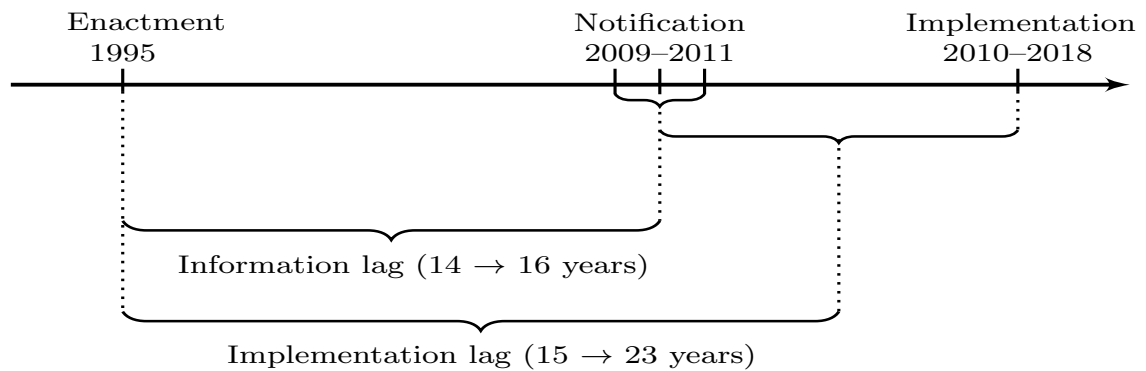


Figure 2.1: Time lags - conceptual model

Both implementation and information lags<sup>1</sup> are common practices in any policy cycle. Due to time and resource restrictions, democratic governments may decide to engage in “retrospective transparency”. This technique allows political administrations to carry out policy interventions without outside interruption, and to then release policy updates at regular intervals (Grimmelikhuijsen 2012). Moreover, policies have an endogenous lag caused by a desire to acquire more information and adjust the costs accordingly. Most complex policies are adopted with a gradual implementation strategy that allows for adjustments in response to the economic effect. Ignoring these lags and assuming that the policy always starts affecting voters on the date the law is enacted leads to theoretically spurious conclusions and empirically biased estimates of policy effects (Elmendorf 2009).

<sup>1</sup>The implementation lag refers to the time period from when the policy is passed (enactment time) and the time it is put into place (implementation time). In this case study, the implementation of the 1995 pension reform had an average lag of 19 years. The information lag refers to the temporal distance between the enactment time and the time when affected individuals are notified about the policy change (notification time). In this particular case, it spanned 15 years on average.

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When the lags cover multiple administrations, governments effectively pass the responsibility to future administrations. This technique complicates the policy feedback process to a point that it overwhelms even the most politically informed citizens (Marsh and Tilley 2010). There is little information available about how the length of the deferral impacts the public reaction to that policy. Do affected citizens have an effective policy process memory and correctly assign credit or blame to those truly responsible for the decision? Or do they channel their evaluation towards “the messenger” (the party which delivers the news or draws their attention to the policy)? Can considerable time lags also lead to an erosion in democratic trust, manifested through negative voting, protest voting or low turnout? I start my quest for answers to these questions by summarising how time lags fit within different accountability frameworks and develop a series of hypotheses grounded in these models. The hypotheses will lay out the theoretical expectations as to *when* and *why* we would expect voters to respond to such policies and tactics.

## **2.2 Responses to slow-moving policies**

Countless theoretical models centred around the role of the people in the accountability process have been developed: both prospective and retrospective; sociotropic or egocentric; arguing for or against the rationality of the median voter (e.g. Schillemans 2013: a cross-discipline meta-analysis). Other models have zoomed in on the importance of information acquisition on voters’ ability to reward or sanction politicians (de Benedictis-Kessner 2018; Dunning et al. 2019) or on the policy’s temporal proximity to election (Wlezien 2015). However, none of these studies tackles responses to time lags which extend beyond a single administration.

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In order to understand how the public responds to distant time-horizons, one would first need to understand the general public mood towards the proposed policy. The raw reaction is oftentimes best captured at the enactment time because there are no lags to complicate the cognitive process at this stage. Education and level of interest in politics are used as proxies for treatment awareness. Holding these awareness indices constant, one would expect that affected individuals will use prospective accountability mechanisms at this stage to signal their (dis)satisfaction if the policy is salient enough. The direction of the effect will depend on the perceived value of the policy for each individual. If the policy is generally seen as a positive change, affected individuals will be more likely to see the incumbent more favourably than their otherwise similar, unaffected peers. If those affected disagree with the policy, their attitudes towards the incumbent would become, on average, more negative than those of unaffected individuals. However, no significant changes in attitudes towards the incumbent could indicate either an indifference towards the policy or political polarisation.

Once the basic sentiments towards the policy are captured at enactment time, it is possible to move on to understanding the possible reactions to distant time-horizons. In this endeavour, I draw from several schools of thought and set forth my theoretical expectations.

The first school of thought would argue that time lags, irrespective of their length, are irrelevant in the feedback mechanism because preexisting preferences are more indicative of voting behaviour than inter-temporal decision-making processes (Glaeser and Sunstein 2013; Grimmelikhuijsen 2012). The need to hold consistent beliefs impedes voters from changing their opinion in response to policy predicaments. Therefore, those whose fun-

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damental political beliefs are at odds with the key message of a policy will dismiss it as wrong or interpret it in a way that resonates with their ideology, irrespective of when they get introduced to its effects and consequences. At the same time, those whose beliefs align with the information received will use that message to reinforce their existing convictions (Festinger 1957). Based on this, I hypothesise that affected individuals will respond the same way to the policy at notification time as they did at enactment time.

A different strand of literature which reaches the same conclusion of temporal irrelevance explains the outcome through voter myopia. Concerns have been raised about voters' ability to evaluate governments' policy choices made early on in their term in office, let alone policy choices made by previous administrations (Wlezien 2015). However, some studies find no evidence to support the theory (Healy, Persson and Snowberg 2017), which may indicate that this behaviour is context-dependent. Among those studies that report a clear and significant effect that voters discard past events in favour of more recent ones, it remains unclear whether the effects stem from the fact that voters prefer short-term expensive economic responses over long-term preventive measures (Healy and Malhotra 2009), because it takes time for the economic outcomes of a policy to filter through to the voter's pocket (Erikson 1989) or because of the uncertainty brought by long-run processes (Healy and Malhotra 2009). Drawing upon this strand of literature, I hypothesise that affected individuals will not respond any differently from their unaffected peers to the notification treatment, as their myopia will impede them from assigning any credit or blame to an event that took place decades prior. In other words, any effects detected at implementation time will dissipate completely by enactment time. If proven to be true, voter myopia has vast implications for accountability, as it enables policy makers to use

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time lags to manipulate the economy without any electoral repercussions (Healy, Persson and Snowberg 2017).

A third body of literature suggests that citizens are capable of and indeed do take into account temporal features of policies when forming political judgments. For example, Jacobs and Matthews (N.d.: 3) argue that most of the electorate possesses a basic form of responsibility reasoning, with more sophisticated lines of reasoning being limited to the politically knowledgeable and engaged. Such reasoning would allow citizens to assign credit or blame, even if the process is not as straight-forward as judging a single incumbent's performance in office. In this case, the electoral consequences can vary depending on the type of voting individuals engage in. For example, those who vote with their pocketbook will assign credit or blame based on the way in which the new policy affects them and their personal finances. Those who vote sociotropically, however, will consider the broader benefits of the policy to society and even future generations (Kramer 1983). It is possible that these two accountability methods will not only lead to different electoral decisions, but will also respond to time lags differently. Such accountability models rely heavily on the existence of a clear and transparent channel which allows voters to assign credit or blame (de Benedictis-Kessner 2018). In its most simple form, accountability in response to a specific policy requires the electorate to connect their basic ideological preferences to the policy, and to understand the political parties' positions in relation to that policy. When information is scarce, the ability to connect the various ideological positions is hindered (Gingrich 2014). In such circumstances, incentive-driven responses to policies (both sociotropic and egocentric) may be better captured at notification, when the role of

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information become clearer and the economic impacts start materialising.

I would therefore expect that if the majority of affected individuals vote egotropically, their views will deviate the most from their peers at notification time, when the effect of the policy reaches their pockets. On the other hand, if the majority of affected voters engage in sociotropic accountability, their political attitudes would crystallize long after implementation, as more information becomes available and the costs and benefits to society become more obvious. This process may extend beyond the notification time, in which case it will not be captured with the existing data. Furthermore, preferences do not need to be consistent across time, as new evidence and further information could reveal previously unaccounted for effects, which can in turn sway voting decisions at notification time and beyond.

The direction of the effect will also reveal important information about voters' cognitive behaviour. Do voters remember the party in power at enactment time decades later and assign credit or blame accordingly? If so, they are not as myopic as a large part of the literature suggests. Alternatively, do they punish or reward the notification incumbent? If so, this may be a sign of pocketbook voting. A third alternative is that affected individuals become politically disengaged in response to such complex policy interventions. If this is the case, it may be indicative of a personality driven response. Some studies have shown that those with a weaker sense of control over their fates are more likely to blame the political system for the challenges they face in their lives (Baird and Wolak 2021). To summarise, irrespective of the mechanism used to assign credit or blame, our expectations of when and why voters respond to welfare reforms need revising in order to account



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for the slow nature of such government spending programs. To start understanding how accountability looks like in these circumstances, the current paper uses the 1995 pension reform in the United Kingdom, which will be introduced in the next section, to study how voters respond to extended time lags.

## **2.3 Background: State Pension reform in the UK**

The focus of the paper is the 1995 Pensions Act, promulgated by John Major's Conservative Cabinet. This reform is only the first in a series of similar policies adjusting the minimum age at which people can claim State Pension. At its core, the policy aims to design a socio-economic structure that can cope with the rapidly unfolding consequences of an ageing population. It proposed measures for equalisation of the State Pension Age (SPA) between sexes by increasing women's SPA from 60 to 65 between 2010 and 2020 (ONS 2011). What is particularly striking about the case is that administration after administration (led by both Labour and Conservatives governments) postponed informing affected individuals about these changes.

The incumbent at enactment time (1995) was the Conservative party led by John Major. In 2011 however, the Coalition government led by David Cameron realised that the minimum State Pension Age is not increasing fast enough and enacted a new reform to firstly speed up the SPA equalisation between the sexes, and then to raise everyone's SPA from 65 to 66 years. See Figure A.4 in Appendix A.1 for a visual representation of the two treatments and the extent to which they overlap. This additional treatment complicates measuring the true effect of the first reform. For this reason, the pure impact of the 1995 PA can only be studied on a small group of women (born between 6 April 1950 and 5th of April 1953)

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who did not receive a further treatment in 2011. The notification letters were sent by two different administrations, such that women born before 6 April 1952 were notified between 2009 and 2010 by Gordon Brown's Labour government, while women born on and after this date were notified between 2011 and 2013 by David Cameron's Coalition Government (formed of Conservatives and the Liberal Democrats).

With such long time lags, the unavoidable question is "how many affected women were aware of the changes in their minimum state pension age before being directly notified?". Official reports on the handling of the policy describe the unknown impact and its consequences:

*Successive governments have bungled the fundamental duty to tell women of these major changes to when they can expect their state pension. Retirement expectations have been smashed as some women have only been told a couple of years before the date they expected to retire that no such retirement pension is now available. (Frank Field, Work and Pensions Select Committee, 11 January 2016).<sup>2</sup>*

An investigation into the possible avenues through which those affected could have gained more information about the policy suggests that most formal channels overlooked the 1995 Pension Reform until recently. The policy did not receive a lot of media coverage outside of business or finance pages of a few newspapers, and political parties did not campaign either for or against it with the exception of two non-mainstream parties.<sup>3</sup> The legislation only became a high profile case in 2019 when two campaigners started a significant legal battle against the Department of Work and Pensions. Although they lost, their campaign brought increased awareness on the government's handling of the rise in women's SPA.

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<sup>2</sup>Further reports reach similar conclusions: *Some of the notification letters reached their recipients so shortly prior to their original retirement age that they were left unable to make alternative retirement plans.* (House of Commons report number HC 899/15 March 2016)

<sup>3</sup>See Appendix A.2 for further details.

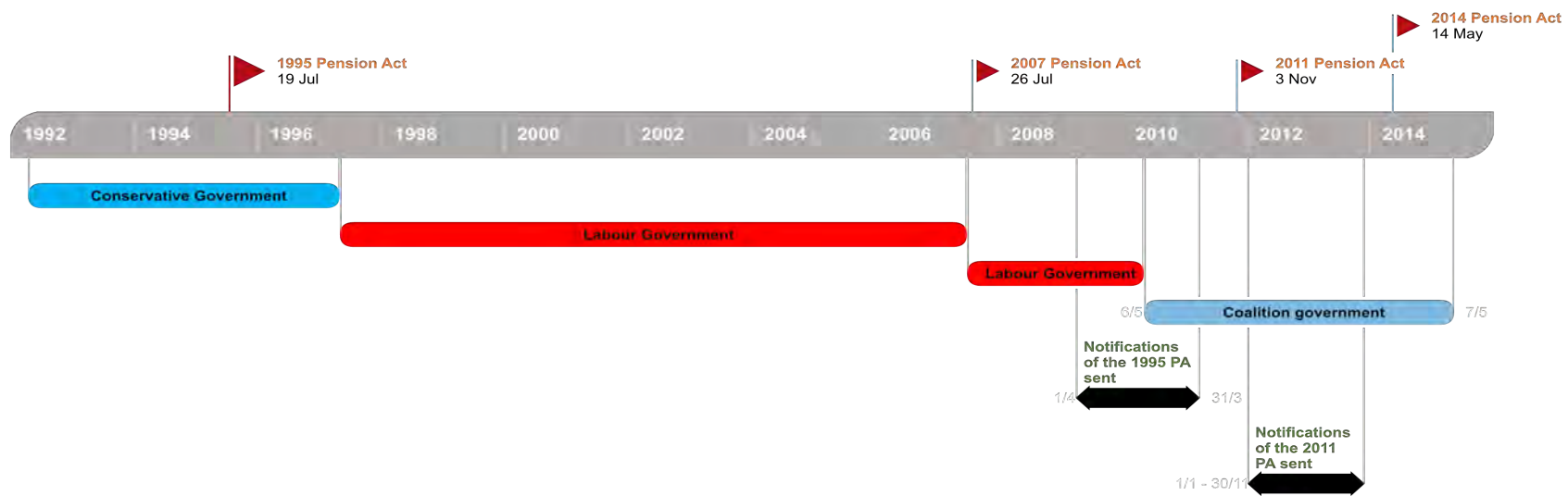


Figure 2.2: Timeline for the 1995 and 2011 Pension Acts - from enactment to notification

Two consecutive administrations sent approximately seven million letters between April 2009 and February 2012 to those affected by changes in State Pension age implemented back in 1995. This unprecedented dissemination exercise provides a unique opportunity to investigate how affected individuals respond not only to the policy, but also to the information treatment. Due to a freedom of information request, we know almost exactly when each of the affected individuals was notified (detailed in Figure 2.3).

<i>Date of birth</i>			<i>Date notified</i>	<i>Number of letters sent</i>
<i>6 Apr 1950</i>	<i>to</i>	<i>5 Jul 1950</i>	April 2009	99,985
<i>6 Jul 1950</i>	<i>to</i>	<i>5 Oct 1950</i>	Jul 2009	96,356
<i>6 Oct 1950</i>	<i>to</i>	<i>5 Apr 1951</i>	Oct 2009	191,465
<i>6 Apr 1951</i>	<i>to</i>	<i>5 Oct 1951</i>	Jan 2010	196,189
<i>6 Oct 1951</i>	<i>to</i>	<i>5 Apr 1952</i>	Apr 2010	188,515
<i>6 Apr 1952</i>	<i>to</i>	<i>5 Oct 1952</i>	Feb 2011	196,594
<i>6 Oct 1952</i>	<i>to</i>	<i>5 Apr 1953</i>	Mar 2011	191,665
<i>6 Apr 1953</i>	<i>to</i>	<i>5 Dec 1953</i>	Jan 2012	275,000 <sup>1</sup>
<i>6 Dec 1953</i>	<i>to</i>	<i>5 Oct 1954</i>	Feb 2012	646,000 <sup>1</sup>
<i>6 Oct 1954</i>	<i>to</i>	<i>5 Apr 1955</i>	Feb 2012	375,000 <sup>1</sup>
<i>6 Apr 1955</i>	<i>to</i>	<i>5 Apr 1960</i>	Oct 2012 - Nov 2013	4,475,000 <sup>1</sup>

Figure 2.3: Department of Work and Pensions, FOI request, Sept 2017

## 2.4 Data and empirical strategy

In order to estimate the effect of the enactment and the notification treatments, the current paper uses data from two longitudinal household panel surveys and combines it with administrative data detailing the discontinuities in State Pension age (Figure A.3 in Appendix A.1), as well as Freedom of Information data monitoring notification dates for each affected cohort (Figure 2.3). Linking the various data sources is done based on gender and full date of birth.

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Two successive nationally-representative household panels are used in the analysis. The British Household Panel Survey (BHPS) (University of Essex. Institute for Social and Economic Research 2010) is the primary data source used for the enactment time results. This household panel ran annually between 1991 and 2009, having 5,500 households and 10,300 individuals as the original sample. In 2009, BHPS was replaced by Understanding Society, the UK Household Longitudinal Study (UKHLS) (NatCen Social Research and University of Essex. Institute for Social and Economic Research 2017). Understanding Society is used here as the primary data source for the notification period analysis. The data is collected biennially from members of approximately 40,000 households (at Wave 1) in the United Kingdom. Both studies incorporate both face to face interviews and self-completion online surveys. While the original BHPS panel participants were asked to take part in UKHLS, those who agreed form only a small sub-sample of the second study. In spite of the large turnaround in respondents across the two panels, both samples are nationally representative and many design features are constant across the two projects. All questions used to construct the variables included in the model are identical across the two time periods and have been asked every year since 1991.

I use a difference-in-differences design to estimate the treatment effect on the strength of support for the various incumbents and on vote abstention. The data is restricted to include only individuals aged 18 and over at any point in the study (voting age population). Moreover, individuals with missing date of birth, date of interview or gender information are excluded from the study, as these variables are key in determining treatment status. The dependent variables, *partisanship strength*, are derived from four questions asking respondents how close they feel to selected parties<sup>4</sup> and are measured on a scale from 0 (not

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<sup>4</sup>The questions used in the construction of the dependent variables are mapped in Appendix A.3.

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a supporter) to 9 (very strong supporter). The other dependent variable, *disillusionment with politics*, is dichotomous, taking value 1 if the respondent *spontaneously* declares that they do not support any party and 0 otherwise. Respondents have to volunteer this answer, as the original survey question does not include this option.

The analysis focused on the enactment treatment uses BHPS data from 1992 to 2007 (wave 1-16).<sup>5</sup> All unaffected individuals are in the control group, together with affected individuals or households interviewed prior to 19 July 1995 (the enactment date of the 1995 Pension Act). All interviews with affected individuals (or households, depending on the model specifications) conducted after this date are placed in the treatment group. Leveraging the quasi-random distribution of the fieldwork period for the British Household Panel Survey relative to the implementation of the treatment, I measure how intensity of support for the incumbent party and the main challenging parties changes as a function of receiving the treatment:

$$StrengthSupport_{p,i,m,y} = \alpha_i + \gamma_m + \lambda_y + \beta TreatmentStatus_{i,m,y} + Z_{i,m,y} + \varepsilon_{i,m,y} \quad (2.1)$$

Where the outcome variable is *StrengthSupport* for party  $p$  reported by individual  $i$  at time  $t$ .  $\alpha_i$  accounts for individual fixed effects,  $\gamma_t$  for month and  $\lambda_y$  for year fixed effects.  $\beta$  is the DID estimator of interest, which estimates the average treatment effect for the treated. *TreatmentStatus* captures possible differences between treated women and untreated individuals at time  $t$ , where  $t$  changes from 0 to 1 if individual  $i$  is interviewed after the law was enacted (19th July 1995). As robustness checks, the control group varies across

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<sup>5</sup>The 2007 Pension Act, which introduces further changes to the state pension age is enacted on 26 July 2007. Therefore, all interviews conducted after this date are excluded in order to avoid treatment contamination.

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models. Affected women are compared to all unaffected individuals and to unaffected women. To calculate household spillover effects, I compare the views of individuals living in affected households to the views of residents in unaffected households. I also compare men only, who are all unaffected, to determine whether spouses of affected women are more likely to change their attitudes than men living in unaffected households. Individual controls  $Z_{i,t}$  account for time-variant confounding effects such as *education*, *age*, *income*, and *interest in politics*.

I use data from waves 14-17 of BHPS (2005-2009) combined with waves 1 to 3 of UKHLS (2009-2011) for the analysis focused on the notification treatment. Together, they provide an overview of political preferences before and after letters were sent to affected voters (between April 2009 and February 2012).<sup>6</sup> Unlike the enactment treatment, this treatment does not have a unique reference date to determine whether the interview is conducted before or after treatment. The letters notifying affected women have been sent on one of 10 dates, depending on the date of birth of the affected individual (See Figure 2.3). Once again leveraging the quasi-random distribution of the fieldwork period for Understanding Society relative to notification letters, I standardise the treatment into “pre” and “post” notification. The notification date is also transferred to other individuals living in a treated household, such that the specific date when the treated household member receives the notification letter is used to determine the pre/post treatment period for all remaining individuals living in that same household (even if they are not directly affected by the letter). If multiple people within the same household receive letters at different dates, the date when the first

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<sup>6</sup>All individuals interviewed on or after 01 January 2012 are excluded from the analysis, as this is when notification letters related to the 2011 Pension act were sent (further treatment). As a robustness check, a different strategy which drops all interviews ever conducted with individuals affected by the 2011 pension act, but keeps the post 2012 interviews conducted with unaffected individuals is presented in Appendix A.5.3. The results do not change.

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letter arrives is used as the treatment period allocation marker. For those households where none of the members get treated, I randomly allocate one of the 10 dates as the treatment time. The same model as in Equation 2.1 is applied.

Figure 2.1 described three key time-points in the life-span of this reform: enactment, notification and implementation. The strategy used to evaluate the impact of the reform at the first two points has been detailed in the previous paragraphs but a similar analysis at implementation time was not possible. The implementation time can be defined in multiple ways 1) either the time when affected voters should have reached pensionable age under the pre-reform law, or 2) the time when they are finally eligible to retire under the new regulation. However, by the time the first women affected by the 1995 Pension act reach the implementation time (whichever way this is defined), there is no meaningful control group to compare them with. Most of their younger counterparts will have already been treated by other interventions, while attrition due to death becomes prevalent among older counterparts. Therefore, a meaningful analysis of the political preference changes at this point in time cannot be achieved.

## **2.5 Results**

I begin by looking at political responses of affected women at enactment time, and compare them with political views of non-affected individuals. This is the time when we are most likely to capture raw responses to the policy, as there are no time lags or additional political actors to complicate the accountability process. Controlling for education, age, income and interest in politics, I find no significant change in support for the incumbent (the Conservative party). The lack of effect was initially attributed in the hypotheses section to



lack of information about the reform, indifference towards the policy, or polarised views. However, as Figure 2.4 shows, the null findings refer to just the incumbent and the main opposition party. I detect a small positive effect for the Liberal Democrats - a small party at the time who had never been in power. This difference only holds at the 95% confidence level. I interpret it as indication that affected voters may have developed an interest towards non-mainstream politics as a result of having their minimum pension age increased.

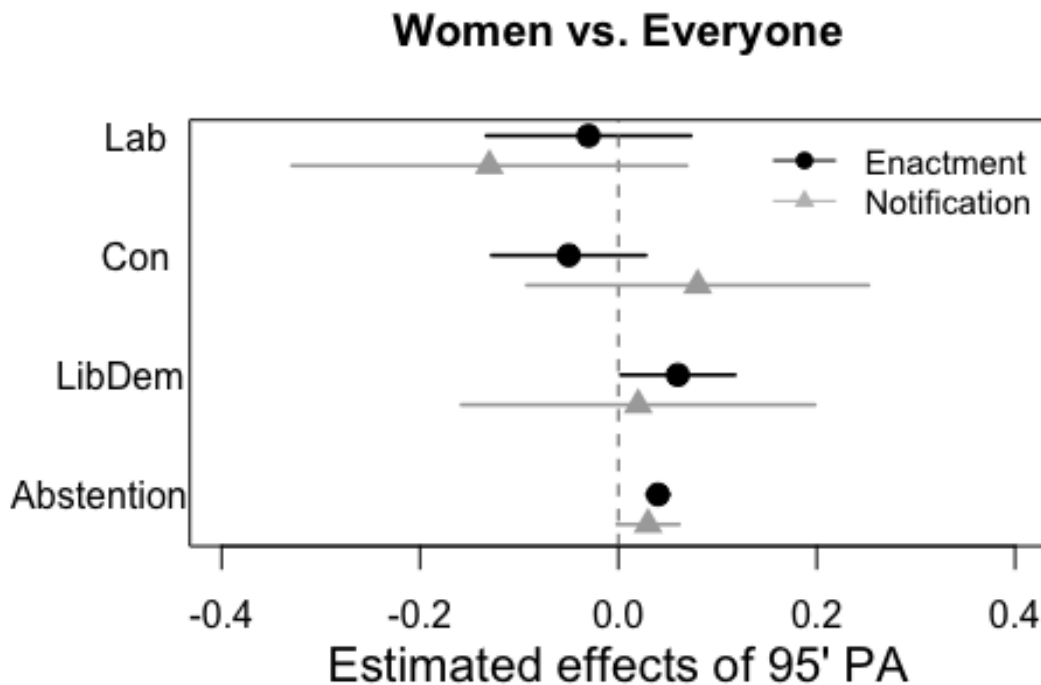


Figure 2.4: Estimated effects of being affected by the 1995 Pensions Act

The results also reveal a strong effect in favour of voting abstention, a finding which holds true at all conventional confidence levels. Affected women are more likely to say that they would not support any party in an election. This is an unexpectedly strong effect given the spontaneous nature of the response.<sup>7</sup> Together, these two findings eliminate the possibility that affected women were indifferent towards or completely unaware of

<sup>7</sup> Respondents are not given this choice when presented with the question, and have to volunteer this answer themselves.

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the policy at enactment time. They may suggest that voters did not respond to the policy changes based on rational economic calculations but based on emotional triggers. In line with recent work published by Baird and Wolak (2021), I conclude that women affected by the 1995 Pension Reform are left with a weaker sense of control over their fates, and therefore become more likely to blame the entire political system for the challenges they face in their lives. The retaliation is manifested through political absenteeism, and an initial spark of rebellion in favour of non-mainstream political parties, which eventually fades out in time.

The findings at enactment time however reflect political attitudes at a time of low salience, as affected individuals were decades away from being directly impacted by changes in their minimum pension age. Furthermore, even though the model accounts for education and interest in politics as a proxy for policy awareness, we still do not know how widespread knowledge of the policy was at the time. To address information-related concerns, I also look at political responses after a large scale dissemination exercise which took place between 2009 and 2011. The information treatment consisted of personalised letters (Figure A.5) sent by post to each affected individual. I use these letters to investigate changes in political preferences in response to acquiring (further) information.

The results from notification time suggest that affected individuals did not change their preferences towards either the enactment incumbent or the notification incumbent in response to being made aware or reminded of changes in pension age. These findings once again suggest that voters do not assign credit or blame based on economic considerations (be it sociotropic or egocentric) in response to the information treatment. They also suggest

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that affected individuals did not act irrationally and “shot the bearer of bad news” (the notification incumbent). Interestingly however, those affected continued to report mistrust in the entire political system and declare that they would not support any party if there was an election the next day. The results hold at the 95% confidence level. Both the inconsistencies in partisan effects at enactment and notification time, as well as the strong support for the alienation hypothesis at both time points indicate that voters were not blinded by preexisting partisan preferences during the decision-making processes. The persistence of the alienation findings disproves the voter myopia hypothesis and shows new evidence that voters are capable of evaluating events that took place decades prior, even if the process of allocating credit or blame is not targeted at specific parties.

In a second stage, I test whether the trends hold if affected women are compared with unaffected, older women. All coefficients are pointing in the same direction as before, the only difference being the statistical significance. In this second model, affected women are significantly less favourable towards the enactment incumbent (the Conservative party) than unaffected women. Unlike the previous model, focusing on gender specific political trends seems to provide evidence of some short-lived economic voting as a consequence of the bill being passed in parliament. These effects however cannot be attributed to pocketbook considerations, as this rationale would imply the effect amplifying once the consequences start reaching the voter’s pocket (at notification time, shortly before retirement). Furthermore, the likelihood of spontaneously expressing alienation feelings remains strong, and even more pronounced than in the general model. However, the level of support for the Liberal Democrats disappears, suggesting that gender is an important

mediating factor in the relationship between this policy intervention and political attitudes at the time.

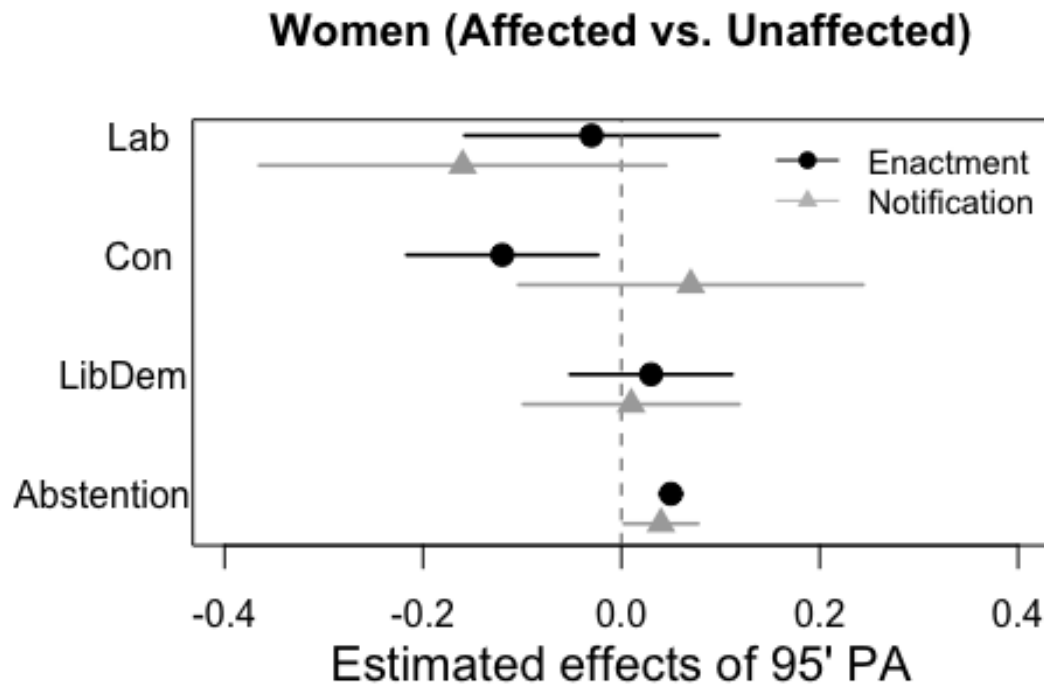


Figure 2.5: Estimated effects for women only

Although this reform only targeted women, it had far-reaching household implications, as entire families undoubtedly had to make financial adjustments. Therefore, in a final step, I look for household level spill-over effects. Consequently, all untreated men living with a treated women (“affected households”) are compared to untreated men living in “unaffected households” (Figure 2.6 left). I also compare all members of treated and untreated households, irrespective of their individual treatment status (Figure 2.6 right). I find no significant differences between these groups, with the exception of the alienation hypothesis which holds when comparing treated and untreated households at enactment time, but not in the men-only model. This suggests that any household differences in political preferences are mainly driven by affected women. I therefore conclude that this

was a very personal and alienating treatment which eroded trust in political actors among the treated.

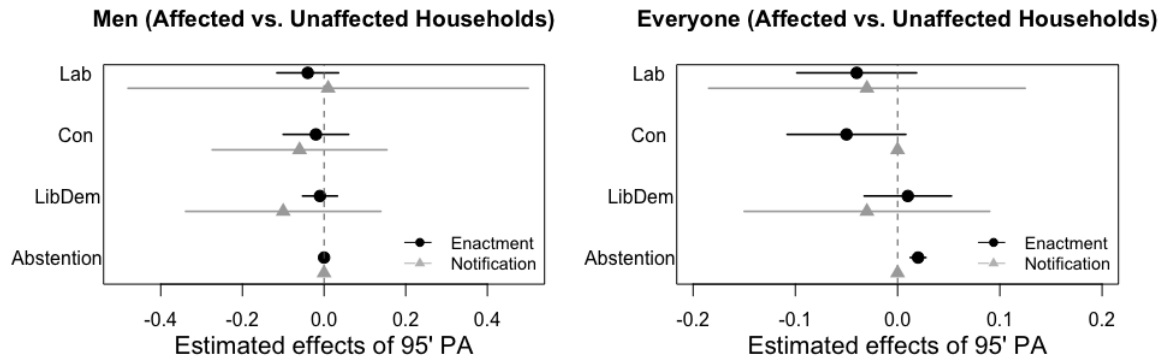


Figure 2.6: Spillover effects at the household level

## 2.6 Discussion and concluding remarks

Retirement related reforms have been implemented in welfare nations across the world to allow their economies to cope with a rapidly ageing population. Changes in official retirement ages proved to be a commonly adopted solution. However, the reforms are posing explosive challenges, being associated with political backlash and even peril. Many countries, Britain included, continue to raise the minimum state pension age, although the adverse social and political effects of these policies remain unclear (Müller and Shaikh 2018).

Further interventions aimed at increasing the minimum retirement age have been implemented since the 1995 Pensions Act. While communication with the public has drastically improved (notification exercises have been conducted earlier in the implementation period and the minimum state pension age has been featured more prominently on the government website and in the news), the evolution of public opinion in response to long-term

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socio-economic interventions deserves further investigations. We need to understand how voters evaluate policy makers when policies implemented by them span different administrations. We also need more evidence as to how the lack of transparency and reliable information aids political alienation and mistrust in politics. This paper lays the foundation for future research on the role of time lags in the electorate's retrospective evaluations of long-spanning policies.

Overall, this paper revealed some interesting behavioural patterns among affected voters and across affected households. Those impacted by the reform displayed signs of partisanship preference shifts in the immediate aftermath of the reform, although some of these effects proved to be contingent on the comparison group used. Interestingly however, not many of these attitudes stood the test of time. Upon receipt of a letter, 15 years later, informing them of how the reform will shortly be affecting them, partisan attitudes among the treated group remained indistinguishable from those of unaffected electorate. The absence of any meaningful changes in partisan evaluations at the notification time can mean several things. Either voters perceive the issue to be a failure of all democratic institutions rather than the fault of any specific party, or self-assessments of support for the main parties is not the right way to measure the effect of the policy. The role of time-lags remains somehow ambiguous in light of the findings, as the current data does not allow a more in-depth analysis of the mechanisms behind the stated preferences. Reports of voting abstention were on the other hand strong, robust across comparison groups, and constant across time. I interpret this as evidence that voters are not irrational (myopic, punishing the bearer of bad news), but they also do not conform to standard accountability models of

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pocketbook voting.

Understanding how voters balance short term personal benefits and individual sacrifices in favour of long term public goods is essential for preparing future responses to contemporaneous challenges as diverse as ageing populations, immigrant integration, creating a sustainable welfare state and even emergency responses to climate change and security threats. Over-generalisation of the findings revealed in this paper is however advised against, as the conclusions drawn here are the product of a pioneering reform. Careful consideration of the circumstances under which this policy and the information dissemination exercise took place can reveal far-reaching implications, but further research is called upon to test the external validity.

Turning to the limitation of the research, a few data availability shortcomings should be highlighted. In spite of their many attractive features, the rich household panels used for the analysis have clear limitations. For once, the political battery of question is limited. Aside from the previously mentioned shortcoming of not being able to use the existing questions to explain the cognitive mechanisms behind the findings, another limitation is the list of parties included in the preference-tracker questions. Most notably, they do not include attitudes towards far right parties who are notoriously known for targeting the “left behind”. A manifesto analysis presented in Appendix A.1 shows that UKIP was actually one of the few parties which directly addressed the Pension Reform issue while campaigning. It is possible that affected women would have become more supportive of UKIP and we simply do not have the data to measure it.<sup>8</sup> Further research is necessary to

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<sup>8</sup>UKIP received its own categorical label (separate from “other party”) in the question “If there were to be a general election tomorrow, which political party do you think you would be most likely to support?” only at Wave 5 of UKHLS (2013-2014)

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identify whether the policy impacted how people view far-right parties.

Turning to the future, the results presented in this paper can provide valuable lessons on how to deal with other unsustainable policy frameworks. We live in an era where politicians and the broader society have to make immediate sacrifices for the benefit of future generations (Heller 2003). With most advanced industrial democracies facing an ageing population problem, and challenges posed by technology advances and climate change becoming more pronounced in political discourse, it is time we acknowledge that time-lags will define the political sphere in the future. Policy makers need to have an honest conversation with the public about the implications of such policies, the uncertainty surrounding them and the sacrifices people will have to make in the name of sustainability.



## Chapter 3

# Migration: Low-Cost Flights and Far-Right Votes

The Eastern enlargement of the European Union (EU) in 2004 and 2007 brought unprecedented access to the UK for citizens of ten Central and Eastern European countries. The likely flows resulting from this development were difficult to predict. One major study commissioned by the UK government considered the country “not a very popular migration destination” and estimated an annual net inflow of between 5,000 and 13,000 from these EU accession countries in the years up to 2010 (Dustmann et al. 2003: 57). Instead, official statistics show actual numbers averaged more than 100,000.<sup>1</sup> This period thus saw a radical change in migration into the UK more generally and its capital city in particular. In this paper, we examine how this affected support for far-right parties, and draw out wider implications for the study of the electoral effects of migration.

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<sup>1</sup>Central and Eastern European residents from the 2004 accession countries increased from an estimated 167,000 in 2004 to 1,323,000 in 2018, while those from Bulgaria and Romania increased from 42,000 in 2007 to 495,000 in 2018 (Vargas-Silva and Fernandez-Reino 2018: 4).

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Our work relates to a growing literature on the electoral impact of migration at the local level (e.g., Barone et al. 2016; Calderon, Fouka and Tabellini 2019; Dinas et al. 2019; Dustmann, Vasiljeva and Piil Damm 2019). Subnational units are more comparable than countries, which helps to mitigate concerns about omitted variable bias (Golder 2016). Yet with rare exceptions (e.g., Dustmann, Vasiljeva and Piil Damm 2019) the location decisions of migrants are not (as-if) random. If migrants avoid hostile areas and are drawn to more welcoming ones, we may underestimate the effect of migration on the electoral support for far-right parties. On the other hand, some migrants have little choice but to settle in locations where socio-economic challenges can make native-born voters susceptible to far-right parties. In attempts to address the problem of immigrant sorting, many scholars rely on instrumental variable approaches including the shift-share instrument (e.g., Edo et al. 2019; Halla, Wagner and Zweimüller 2017; Mayda, Peri and Steingress 2018). The latter approach exploits an initial distribution of migrants to project the flow of new migrants across geographic units, based on the idea that location decisions are influenced by existing networks (Jaeger, Ruist and Stuhler 2018).

Beyond adding evidence from Britain (Cools, Finseraas and Rogeberg 2021), our analysis makes several contributions to the literature on the effects of local-level migration on support for far-right parties in local or national elections. First, we show the utility of a more nuanced analysis that distinguishes different categories of migrants. Transport and geography scholars highlight a recent type of “hypermobile” migration characterized by a far higher degree of connectivity between the country of birth and the host country of migrants than in previous population movements (Burrell 2011; Pooley 2017). Previous migrants typically faced various (logistic, legal, political, affordability, etc.)

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constraints to frequent travel back to their countries of birth. In recent decades, however, the liberalization of transportation markets in some parts of the world has transformed the availability and affordability of cross-national travel (Akgüç, Beblavý and Simonelli 2018; Dobruszkes 2009). We use this insight to underpin our empirical strategy, but it may also be substantively important. Indeed, prior research shows that different categories of migration can affect election outcomes in distinct ways (e.g., Edo et al. 2019; Mayda, Peri and Steingress 2018). This category is especially important where policymakers actively promote economic integration and cross-national mobility.

Second, our empirical strategy offers an alternative to the commonly used shift-share instrument that is useful when data limitations hamper the deployment of the latter, or as a robustness check. Where migrants maintain regular links with their home countries, their location decisions partly reflect ease of access to relevant travel infrastructure (Burrell 2011; Dobruszkes 2009). Our study shows that the distribution of migrants from the new EU member states across London is linked to the location of pre-existing infrastructure that provides access to the principal means of travel to and from their home countries, especially low-cost airlines. We combine 2001 and 2011 census data on the inflow of migrants into London with information on the location of pre-existing bus stops serving the two dominant airports for cheap flights to and from Central and Eastern Europe. Passenger survey data confirm the importance of this mode of travel to the relevant airports. We also confirm empirically that these bus stops are associated with changes in the population of migrants from this region, but not other migrants. Using proximity to these bus stops as an instrument for ward-level increases in the population from these countries, we uncover a large positive effect on changes in support for far-right anti-immigrant parties between the

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2004 and 2012 London elections.

Finally, we investigate the channels that drive the electoral response. Using a split sample strategy that exploits variation in initial conditions across wards, we study both economic and cultural threat channels. Amongst the major grievances emphasized in the political campaigns of far-right parties is the lack of affordable housing. We find that wards with lower initial housing costs show higher support for one of two far-right parties in response to migration. Housing costs condition the electoral response to migration for the populist right, but not for the main fascist party. This lends further support to claims by scholars who have emphasized the importance of distinguishing different categories of far-right parties (see Golder 2003: 443). The far-right vote is not homogeneous and different parties use different mechanisms to attract support. In the UK, economic stress stemmed from the abolition of housing-related benefits has been identified as a significant factor explaining subsequent support for exit from the EU (Fetzer 2019). Our analysis chimes with this in suggesting concerns about housing costs as a key driver of support for the populist right.

### **3.1 Transportation, migration, and politics**

The role of transportation infrastructure is a recognized but neglected element in the literature on migration and mobility (Pooley 2017). Studies of different urban contexts find that location decisions are linked to access to public transport, on which especially new migrants tend to rely more than established residents (Chatman and Klein 2009; Perez, Dragicevic and Gaudreau 2019; Tsang and Rohr 2011), and that some migrants value access to airports (Maslova and King 2020). In this section, we examine the link between transportation and migration with high cross-national mobility. We then set out how we

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use insights from the transportation and geography literature and related fields in order to address empirical challenges in our study of the electoral consequences of migration.

The period around the start of the millennium was characterized in many industrialized countries by increased competition in aviation markets. The entry of low-cost operators, containing operating costs and offering more basic “no-frills” services than traditional airlines, made air travel affordable on a wide scale not seen before. In 1993, the US Department of Transportation described this as the “Southwest Effect” with reference to the airline’s expansion. In Europe, too, low-cost or “budget” airlines grew rapidly during this period. Liberalization from 1992 allowed EU airlines with an operating license to serve the entire EU market without commercial restrictions. Low-cost carriers took advantage of this development and increased their seat share from less than 2% in the early 1990s to more than 40% by 2010, exceeding that of “legacy carriers” - those with established routes prior to liberalization - thereafter. In 2016, Ryanair, the EU’s first low-cost carrier, became Europe’s largest airline by passengers carried (Akgüç, Beblavý and Simonelli 2018).

During this period, low-cost carriers were a powerful force behind an expansion and diversification of flight networks on west-east routes with Central and Eastern European member states that joined the EU in 2004 (the “EU8” comprising the Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, Slovakia, and Slovenia) and 2007 (Bulgaria and Romania, the “EU2”).<sup>2</sup> Especially the Irish carrier Ryanair and Hungarian airline WizzAir aggressively increased their provision of flights on west-east routes and commenced a

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<sup>2</sup>Official UK statistics label these groups of new member states as the “EU8” and “EU2” respectively and we collectively refer to them as the “new EU” or “EU8+2” countries.

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range of new services (Dobruszkes 2009; Jankiewicz and Huderek-Glapska 2016).

Policy assessments of the consequences of low-cost travel in Europe have focused on economic aspects and benefits in terms of mobility. The European Parliament has noted impacts on established airlines and airports, competition, and regional development (Macário et al. 2007). Low-cost air travel can increase tourism and business-related travel, and it boosts the mobility of labour and students by lowering the cost of migration. Following the initial migration of an individual, affordable travel facilitates visits to and from friends and relatives, or “VFR” travel in industry jargon. A leading EU think tank concludes that low-cost carriers “play a vital role in bringing Europe closer together by fostering mobility and making air travel affordable to a wider public” (Akgüç, Beblavý and Simonelli 2018: 44).

This unprecedented level of connectivity is a crucial new element that characterizes this period of European integration. In sharp contrast, earlier migration from Central and Eastern Europe to the West was typically final and much more constrained (Ignatowicz 2011: 35). Traditional airlines were too expensive and alternative modes of transport - coach, ferry, or rail - too cumbersome to enable large volumes of migration with high levels of mobility (Akgüç, Beblavý and Simonelli 2018). When the EU’s Eastern enlargement removed restrictive work and travel rules, low-cost carriers fuelled and shaped this new wave of migration. Citizens from the “new” EU member states could travel to and work in the “old” ones with far greater ease than at any point in the past. Moreover, access to regular and affordable journeys back home allowed them to maintain family and social ties, and lowered the perceived risk of migration. According to Ignatowicz (2011: 43), this

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also had significant emotional value, since mobility is “not only about the actual physical movement but also about a feeling of being in a privileged position to go home at any time.” Burrell (2011: 1023) sums up thus: “Ryanair flights define this new migration.”

What do these developments imply for the study of the electoral consequences of migration? The transportation-focused literature cited above does not cover political or electoral implications. Yet it provides the underpinnings of our empirical approach. The insight we develop is that the distribution of highly-mobile migrants is likely to be systematically linked to their access to low-cost travel infrastructure that connects them - both practically and emotionally - with their home countries. In our empirical context that we introduce below, this allows us to link the location decisions of this category of migrants to specific pre-existing access points to low-cost air travel to and from Central and Eastern Europe. If empirically confirmed, this link offers a strategy to overcome bias due to immigrant sorting and to assess the causal effect of migration on electoral support for far-right parties in such settings.

## **3.2 Context**

Our study exploits significant spatial heterogeneity in the changing composition of London’s population as a result of the Eastern enlargement of the EU. This offers a unique context to examine the electoral effects of migration caused by the UK government’s decision not to impose transitional work restrictions on workers from EU8 countries. Most other EU governments feared a large influx of migrants would put pressure on labor markets, and implemented a phasing-in period of up to seven years for the free movement of workers from the accession countries. However, in 2004 the UK, Ireland and Sweden

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were the only countries which did not impose any restrictions and allowed unfettered access to their labor market from day one (Becker and Fetzer 2016). With significantly fewer restrictions here compared to countries such as Germany or Austria, many Eastern European migrants chose to come to the UK. This resulted in a substantial inflow of migrants into the capital in particular, but with significant variation in settlement patterns (Kone 2018).<sup>3</sup>

This context also allows us to focus on a category of migrants that has hitherto not been studied. Earlier studies of the political effects of migration look at migrants in general (e.g., Barone et al. 2016), or specific subcategories such as refugees (e.g., Dinas et al. 2019; Dustmann, Vasiljeva and Piil Damm 2019). Others distinguish migrants by education or skill level (e.g., Edo et al. 2019; Halla, Wagner and Zweimüller 2017; Mayda, Peri and Steingress 2018). We are the first to consider the role of post-migration travel to their country of birth.

We analyze local elections as opposed to national ones because they allow us to carry out a highly spatially disaggregated analysis of voting patterns at the ward level. In Britain, national election results are announced at the borough or constituency level, which are far larger geographic areas.<sup>4</sup> The granularity of our ward-level study also stands out in

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<sup>3</sup>Bulgaria and Romania joined the EU in 2007, slightly later than the EU8. This matters little for our study. We analyze the impact of migration as captured in the 2001 and 2011 census rounds, which span both accession dates. Although EU2 nationals, unlike their EU8 counterparts, were subject to interim work restrictions until the end of 2013, census data show a sharp increase in London residents born in Bulgaria (+24,200 or 802%) and Romania (+41,800 or 1371%) between 2001 and 2011. These growth rates are the sixth and second highest of all immigrant groups, which is also reflected in EU2 air passenger traffic during this period (see Figure B.3 in Appendix B.2).

<sup>4</sup>This analysis focuses on 620 wards across 32 local authorities, while for general elections London had 74 parliamentary constituencies from 1997 until 2010 (when the number was reduced to 73). The total population of wards ranged between 4,692 and 17,257 in 2001 and between 5,110 and 23,084 in 2011. In contrast, parliamentary constituencies in 2010 had total populations between 81,831 and 136,111 (London Parliamentary Constituency Profiles 2010, London Datastore).



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comparison to prior work, which uses data at the level of municipalities (Barone et al. 2016; Dinas et al. 2019; Dustmann, Vasiljeva and Piil Damm 2019; Steinmayr 2020), counties (Calderon, Fouka and Tabellini 2019; Mayda, Peri and Steingress 2018), regions (Edo et al. 2019), or congressional constituencies (Mayda, Peri and Steingress 2016). Our setup allows us to probe, in the final section, underlying channels at the neighbourhood level.

The unique electoral system used in London Assembly elections motivates our focus on the capital. This is unusual among local governments in the UK in that it uses an “additional member system” combining first past the post as well as closed list proportional representation. Studying the effects in the London-wide Assembly Members election context means that we can capture the purest form of support for far-right parties. Votes cast for the London-wide party list are least likely to be distorted by strategic considerations that play a role in other settings, notably the first-past-the-post electoral system used for local and parliamentary elections in Britain. Furthermore, this is the only ballot choice where voters are asked to vote for a party, not a candidate, meaning that party affiliation will be the primary heuristic used by most voters. Finally, the ballot is unique to the whole of London, hence there is no need to account for candidate effects, ballot order effects, or whether a party is fielding a candidate or not.

This period offered fertile ground for far-right parties to tap into real or perceived grievances associated with immigration (Campbell 1965; Dancygier 2010; Golder 2003; Ivarsflaten 2008). In addition to fears related to increased competition for jobs, these parties amplified welfare concerns by claiming that migrants put pressure on housing and local services, and

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extract welfare benefits (Becker and Fetzer 2016; Clarke et al. 2017).<sup>5</sup> In the run-up to the 2016 referendum on leaving the EU, one infamous campaign poster claimed the UK was at “breaking point.” It showed a long queue of dark-skinned migrants and demanded: “We must break free of the EU and take back control of our borders.” A popular portrait of the capital city during this period portrays it as teeming with migrants, many unable to speak English, sleeping rough or in overcrowded housing, and working for cut-throat wages (Judah 2016). In London, the far-right UK Independence Party (UKIP) and British National Party (BNP) both campaigned on policies to prioritise natives over migrants, especially for housing and jobs.<sup>6</sup> Our analysis probes to what extent these messages are related to voting patterns.

### 3.3 Data

This section provides an overview of our data, with full details in Appendix B.4. The dependent variables are the percentage point changes in support for UKIP and the BNP between 2004 and 2012. While the BNP is a fascist party and UKIP held more populist right-wing positions (see Golder 2003: 443), both strongly opposed immigration. We focus on 2004 and 2012 because these elections are temporally closest to relevant census years, 2001 and 2011. There were also minimal boundary changes in this period, so that our results are directly comparable without any adjustments or imputation. Although London Mayoral and Assembly elections were held in 2000, when these institutions were established, this was followed by major boundary changes. Moreover, ward-level results

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<sup>5</sup>Contrary to such perceptions, European migrants in fact make a positive net fiscal contribution to the UK exchequer, unlike natives and other migrants (Dustmann and Frattini 2014).

<sup>6</sup>In their 2012 London electoral campaign, UKIP promised “more jobs for Londoners by saying ‘No’ to open-door immigration” and prioritising Londoners “over migrants and asylum seekers” for jobs and housing. The BNP demanded that “British people must be housed first” and “British jobs for British workers.” Appendix B.1 displays the 2012 campaign leaflets of these parties.

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were not collected until 2004.

Importantly, the 2004 elections took place only one month after the EU's enlargement. As a result, the number of migrants from the new EU member states who would have had time to settle in London is likely to be negligible. Therefore, we use the 2004 election results as the baseline for pre-enlargement political views. If the inflow of migrants in the first few weeks following the 2004 Eastern enlargement was already sizable enough to leave an impression on the local population, and to affect their voting behavior, our results may understate the electoral response.

The main independent variable is the percentage point change in residents born in any of the EU8+2 member states who settled in each of the 620 London wards in our dataset. To calculate this, we commissioned the UK Office for National Statistics to produce ward-level census data for 2001 on the country of birth of all residents, consistent with information from the 2011 census.<sup>7</sup> A positive number indicates an increase in migrant residents from EU8+2 countries. The values range from -0.06 percentage points (in Hacton, Havering, which is the only ward with a negative trend with an EU8+2 share that dropped from 0.86% in 2001 to 0.8% in 2011) to +15.28 percentage points (in Grove Green, Waltham Forest, where it jumped from 0.51% in 2001 to 15.79% in 2011). Figure 3.1 depicts these data.

We account for several potentially confounding socio-economic and demographic factors that might be correlated with both changes in voting patterns and the composition of the

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<sup>7</sup>Such high-quality and granular census data may not be available for parts of the country with fewer migrants, due to statistical disclosure controls that involve the swapping of records to safeguard personal information. This approach is targeted at households with unusual characteristics in small areas, such as wards.

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population. The first category encompasses changes in the median household income and in the unemployment rate between 2001 and 2011, as well as median house prices in 2001. The latter variable is important for modeling patterns of migration, as affordability of housing is a key driver of residential choice. The second category includes the percentage point changes in the retired and student populations, as well as the percentage point change in all other foreign-born residents (excluding those born in EU8+2 countries). Changes in the native population constitute the omitted reference category.<sup>8</sup>

### **3.4 Empirical strategy**

In exploiting the geographic distribution of migrants, we face the problem of endogeneity of settlement choices. To overcome this issue, many papers use a shift-share instrument, which interacts national inflows by country of origin with an initial geographic distribution of immigrants (Jaeger, Ruist and Stuhler 2018). This is an attractive solution where previous supply shocks are unlikely to be correlated with ongoing responses. However, constructing a shift-share instrument is not always possible. For example, in our case, we can use data from either the 1991 or the 2001 census to recreate the original (pre-enlargement) stock of migrants from new EU member states. The 1991 census data capture the distribution of migrants prior to the granting of freedom of movement and residence rights to nationals of EU countries in the Treaty of Maastricht in 1992.<sup>9</sup> However, most countries that joined the EU in 2004 did not exist as independent states when the 1991

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<sup>8</sup>We also considered accounting for the change in cultural diversity and in relative deprivation. However, we are worried about post-treatment bias and do not account for these variables in our main specifications. Table B.10 in Appendix B.7 reports results with alternative controls.

<sup>9</sup>The Treaty of Maastricht created the EU and took effect in November 1993. It established, among others, the freedom of movement of persons. This refers to freedom of settlement, freedom to access employment and to work, and freedom to provide services in another EU member state.

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census was designed.<sup>10</sup> While the 2001 census contains reliable disaggregated country of birth data, we worry that the distribution it captures is not exogenous as migration patterns may have started to shift in anticipation of the EU's Eastern enlargement.

We implement an alternative instrument based on proximity to pre-existing travel nodes. The literature reviewed earlier emphasizes that proximity to relevant travel nodes has both practical and emotional value for this category of migrants. Hence, it should be reflected in their location decisions. We harness this insight and instrument the distribution of new EU migrants across the London wards between 2001 and 2011 using distance from the nearest relevant travel hub:

$$z_w = \min(| \text{Centroid}_w - \text{BusStop}_i |) \quad (3.1)$$

where the instrument  $z_w$  captures the distance, in kilometres, to the nearest pre-existing 757 bus stop (connecting central London to Luton, the base of WizzAir) or A6 bus stop (for Stansted, the base of Ryanair). As we discuss in detail in Appendix B, these two operators dominated bus transport to the respective airports and their buses travelled along the same main route north through London. We only consider stops established prior to May 2004. We calculate the distance from each ward's geometric center  $\text{Centroid}_w$  to these pre-existing 757 and A6 bus stops. For each ward, we then keep the shortest distance. This is illustrated and summarized in Figure 3.2, where the bus stop locations are indicated as white dots. The darker the shading of a ward, the closer its centroid is to the nearest of these bus stops. The inset map shows the location of Luton and Stansted airports, both to the north of London. Flights to and from the capital are also available at several other

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<sup>10</sup>In 1991, Estonia, Latvia, Lithuania became independent from the USSR, and Slovenia declared independence from Yugoslavia. In 1993, Czechoslovakia dissolved into two independent states, the Czech Republic and Slovakia

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airports that are omitted here, including Heathrow in the West and Gatwick in the South, which are the two busiest airports in the country, as well as London City and Southend in the East.

We focus on bus stops, as opposed to access points to other modes of transport, for several reasons. Luton and Stansted airports are not connected to London's underground network, unlike the capital's largest airport Heathrow, thus ruling out this mode of transport. We know that public transport is disproportionately important for recent migrants in particular, as they are less likely to have a car (e.g., Ignatowicz 2011; Tsang and Rohr 2011: 36).<sup>11</sup> For those without a car, taking a train or taxi would be alternatives, but both of these are significantly more expensive than a bus.<sup>12</sup> This makes the bus a likely choice for budget-conscious travellers. Statistics from this period confirm that sizable proportions of travellers used the bus to get to both Luton and Stansted airports.<sup>13</sup> Overall, these are strong reasons to focus on this mode of transport to the airport for EU8+2 migrants who came to the capital during these years.

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<sup>11</sup>Census 2011 Table DC4203EW from the Office for National Statistics shows tenure by car or van availability by ethnic group. In London, merely 28% of those classified as English, Welsh, Scottish, Northern Irish, or British lived in households without a car or van, while the equivalent figure for "other white" (such as European migrants from the EU8+2 countries) was 43%.

<sup>12</sup>The price of train journeys between London and Luton or Stansted listed in travel guides from 2004 that we consulted (see Appendix B.3) was up to twice the cost of a bus. The listed price of a one-way transfer on a London taxi, or "black cab," was up to 11 times the price for a bus. Private hire vehicles or minicabs can be somewhat cheaper than a black cab.

<sup>13</sup>The UK Civil Aviation Authority carries out an Annual Passenger Survey. In the years 2004, 2005, and 2006, the reported shares of departing passengers who travelled to the airport by bus/coach were, respectively: 24.1%, 10.4%, and 29.8% for Luton; and 11.4%, 14.3%, and 16.3% for Stansted. From 2007, the reported categories of transport modes are different. ([www.caa.co.uk/Data-and-analysis/UK-aviation-market/Consumer-research/Departing-passenger-survey/Survey-reports/](http://www.caa.co.uk/Data-and-analysis/UK-aviation-market/Consumer-research/Departing-passenger-survey/Survey-reports/), Table 9a "Modes of transport used at the [...] survey airports," downloaded on January 27, 2021).

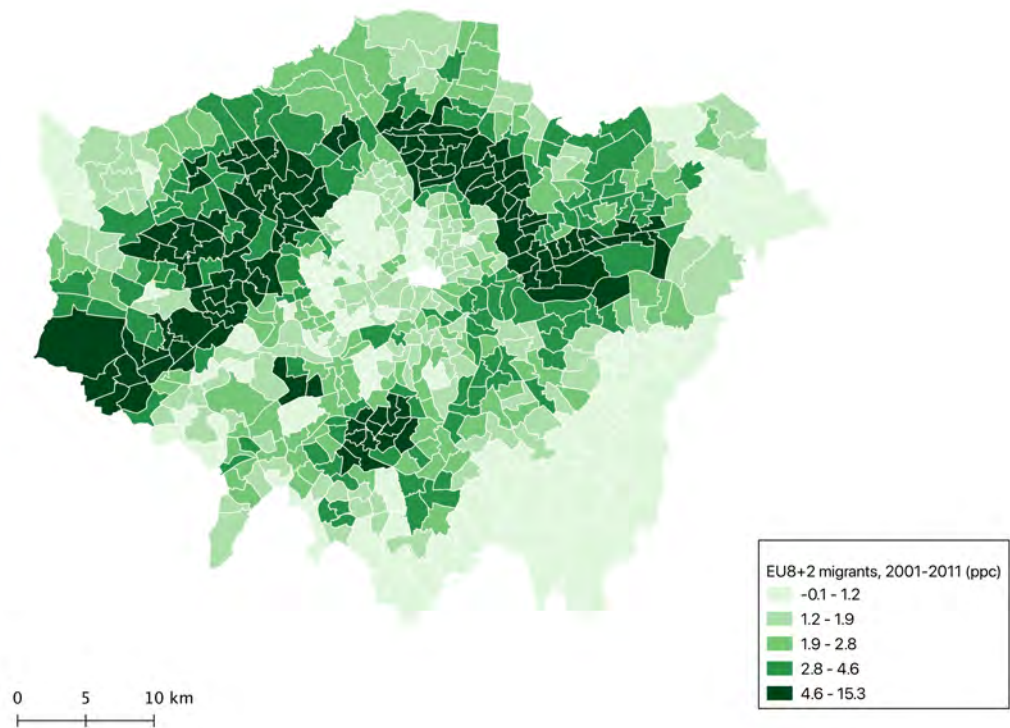


Figure 3.1: Percentage point change in residents born in one of the new EU member states

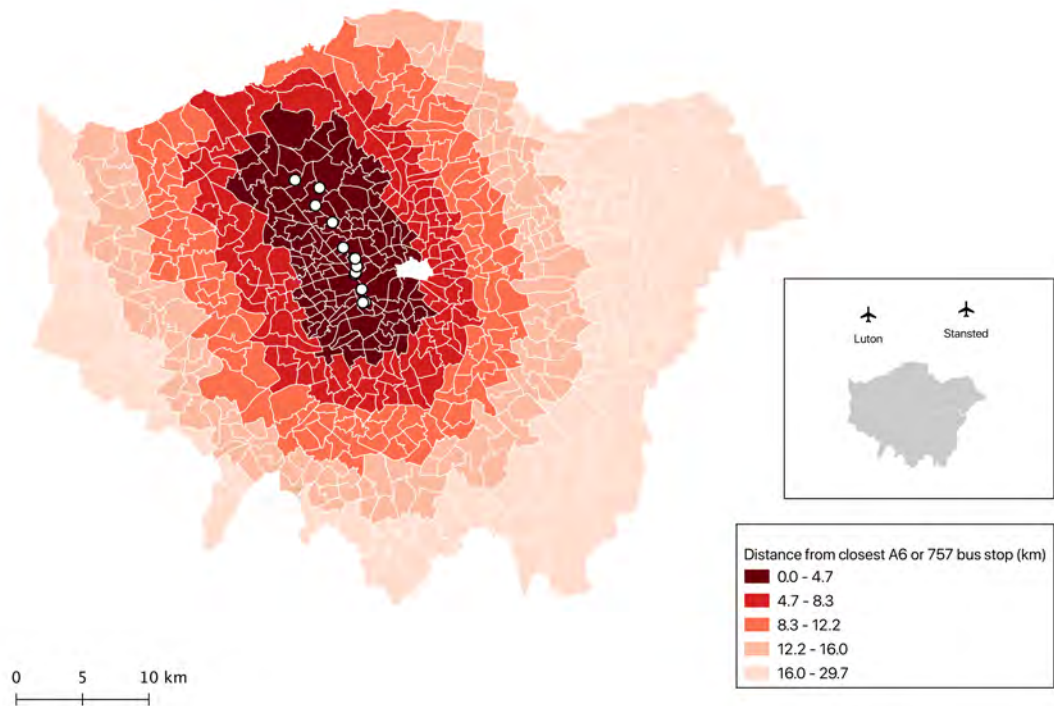


Figure 3.2: Proximity to transport nodes instrument

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The validity of our instrument rests on the key assumption that the evolution of far-right political success was not affected by the existence of these travel nodes, except for their effect through the proportion of immigrants who decide to settle in their proximity for easy access to means of travel to their home countries. In our case, the establishment of this specific transport infrastructure precedes the Eastern enlargement of the EU. To verify the location of individual bus stops at the time of enlargement, we obtained official confirmation from the responsible government body, Transport for London, of the pre-existing stops on the main routes of the two dominant operators.<sup>14</sup> In the case of the 757 to Luton, this line was established before the end of the Cold War, in the wake of the 1980 Transport Act deregulating coach services. Bus services to the redeveloped Stansted airport were in place following the opening of a new terminal building in 1991.

While the decisions of some airlines to expand their routes between London and Central and Eastern Europe responded to demand, our identification strategy requires that their choice of airport base in London was exogenous to the processes we study. Indeed, low-cost carriers chose their airport bases before any large-scale migration movements from the region to London. Ryanair moved its base from Gatwick to Stansted in 1991 already, at a time when the outcomes of the transformation process in Central and Eastern Europe were highly uncertain and more than a decade prior to the accession to the EU of new member states emerging from this process. WizzAir commenced flights to and from Luton in May 2004, coinciding with their accession. The airline had also considered Stansted as a base. Importantly, these were not the only feasible airports for low-cost carriers, as

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<sup>14</sup>Coach companies need permission to operate any bus routes within London. Transport for London (TfL) is the governing body which authorizes these requests. As we document in Appendix B.3, we confirm the dominant pre-existing bus routes and operators against sources including travel guides and official documents from the period. Smaller operators such as EasyBus or Terravision varied over time and often travelled along the same or similar routes.



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London had a total of six international airports. Had these airlines chosen different airports as their bases, we contend, subsequent migrant settlement patterns across London would likely look different, too. Given the timing of the different decisions involved, it seems highly implausible that these companies chose their London base with reference to some anticipated spatial distribution of Central and Eastern European migrants across the capital, or how far-right parties might perform across different wards.

The exclusion restriction would be violated if these travel nodes affected the location decisions of other migrant categories. This is unlikely. On average, migrants from Western Europe constitute the wealthiest population segment in the UK (Dustmann and Frattini 2014), which makes them less reliant on low-cost transportation. Those among this group who prefer low-cost flights have options that are more geographically dispersed. For instance, another major low-cost airline, EasyJet, services a dense network of Western European destinations from its largest base, Gatwick, which is South of the capital. In addition, convenient access to several Western European countries exists via several other modes of transport, including a high-speed rail service, the Eurostar, or by car using the Channel Tunnel opened in 1994.<sup>15</sup> Non-EU migrants have longer travel distances to their countries of birth, which makes travel to these destinations less convenient and more expensive. As a result, they are less likely to travel home as frequently as those from the EU. This, in turn, makes it unlikely that the location decisions of non-European migrants are influenced by the proximity or accessibility of any particular international travel nodes.

While these are strong reasons to believe that the relevance of our instrument is specific to

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<sup>15</sup>Eurostar alone transported 100 million passengers between 1994 and 2009. See [www.eurostar.com/uk-en/about-eurostar/our-company/our-history](http://www.eurostar.com/uk-en/about-eurostar/our-company/our-history) (last accessed January 24, 2021).

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EU8+2 migrants, we also confirm this empirically in the following section.

Casual inspection of the two maps above also suggests that our instrument is relevant. Overall, the Northern half of the capital was both better connected to Luton and Stansted airports, via the bus stops on the main coach routes to these airports, and it contains many more wards with substantial post-enlargement migrant inflows than the South. It is striking that the South East of London is furthest from access to the key travel nodes we identify, and at the same time the part of the capital that received the fewest migrants from the region between 2001 and 2011. In the following section, we formally assess the relevance of our instrument.

### 3.5 Specification and results

Our model specification is as follows:

$$\Delta \text{New EU residents}_{w,2001-2011} = \alpha_{1b} + \beta z_{1w} + \gamma \Delta C_{1w} + \Delta \epsilon_{1w} \quad (3.2)$$

$$\Delta \text{Votes}_{p,w,2004-2012} = \alpha_{2b} + \rho \widehat{\Delta \text{New EU residents}}_{w,2001-2011} + \gamma \Delta C_{2w} + \Delta \epsilon_{2w} \quad (3.3)$$

where  $\beta$  in equation 3.2 captures the first-stage effect of our instrumental variable  $z_w$  on the percentage point change in immigrants from EU8+2 countries in ward  $w$  between 2001 and 2011, while accounting for ward-level covariates  $C$  and borough fixed effects  $\alpha_b$ . The fitted values  $\widehat{\Delta \text{New EU}}_{w,2001-2011}$  enter the second stage specified in equation 3.3, where the outcome is the percentage point change in votes cast for party  $p$ , either UKIP or the BNP, between the 2004 and 2012 elections.

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### 3.5.1 Main results

Our main findings for both outcomes are presented in Table 3.1. The first two columns report baseline OLS results. The coefficients on our variable of interest suggest no relationship in the case of UKIP, and at best a substantively small and statistically weak relationship for the BNP. A one percentage point increase in the share of migrants from the new EU member states is associated with a .01 percentage point increase in the share of votes cast for UKIP, and an equivalent of .06 for the BNP. These results give us a baseline against which we can evaluate our empirical approach.<sup>16</sup>

The first-stage results in column three of Table 3.1 allow us to assess the strength of our instrument. The instrument is a significant predictor of the change in new EU residents. The first-stage F-test on the omitted instrument comfortably exceeds the conventional cut-off value of 10, indicating that our instrument provides sufficient exogenous variation. In other words, our bus stop instrument and the spatial distribution of the accession-induced migration shock are sufficiently correlated for the instrument to be relevant.

We also carry out a placebo first-stage regression, where we use the change in other foreign-born residents as the dependent variable and control for the change of EU8+2 residents. These results are presented in column four of Table 3.1. As we argue above, other migrant groups are not obviously dependent on the specific transportation infrastructure that underpins our identification strategy. Indeed, the results confirm that our instrument is unrelated to the change in non-EU8+2 migrants across wards. This provides

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<sup>16</sup>In our setting there is no pre-period that would allow us to assess parallel trends, or to run a placebo regression using electoral outcomes for the pre-period. As noted earlier, although the first elections to the London Assembly took place in 2000, no ward-level results were retained.

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additional support for our claim that the bus stops in Figure 3.2 are likely to affect the location decisions of EU8+2 migrants in particular, but not those of other foreign-born residents where the country of birth is further away or conveniently accessible via a wider range of travel options.

Columns five and six in Table 3.1 implement our instrumental variables (IV) approach. The second-stage point estimates of .67 (for UKIP's vote share) and .61 (for the BNP's) are positive and statistically significant at all conventional thresholds. In relative terms, the IV estimates are much larger than their OLS baselines, increasing ten-fold for the BNP and over sixty-fold for UKIP. These increases in coefficient size relative to the OLS results underscore the importance of modeling the location choices of migrants. The OLS estimates suffer from substantial bias due to immigrant sorting that masks the true effect of migration on support for far-right parties. Our instrument addresses this bias by identifying a channel which functions independently of the electoral dynamics we examine. Translated into the absolute number of votes, a one percentage point increase in EU8+2 migrants in a ward generated 27 additional votes for UKIP and 19 for the BNP (Table B.8). These are sizable impacts given that in 2012 these parties averaged 124 and 59 votes, respectively (Table B.3).

Table 3.1: Migration from new EU member states and support for far right parties in London

	OLS		First Stage	Placebo 1st Stage	2SLS		Reduced Form	
	UKIP	BNP	$\Delta$ New EU	$\Delta$ All other foreign-born	UKIP	BNP	UKIP	BNP
$\Delta$ New EU residents (pp)	0.01 (0.21)	0.06 (1.54)		-0.12 (-1.87)	0.67*** (3.53)	0.61*** (3.70)		
$\Delta$ All other foreign-born residents (pp)	-0.14*** (-5.18)	-0.11*** (-3.96)	-0.07 (-1.73)		-0.09* (-2.12)	-0.07 (-1.76)	-0.13*** (-5.03)	-0.11*** (-4.00)
$\Delta$ Unemployed residents (pp)	-1.11** (-3.30)	-1.20*** (-3.96)	1.00* (2.49)	2.91*** (5.70)	-1.42*** (-3.41)	-1.46*** (-4.07)	-0.75* (-2.21)	-0.85** (-2.85)
$\Delta$ Retired residents (pp)	3.25*** (5.37)	2.31*** (4.31)	-1.38 (-1.63)	-1.05 (-0.84)	4.20*** (4.91)	3.11*** (4.08)	3.28*** (5.44)	2.27*** (4.41)
$\Delta$ Student residents (pp)	-0.65* (-2.23)	-0.90*** (-3.75)	-0.03 (-0.15)	3.33*** (7.98)	-0.67* (-2.08)	-0.92*** (-3.48)	-0.69* (-2.34)	-0.94*** (-3.83)
$\Delta$ Median household income (£000s)	0.06 (1.48)	-0.02 (-0.68)	-0.09 (-1.22)	-0.06 (-0.71)	0.14* (2.05)	0.04 (0.75)	0.08* (2.09)	-0.01 (-0.31)
Median house price in 2001 (£000s)	0.01*** (3.54)	0.01*** (3.99)	-0.01*** (-4.73)	-0.00 (-0.76)	0.01*** (4.31)	0.01*** (4.72)	0.00* (2.39)	0.00* (2.54)
Distance from closest A6/757 bus stop (km)			-0.20*** (-5.62)	0.05 (0.92)			-0.14*** (-4.44)	-0.12*** (-4.48)
Observations	620	620	620	620	620	620	620	620
Borough fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
F-test on excluded instrument			31.56	0.84				
R <sup>2</sup>	0.74	0.70	0.61	0.65			0.75	0.71

Notes: OLS or 2SLS estimates with t-statistics based on robust standard errors. The dependent variable for columns 1, 5 and 7 is the percentage point change ( $\Delta$  pp) in votes cast for the UK Independence Party between 2004 and 2012. Columns 2, 6 and 8 have as outcome the percentage point change in votes cast for the British National Party between 2004 and 2012. The outcome variable in column 3 is the percentage point change in residents from new EU member states between 2001 and 2011, while the outcome variable for column 4 is the percentage point change in all other foreign-born residents during the same time period. Columns 1 and 2 estimate the OLS results. Column 3 estimates Equation 3.2, while column 4 is a placebo first stage, where we estimate the role of A6 and 757 bus stops on the settlement patterns of non-EU8+2 migrants. Columns 5 and 6 estimate Equation 3.3. The final two columns show the reduced form for the bus stop instrument. All  $\Delta$  in the control variables refer to the percentage point change between 2001 and 2011, except for median household income, which refers to the change between 2001/02 and 2012/13, the closest available data. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

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To round off our results, the final two columns in Table 3.1 present reduced-form regressions for both outcome variables. A statistically insignificant coefficient on our instrument in these regressions would result in no significant effect in our IV regressions, which is not the case. Conversely, a significant coefficient would be a particular concern if it coincided with no result in the first stage, indicating a violation of the exclusion restriction that our instrument works only through its effect on the location decisions of EU8+2 migrants. In our case, the coefficient on the instrument is statistically significant in both the first-stage and reduced-form regressions.

### **3.5.2 Robustness**

We assessed our empirical strategy against alternative approaches in related literature. Appendix B.6 provides a detailed discussion of these results. We use, in turn, initial distributions of EU8+2 migrants in 1991 or 2001 and versions of the shift-share instrument exploiting this information (Table B.6). As the 1991 census does not contain precise data on EU8+2 migration, we develop an imputation strategy. The resulting instruments are somewhat weaker, perhaps due to these data limitations. In contrast, instruments using the 2001 initial shares are strong. In addition, we also use an alternative transportation-related instrument. Analogous to our bus stop instrument, we calculated each ward's distance to the nearest of three train stations with direct connections to Luton (King's Cross/St Pancras) or Stansted (Tottenham Hale and Liverpool Street). Finally, we combine our preferred bus stop variable and either 1991 or 2001 initial shares as instruments (Table B.7). As it turns out, for each party we analyze, the IV estimates obtained with any of seven alternative instruments or their combinations with our bus stop instrument are very

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similar to our core results.

Further, we explore the sensitivity of our results to variations in control variables in Table B.10. First, we drop all controls. In this specification, our instrument is too weak. A more complete model of residential choice includes the affordability of housing. When we add median house prices in 2001 as a control, our instrument becomes highly relevant. We then introduce, in turn, borough fixed effects and demographic controls. This reduces the size of the coefficients of interest, but does not affect the pattern of results. Finally, we experimented with an index of deprivation widely used by policy-makers in the UK. This required dropping some other controls that are related to or incorporated into this measure, such as house prices. Our instrument is borderline weak in this specification. All of these alternatives yield positive and statistically significant estimates on our variable of interest. Our results are robust and our main specification produces the most conservative estimates.

### **3.5.3 Channels**

One potential complication is the possibility that EU8+2 migrants themselves directly influenced the outcome of these elections, as citizens from one EU country enjoy voting rights at the local level in other EU countries. We checked the extent to which EU8+2 nationals were registered to vote by matching ward-level data on electors by nationality that we requested from the electoral services managers of all 32 London boroughs, to census data we commissioned from the Office for National Statistics on the country of birth of the population of voting age (18 years or above).<sup>17</sup> We used this to approximate EU8+2 registration rates across 216 wards from the 11 boroughs that supplied data, yielding

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<sup>17</sup>Census 2011 Commissioned Table CT0796. There is a mismatch in the underlying definitions. However, few of these migrants were likely to have acquired British citizenship at this point. The latter would bias our estimates of registration ratios downwards.

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an average of 62.1% (for further details, see Appendix B.5). A sizable share of EU8+2 migrants did register to vote.

How plausible is it that these migrants themselves voted for the BNP or UKIP? Although we cannot directly verify their participation in elections, we find no significant effect of EU8+2 migration on overall turnout (Table B.9). To explore the composition of far-right party support, we turn to survey data. In 2016, wave eight of the internet panel of the British Election Study 2014-2023 (Fieldhouse et al. 2020) included the question: “Which party or independent candidate will you vote for to be your Assembly-wide member?” Out of 2,798 respondents, 282 declared they voted for UKIP. Of these 282, merely five had another EU country’s citizenship, four were Commonwealth citizens, and two declared another non-British citizenship. The EU category includes EU8+2 nationals but also respondents from the 15 member states of the EU prior to 2004. While results for the BNP are not available from this study, it is even less plausible that immigrants would vote for this ultra-nationalist party (John et al. 2006). This strongly supports the conclusion that the electoral response we document is driven by the voting behavior of the UK-born population.

This provides a basis to explore the channels through which this migration wave triggered far-right support among natives. The London election campaigns of UKIP and the BNP emphasised two perceived economic threats: competition for jobs and for housing. In our analysis, the unit of analysis is too small for a closed labor market, as most Londoners work in a different ward to the one they live in. It may therefore be difficult to detect this channel in our data. In terms of pressure on housing, this is likely to be keenly felt in the immediate neighbourhood. In addition, perceived cultural threat is an alternative channel that these



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and other far-right parties routinely exploit. The latter could be particularly relevant in this case, as significant language barriers impede sustained and meaningful contact with natives.<sup>18</sup> Allport (1954) proposed the inter-group contact theory as the mediating factor between out-group presence and attitude formation towards the other group. More recently, Steinmayr (2020) finds consistent evidence that far-right party support in Austria decreases with sustained interaction with refugees but increases with brief exposure.

To examine underlying mechanisms in more detail, following Edo et al. (2019) and Halla, Wagner and Zweimüller (2017), we adopt a split sample strategy that exploits variation in initial conditions across wards. Using data for 2001, we distinguish wards at or below the median, and those above it, in terms of house prices (as a proxy for overall housing costs) and unemployment rates.<sup>19</sup> Our expectation is that those living in areas where housing is relatively cheap would be more vulnerable to rising costs, and perceived job competition is likely greater where unemployment is already high. In addition, we probe a potential link to contact theory. Using our detailed census data on country of birth, we construct an initial Herfindahl-based diversity score for each ward, to distinguish those with relatively high pre-existing diversity from more homogeneous ones (e.g., Alesina et al. 2003).<sup>20</sup> Our assumption is that a subsequent influx of migrants will stand out more in areas that are initially less diverse, where residents are less likely to have had prior and sustained contact

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<sup>18</sup>Data from the 2011 Census shows that 21.1% of the new EU migrants living in the UK say they “cannot speak English well” or “cannot speak English”. In comparison, only 3.9% of old EU migrants report such values. The only groups which fare worse in terms of language acquisition are Bangladeshi (30.2%), Pakistani (22.9%) and Chinese nationals (22.5%).

<sup>19</sup>Housing costs are also linked to the quality of local services, especially schools.

<sup>20</sup>This measure is constructed by calculating and combining, for each ward  $w$ , the 2001 shares of each migrant category  $i$  as follows:

$$Fractionalization_{w,2001} = \left[ 1 - \sum \left( \frac{Residents_{c,w,2001}}{Residents_{Total,w,2001}} \right)^2 \right] \quad (3.4)$$

with (other) migrants, thus triggering a stronger electoral response.

Table 3.2: Exploring channels with split sample regressions using unemployment, house prices, and contact with EU10

	Main results	Unemployment		House prices		Prior diversity	
		Below median	Above median	Below median	Above median	Below median	Above median
I. UKIP							
$\Delta$ New EU residents (pp)	0.67***	0.56**	1.16	0.58***	0.16	0.63*	1.27
	(3.53)	(2.58)	(1.83)	(3.78)	(0.81)	(2.35)	(1.11)
II. BNP							
$\Delta$ New EU residents (pp)	0.61***	0.60**	0.79	0.42**	0.40*	0.67**	0.75
	(3.70)	(3.02)	(1.42)	(2.99)	(2.45)	(2.81)	(1.09)
Observations	620	310	310	308	312	311	309
F-test on excluded instrument	31.56	25.94	4.35	40.69	11.90	24.98	1.17
Borough Fixed Effects	✓	✓	✓	✓	✓	✓	✓
Control variables	✓	✓	✓	✓	✓	✓	✓

Notes: 2SLS estimates with t-statistics based on robust standard errors. Further details are in the notes of Table 3.1, and Appendix B.4 provides sources for the variables used to create the split samples. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Results are reported in Table 3.2. Our instrumental variable is weak in some subsamples, which means we cannot make meaningful comparisons relating to initial levels of unemployment or diversity. We do, however, get a clear result relating to housing costs. Here, the effect on UKIP support is almost four times larger in wards where initial house prices are low, but there is no difference in support for the BNP.<sup>21</sup> The perceived economic

<sup>21</sup>Using the same data source, we calculate that actual increases in housing costs over the following decade are very close to the overall sample mean of 85% in both subgroups. However, such an increase might be more keenly felt by those dependent on affordable housing.

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threat of migration to the affordability of housing seems to have played a role in the electoral success of UKIP. This is consistent with findings by Fetzner (2019), who shows that austerity measures that cut housing-related benefits boosted electoral support for UKIP and eventually led to the success of the “leave” campaign in the 2016 Brexit referendum. While Fetzner shows large post-2004 UKIP gains in coastal regions, Wales, and some industrial areas of the Midlands, our more granular analysis below the local authority level reveals that the same warning signs existed in metropolitan areas as well. The fact that this mechanism does not explain support for the BNP suggests that the two parties appeal to different electorates. Support for the BNP seems to be driven more strongly by other channels, possibly concerns about national or cultural identity.

### **3.6 Conclusion and implications**

To examine the electoral consequences of migration, we draw on two separate strands of research: a growing political economy literature on the electoral impact of migration at the local level, and the transport and human geography literature related to migration. Our empirical work exploits substantial heterogeneity of changes in the composition of residents across 620 wards in London following the EU’s Eastern enlargement in 2004 and 2007. The enlargement brought a policy shift by disabling legal barriers to migration from the region and coincided with a boom in the low-cost aviation industry that ensured regular and affordable flight connections. We show that travel infrastructure shaped the location decisions of these highly-mobile migrants, which in turn changed the electoral geography of support for far-right parties. Based on this, we estimate that a one percentage point increase in the share of migrants from the EU’s Eastern accession countries increased the vote shares of the two major anti-immigrant far-right parties, UKIP and the BNP, by

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about two-thirds of this amount. This is very close to the average effect calculated in a recent meta-analysis of studies investigating other Western European countries, which did however highlight very substantial heterogeneity in estimates (Cools, Finseraas and Rogeberg 2021).

Our work makes several contributions to the study of migration and its electoral consequences. First, much of this literature has focused on groups with typically infrequent post-migration travel to their country of birth, including refugees. Yet the population of migrants is extremely heterogeneous. Prior studies incorporate different distinctions, for instance between high and low-skilled migrants (e.g., Halla, Wagner and Zweimüller 2017; Mayda, Peri and Steingress 2018). We are the first to study the effect on electoral outcomes of highly-mobile migration, which is particularly relevant in settings where policy-makers promote regional economic integration. Our work suggests that a distinction between highly-mobile and less-mobile migrants deserves further exploration. For instance, given that mobility may affect pressures to assimilate, future work should assess the integration trajectories of more and less mobile migrants, and observe how these patterns influence or hinder opportunities for sustained contact with the native population.

Our findings also highlight important nuances in how migration affects support for different types of far-right parties. While both populist as well as fascist far-right parties benefit from migration, they do so for different reasons. In our case, UKIP strongly benefits from migration in areas that rely on affordable housing, which is not an important channel for the BNP. This is consistent with work by Fetzer (2019), showing that austerity measures in the form of cuts to housing-related benefits increased support for UKIP. Our analysis

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suggests that warning signs about the political consequences of such actions were already present in 2012, the year prior to the implementation of these measures. This also implies that effective strategies to counter far-right support need to recognize distinct channels through which subsets of voters are attracted to different types of far-right parties.

In addition, our approach expands the methodological toolkit for the study of the consequences of migration, which thus far has relied heavily on versions of the shift-share instrument in attempts to tackle endogeneity of settlement choices among new migrants. Yet over-reliance on this strategy has been a growing concern among migration scholars (Jaeger, Ruist and Stuhler 2018). Alternatives are also needed where data constraints preclude or limit the usefulness of this approach. Our paper provides such an alternative. Drawing on a hitherto separate literature on transport and human geography, we develop an instrumental variable based on the proximity to travel hubs that are of specific importance to the migrant group we study. This offers a potential solution to researchers who face similar data constraints, and it can serve as a robustness check for studies that primarily rely on the traditional shift-share approach.

We see significant potential to deploy our approach across a wider range of settings to analyze the electoral consequences of migration. One of the distinguishing features of the wave of migration into the UK that we study is that it resulted in settlement patterns that are more geographically spread out than previous waves of migration (Kone 2018). This may well be linked to the evolution of flight networks across the UK during this period, and similar relationships may hold in other countries where low-cost travel boosted international mobility. Moreover, the flip side of the phenomenon we examine is emi-

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gration, which affects the demographic composition of communities that are left behind. Recent scholarship exploits geographic heterogeneity in emigration to analyze its electoral consequences (Anelli and Peri 2017). In this area, too, future work could deploy empirical strategies based on pre-existing transportation links.

Our analysis also suggests policy implications at a time when migration is a major theme in political debates in many countries. In the EU, policy debates relating to the liberalization of travel markets and increased mobility have highlighted economic benefits, with claims of low-cost airlines “bringing Europe closer together” (Akgüç, Beblavý and Simonelli 2018). Our work highlights a political dark side of mobility that pulls into the polar opposite direction. In drawing lessons from Brexit for the prospects of European integration, and regional economic integration elsewhere, discussions of cross-national mobility should look beyond economic benefits. Policy-makers who promote mobility must also pay attention to its potential political consequences and consider how they might be mitigated.

## **Chapter 4**

# **Naturalisation: Brexit and the Making of New Citizens**

The Brexit referendum led to a surge in naturalisation applications from EU residents, yet little investigation has been done on the profile of the new citizens. We know even less about what motivated them to naturalise and how will these drivers affect their social and political preferences in the future. To fill this gap, the current paper uses administrative data from the Home Office, the Office for National Statistics (ONS) and the Organisation for Economic Co-operation and Development (OECD) to identify when and who applies for British citizenship. I connect the uncovered naturalisation drivers and prevailing socio-demographic characteristics of those who apply for a British passport with the literature on political engagement and civic duty. This allows me to describe the profile of new British citizens, develop hypotheses about the factors motivating their decision to take up British passports and inspire future work on their political participation.

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The results suggest that the 2016 referendum on leaving the European Union served as a naturalisation trigger for many European residents who might not have ready to apply for citizenship. The referendum therefore had the potential to produce a series of citizens with weak British identities, but strong incentives to remain connected to both their home and host society. While this is partially true, the analysis in this paper suggests that there is another side to the story. The referendum seems to have served as a filtering mechanism: those not fully integrated returned to their home countries in the aftermath, while the “remainers” were more likely to naturalise to more obstacles they faced (e.g. financial considerations or dual nationality restrictions). These findings have widespread implications, as many political parties lack experience in engaging with this previously disenfranchised group. First and foremost, the current research enables policy makers to explore the heterogeneous composition of the new electorate when engaging with them. This paper contains the necessary ingredients for successful targeted engagement with various segments of the enfranchised EU community in the UK. Secondly, it advocates for greater integration of different migrant groups in national survey studies. Lastly, the paper empowers EU migrants to use their newly gained voting rights to engage with British politics and make a real impact for themselves and other immigrants.

EU-born residents were the largest immigrant group in the United Kingdom at the time of the 2016 referendum (James 2020: Figure 2) and had around 1.9 million representatives (4.5% of all registered voters) featured on the electoral registers in England and Wales (Uberoi and Johnston 2018). Prior work on the electoral consequences of migrant enfranchisement suggests that if the enfranchised group is large enough and sufficiently active in politics, they can affect democratic politics (Vernby 2013). Furthermore, naturalised EU



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migrants have an unusual profile due to two characteristics: 1) those who self-select into applying for citizenship have very different socio-economic profiles (e.g. wealthier, more educated and more integrated) than the average migrant; and 2) unlike other migrants and the native population, EU migrants contribute more to public finances than they detract (Dustmann and Frattini 2014). All these characteristics make them in principle more likely to empathise with the Conservative party on the economic dimension, while their cultural background suggests that the Labour party's pro-immigration policies could represent their interests better. This inherent duality is indirectly explored throughout the paper. If we are to avoid losing touch with this new generation of voters, more efforts are necessary to understand their naturalisation journey and how it may affect future engagement with the host country. The current paper explores various avenues connecting the naturalisation journey pre- and post-Brexit with the profile of the average EU-born British citizen in the hopes of gauging a better understanding of their future political engagement and preferences.

I set forward to summarise the existing literature addressing questions about (1) who naturalises?, (2) among the newly naturalised citizens, who has a higher propensity to vote? and (3) who are they likely to vote for, given their socio-economic background? The data and empirical strategy sections are then followed by the results, where I explore how the 2016 referendum on leaving the European Union has shaped naturalisation decisions. I conclude on an optimistic note, arguing that EU voters, due to their sheer size and profile, have the power to change British politics if they become civically active.

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## 4.1 Understanding the EU-born electorate

### 4.1.1 Who is more likely to become a citizen?

Policy changes and the perceived added value of (another) citizenship are among the most prominent reasons explored in the literature for why people naturalise (Reichel and Perchinig 2014). The Brexit referendum serves as a perfect opportunity to test this assumption, as it followed a series of policy changes which simplified the complex residency registration system for EU nationals who needed to fulfil the post-Brexit legal residency requirements. Inadvertently, simpler residency processes also led to a simplified naturalisation process for EU migrants, although this was not a mandatory step (unlike the residency registration). The referendum therefore provided an opportunity for eligible EU nationals to obtain British citizenship easier and cheaper than before. It also served as a mean of differentiating between those who value becoming British and those who do not or between those who can and cannot afford it. Due to a myriad of unobservable factors, studying the effect of reduced application costs on individual-level naturalisation decisions in a causal framework is difficult. However, focusing on general trends can provide a more convincing portrayal of the role bureaucracy plays in naturalisation decision, even though the individual motivations may remain unknown.

*H1a: All else equal, EU nationals will naturalise in higher numbers after 2016 than in previous years.*

Legal constraints and eligibility requirements are just some of the considerations shaping selection into treatment. For example, studies have showed that the permissiveness of

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dual citizenship laws has a positive effect on the propensity to naturalise (Mazzolari 2009). While the UK does not require new citizens to renounce previously held citizenship(s), other countries may impose such restrictions. I therefore expect that those coming from EU countries who do not allow dual nationality naturalised in far fewer numbers before the referendum than those who were permitted to hold multiple passports. I expect the direction of the relationship to have remained unchanged after 2016, although the application gap between those who are and are not allowed to hold multiple nationalities to have narrowed.

*H1b: All else equal, those who have to renounce previously held citizenship(s) in order to get a British one will naturalise less than those who do not face such restrictions, irrespective of treatment status.*

Naturalisation can be seen as an opportunity structure to improve the living conditions and perspectives of foreign residents (Reichel and Perchinig 2014). It can bring the new passport holder more rights within the UK and/or more rights when travelling internationally. In terms of internal residency and employment rights, EU nationals were already enjoying equal opportunities as native-born British nationals prior to the referendum, so the incentives to naturalise were low. The results of the 2016 vote created new domestic incentives to apply for a British passport, as basic residency rights became uncertain. From an external rights perspective, the more opportunities a British passport offers compared to the passport of the home country, the more valuable it becomes. This being said, all EU passports are among the top 30 most powerful passports in the world in terms of travel freedom (Kochenov 2016). UK was ranked 9th in 2018, after all other old EU member

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states but before all the new member states joining on and after 2004. If this argument is to be believed, non-EU migrants are the ones gaining the most from obtaining a British passport, followed by new EU migrants (EU8 and EU2), while old EU migrants have limited external gains.

*H1c: Prior to the Brexit referendum, citizens of new EU member states naturalised more. After 2016, all EU nationals will naturalise in higher numbers than in previous years.*

However, not all residents have the time and resources to apply for citizenship. Since 2009, a series of laws and policies have transformed the naturalisation process in the UK into one of the most bureaucratic and expensive in Europe (Huddleston and Niessen 2011). Some policy makers have argued that tougher naturalisation rules and a highly selective process will lead to a more homogeneous population, with similar values and lingo-cultural characteristics (Johnson et al. 2001). These costs have been shown to place barriers to citizenship uptake, especially among immigrants lacking language skills, or those with limited financial resources to pay the application fees (Hotard et al. 2019). Furthermore, other findings suggest that economic, political, social, cultural and geographical conditions in the country of origin are also significant predictors of citizenship acquisition (Yang 1994). We know that Eastern and Western European residents in the UK differ significantly in aspects linked by these studies to their propensity to take up citizenship (e.g. likelihood of further or return migration<sup>1</sup>, educational attainments<sup>2</sup> and economic security<sup>3</sup>) (D'Angelo

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<sup>1</sup>Eastern Europeans were found to be more likely to return to the countries of origins when their economies fair better than the UK's economy (Pollard, Latorre and Sriskandarajah 2008).

<sup>2</sup>Western and Southern European residents are much more likely to hold a degree than British nationals, while migrants from Eastern Europe are less or equally educated compared to native British nationals.

<sup>3</sup>Western and Southern European residents are much more likely to hold managerial or professional jobs, while Eastern Europeans are considerably more likely to be paid less than the first group.

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and Kofman 2018).

*H1d: Western European migrants are more likely to naturalise than Eastern European residents.*

#### **4.1.2 Who is more likely to vote?**

Once naturalised, foreign-born residents enjoy full voting rights in all UK elections. However, studies have shown that some nationalities have a higher propensity to vote than others. This section will identify the most common aggregate-level characteristics associated with civic participation and will use these findings to set forth a series of theoretical expectations related to future turnout of naturalised EU nationals in British elections. While this paper will not directly test these hypothesis, they can pave the way for future research and help us understand the implications of the current findings.

The first step in determining propensity to vote is to identify who is more likely to register to vote. Registration rules could present a real barrier to participation, especially across less educated and poorer residents. Recent arrivals, those without British citizenship and those with lower English language skills are less likely to register to vote (Ford and Grove-White 2015; Heath and Khan 2012). The Electoral Commission has acknowledged that the registration rules are affecting disproportionately students, young voters, certain Black and Minority Ethnic groups, as well as enfranchised foreign-born groups. Commonwealth and EU citizens have been identified as some of the least likely groups to be registered to vote (Ziegler 2018). Therefore, electoral register rules are believed to have suppressed interest and participation in local politics among EU-born nationals. One explanation for

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why political engagement among foreign-born residents is lower than the national average (Heath and Khan 2012) is the high costs associated with learning about a new electoral system, about procedures and parties which may be starkly different from the ones they had been used to in their country of birth. However, such trends are ameliorate during the naturalisation process. While studying for the “Life in the UK” exam and attending the citizenship ceremony, future citizens receive vast information about the British political system and are exposed to strong nudges to become civically active.

*H2a: Among residents who have voting rights prior to naturalisation, registration rates will be higher for those who have British citizenship than those who do not.*

Only 54% of EU residents were registered to vote in 2018, compared to 62% of Commonwealth and 86% of British residents (Electoral Commission 2018). A 2019 constituency level breakdown of EU nationals’ registration levels finds London boroughs to have the highest registration levels in the country (e.g. Brent: 18.4%, Kensington and Chelsea: 17.6%, Newham: 16.6%). Outside of London, the highest number of EU voters were reported in the Boston and Lincolnshire local authorities (14.8%) (Uberoi and Johnston 2018). Therefore, geography matters. Both the country of birth and the geographical settlement within the UK should be factored in when estimating people’s intentions to vote.

The literature suggests that vote turnout in a new country is associated with political participation in home countries (Black, Niemi and Powell 1987; Wals 2011). Looking at the aggregate level, one would expect that, all else equal, residents socialised in countries

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with compulsory voting will be more politically engaged. Similarly, those coming from countries with significantly higher turnout levels than the UK will be more likely to participate than those coming from countries with significantly lower turnout. Furthermore, age is expected to be a relevant mediating factor here, as studies have found that young voters are less impacted by the civic duty argument even in countries with compulsory voting (Quintelier, Hooghe and Marien 2011). This is perhaps associated with the argument that voting is a muscle, and the more one participates in previous elections (Niven 2004) or engages with political parties (Ramakrishnan 2005; Wong 2006), the more likely they are to turn into a habitual voter. 84% of European migrants residing in the United Kingdom are of working age and have a relatively young age profile (Sirkeci et al. 2018). Since young people have been consistently found to display less interest in politics and to have lower voting rates than their older counterparts (Fraga and Holbein 2020), one would expect that the lower the average age of a naturalised citizen from any given country, the less likely that group will be to participate in British politics (irrespective of political socialisation).

*H2b: New citizens who grew up in a politically active society are more likely to participate in British politics than those who come from less politically active societies.*

*H2c: All else equal, the younger the average naturalised citizen from a given country is, the less likely that national group will be to turnout to vote.*

Furthermore, electoral participation seems to fluctuate across different types of elections. For example, the foreign born electorate in Scandinavian countries was found to participate in higher numbers in local elections (Bergh and Bjørklund 2011; Bevelander and Pendakur

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2008; Togeby 1999). Holding a British passport could motivate new citizens to engage more with politics, although they may still feel more comfortable participating in some elections but not in others. Linking once again to the political socialisation aspect, one might expect that EU citizens will be more likely to vote in local elections than national ones even after naturalisation, especially if they participated in similar elections prior to becoming British citizens.<sup>4</sup>

*H2d: All else equal, EU citizens who participated in local elections prior to naturalisation are more likely to vote in both local and national elections after taking up British citizenship.*

One final aspect found to impact political participation is the projected efficacy associated by each voter to their ballot. The most prevailing finding in the literature is that class and other social cleavages negatively impact efficacy and consequently decrease political engagement. According to valence theory, poverty and discrimination affect political engagement and make those who experience them less likely to participate in politics (Clarke et al. 2011). Migrants experience on average more discrimination in the labour market and in daily life than the native population (See for example Wood et al. 2009; Li and Heath 2010). These discriminatory experiences further vary across migrant groups (Lessard-Phillips 2017). Recent studies on Eastern European migrants suggest that they experience more labour market discrimination than Western and Southern European migrants, and even compared to other non-white migrant groups (Johnston, Khattab and Manley 2015). These findings explain not only why migrants are less politically active

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<sup>4</sup>UK residents born in another EU member state have the right to vote in local elections, irrespective of their British citizenship status.



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than natives, but also why some migrants are more likely to vote than others.

*H2e: Naturalised Eastern European migrants (those born in one of the new EU Member States) are less likely to participate in politics than Western European migrants (those born in one of the old EU Member States).*

Irrespective of the motives for abstention, politicians tend to respond to the interests of voting citizens over non-voting ones. For this reason I move my attention in the next section to the political preferences of active foreign-born voters.

### **4.1.3 Who are the politically active likely to vote for?**

What do we know about the voting patterns of EU-born electors? The truth is that we have limited information about all immigrant residents, but the information is particularly scarce in respect to foreign-born groups who, like EU migrants, do not have voting rights in high stakes elections. Being a relatively small and hard to reach section of the population, many well-established public opinion surveys fail to include a representative sample of non-native residents. Moreover, voting behaviour research focuses predominantly on high-stakes elections (e.g. general elections). This means that groups who cannot vote in national elections are not included in the main electoral studies or prediction models.<sup>5</sup>

Is the immigrant vote driven by specific policies or partisan affinities? Unlike other countries (in the United States: Alesina, Baqir and Easterly 1999; Vernby 2013: in Sweden), the consensus from political behaviour studies of migrant residents in the UK is that immi-

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<sup>5</sup>For example, the British Election Study (BES) and even its Ethnic Minority sub-study (EMBES) failed to capture the political preferences of EU migrants in the UK because they had historically low naturalisation rates.

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grants' electoral behaviour is not driven by policy interests, rather by party allegiances. Foreign-born electors in the UK stick to party lines even when the vote goes against their preferred policy. Dancygier and Saunders (2006) find that immigrants in Britain do not depart significantly from the native population in terms of support for social spending or redistributive policies. If anything, they hold more conservative preferences on these dimensions than otherwise similar natives. Yet the majority of immigrants end up overwhelmingly supporting the Labour party (Saggar and Heath 1999). These findings suggest that partisanship is more salient than policy preferences for British migrants.

A frequently cited reason for the pro-Labour attitudes is the profile of the immigrant voter, which matches that of the average Labour voter: younger, more likely to be uneducated and unemployed, lower income, and more dependent on social housing (Pons and Liegey 2019). The political agenda of parties on the left can be more appealing to people with these characteristics. Another frequently cited reason is that immigrants side with parties which promote race equality. Historical responses to immigration have a lasting impact on political loyalties and the "non-white British" support for the Labour party in the UK is indeed deeply rooted in history. The country's colonial past brought the first wave of migrants on British shores as early as 1950s and 1960s. These early migrants came to settle in Britain as British citizens, having full political rights. However, they received no assistance from the government to settle or assimilate. The Conservative party was particularly hostile towards them, campaigned for immigration restrictions (Dancygier and Saunders 2006). The Labour party on the other hand was more open to immigration and it was perceived as a protector of migrants and minorities (Khan 2015). Today's newcomers may not have any memory of that period, but the stories portraying the Conservative party

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as the “nasty party” continue to shape voting patterns among enfranchised new arrivals.

*H3a: All else equal, EU residents are more likely to vote for Labour compared to their native-born counterparts.*

Among non-Labour voters, allegiances have been equally split between Conservatives and Liberal Democrats. Only 2 percent of minority non-Labour voters supported smaller parties (Heath and Khan 2012). However, the 2015 election saw a significant pro-Conservative shift among minority groups. A Survation poll conducted for British Future in May 2015 ranks the parties’ popularity among non-white British electors as follows: 1) Labour party (1.5-1.6 million votes in the 2015 General Election), 2) Conservative party (approximately one million votes), 3) Liberal Democrats (approximately 150,000), 4) Green party (approximately 150,000) and 5) UKIP (approximately 60,000 votes) (British Future 2015). The takeaways from this election are that (1) preferences of the minority have started in recent years to converge with those of the majority population and (2) smaller parties (including the Liberal Democrats) are losing ground in front of the Conservative party, who now appeals more and more to foreign-born non-Labour supporters who also want to vote strategically. If the Labour party continues to rely on the historical support received from non-native electors, they will face mass desertion. Therefore, the vote calculations of newly naturalised Europeans are strikingly similar to those made by the rest of the electorate. In England at least, migrants have to chose between Labour and the Conservatives, or face a high chance of wasting their vote.

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*H3b: The political preferences of European migrants will converge to those of the native white-British, the longer time they spend in the country.*

While the expectation is that in the long term the average profile of the newly naturalised EU-born voter will start resembling the median voter, more efforts should be made to differentiate between and within migrant groups. Generally speaking, the more successful a migrant group is in the labour market, the higher propensity the group will have to vote for the Conservative party. On the other hand, unemployment, poverty and economic discrimination are associated with an increased likelihood of voting for the Labour party (Khan 2015). The overall class composition of the group can also explain differences in political behaviour. For example, if the overall class distribution of any national group is similar to that of white British (e.g. significant numbers of middle class families), this structure is likely to embed a feeling of fairness and economic prosperity in the community. The Conservative rhetoric will resonate better with these groups, even across social classes. At the other end of the spectrum, if the middle class is relatively small and the perceived risk of unemployment is high even among middle class families, the group overall will perceive Labour's political discourse to be more aligned with their economic and social interests (Khan 2015). As previously discussed, Eastern European migrants are more likely to fall within the latter group, while Western Europeans fit the description of the former group (Johnston, Khattab and Manley 2015).

*H3c: All else equal, the average naturalised Western European migrant in England is more likely to vote Conservatives, while the average Eastern European is more likely to support Labour.*

## 4.2 The profile of the naturalised EU citizen before and after Brexit

There are two routes to obtaining British citizenship: registration and naturalisation. This paper will only focus on the naturalisation path. This route incorporates adults who have resided in the United Kingdom for at least five years, or have been a spouse or civil partner of a British citizen and resided together in the UK for at least three years. Aside from residency requirements, successful applicants need to meet ‘good character’ requirements, language requirements (ability to communicate in English, Welsh or Scottish Gaelic), and knowledge of “life in the UK” assessed via a 45 minutes and 24 questions test based on information presented in the Life in the United Kingdom book. Since the costs were high<sup>6</sup> and the incentives low, very few EU nationals applied for British citizenship prior to UK’s decision to leave the European Union.

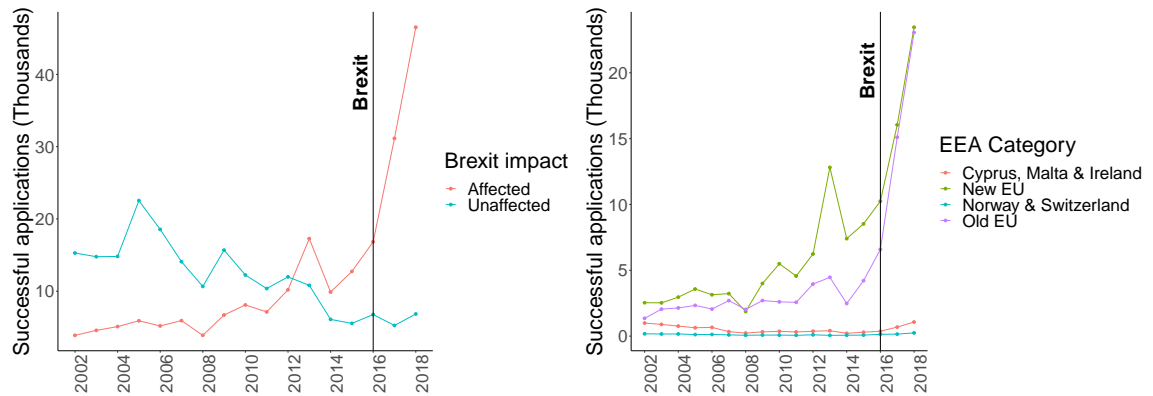


Figure 4.1: Successful naturalisation applications across European states  
 Figure 4.2: Successful naturalisation applications across EEA

The Brexit referendum changed this. As Figures 4.1 and 4.2 indicate, interest in British citizenship was already on the rise before the referendum results were announced. EU residents began preemptively applying for citizenship since the referendum was announced

<sup>6</sup>The cost of the application alone was £1330 in November 2020.

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in 2015. However, it is not until 2016 when it really picked up. As soon as the results were revealed, residents of both old and new EU member states mobilised and applied for British citizenship. The referendum served as a naturalisation catalyst, quadrupling the difference in successful applications between affected and unaffected individuals over the span of just two or three years (2015/2016 vs 2018). European countries who are not part of the EU, on the other hand, show low and stable naturalisation rates, as do countries who have separate bilateral or multilateral agreements with the UK (Cyprus, Malta and Ireland) and EEA countries who are not officially part of the European Union (Norway and Switzerland).

Can these patterns be a by-product of increased globalisation? It is possible that the increase in successful applications was an artefact of mass migration of European nationals to the UK, and not related to the referendum. I use data from the Annual Population Survey to construct the percentage of the total EU-born residents living in the UK who apply for British citizenship every year. This measure accounts for annual migratory trends and portrays a more robust image of the degree of interest in British passports. The percentage of EU residents who naturalised in the United Kingdom quadrupled (from 0.5% to 2%) in 2018 compared to the average pre-referendum year (Figure 4.3). Furthermore, in 2018, EU migrants overtook non-EU residents in relative naturalisation trajectories, with almost 2% of the total EU resident population deciding to naturalise in 2018 alone. The groups who responded the strongest to the referendum are EU14 and EU2, followed closely by EU8 (Figure 4.4).

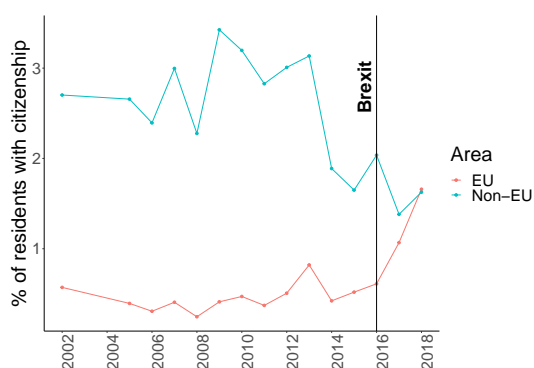


Figure 4.3: Successful naturalisation applications as percentage of the total population of EU and Non-EU residents in the UK

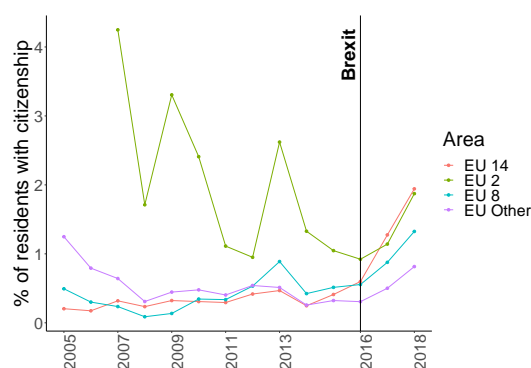


Figure 4.4: EU sub-populations

With such a sharp increase in citizenship applications, one may wonder if the profile of the naturalised EU resident changed dramatically after the 2016 Brexit referendum. Table 4.1 suggests that it has not. The median citizen continues to be male, aged 35-44. The characteristics of the home country also stay constant. The median naturalised citizen continues to come from a EU country which allows dual citizenship and does not have compulsory voting, but does have a higher electoral turnout than in the UK and a higher unemployment rate than in the UK. Therefore, these simple summary statistics seem to indicate that the referendum did not attract a more diverse pool of citizens, it only accelerated the speed to which those who might have eventually naturalised did so. The number of naturalisations almost quadrupled after 2016 and the percentage of the resident population from each country who decided to naturalise in any given year more than doubled on average after the referendum.

Table 4.1: Characteristics of successfully naturalised EU residents before and after the referendum

	Before 2016			On or after 2016		
	mean	median	sd	mean	median	sd
Naturalisation (N)	337	172	584.6	1313	629	1790.1
Naturalisation (%)	0.46	0.32	0.54	1.13	1.21	0.51
Sex	1	1	0.6	1	1	0.5
Age	5	5	2.5	5	5	2.5
Dual citizenship	2	2	0.5	2	2	0.5
Compulsory voting	0	0	0.3	0	0	0.4
Turnout higher than UK	1	1	0.5	1	1	0.5
Unemployment higher than UK	1	1	0.4	1	1	0.4
Observations	287			72		

### 4.3 Data and empirical strategy

The analysis will combine several data sources to explore how push and pull factors interact and influence naturalisation decisions among EU nationals affected by the referendum results. The first data source is commissioned from the Home Office (2002-2018) and it consists of annual applications for British citizenship (successful and declined) by country of birth, gender, age and reason for applying. Another data source is the MACIMIDE Global Expatriate Dual Citizenship Dataset (Vink, De Groot and Luk 2015) measuring restrictiveness or permissiveness towards dual citizenship around the world from 1960 to 2020. I also use annual population estimates data (2004-2018) from the Annual Population Survey (2021), portraying a breakdown of all UK foreign-born residents by broad geo-



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graphical region of origin in order to construct the percentage of the total population that naturalises each year. The combined dataset consists of annual successful naturalisation application totals by country of origin between 2005 and 2018.<sup>7</sup> I apply a difference in differences design:

$$Y_{c,t} = \alpha_c + \lambda_t + \beta \text{Affected by Brexit}_{c,t} \times \text{Dual nationality permitted}_{c,t} + \varepsilon_{c,t} \quad (4.1)$$

where  $Y_{c,t}$  is the outcome of interest for residents born in country  $c$ , who naturalise in year  $t$ . Two outcomes are investigated: the absolute number of successful citizenship applications and the *estimated* percentage of UK residents who naturalised in any given year by country of birth. The latter outcome is derived from Annual Population Survey (2021) and calculated in two steps. First, I estimate the resident population from each country for each year based on naturalisation distributions. I make the assumption that the share of naturalised migrants is equal to the share of residents from each country, based on geographical area totals (Equation 2). Secondly, I use the population estimates calculated in Equation 3 to estimate the percentage of residents from each country who naturalise each year in the UK (Equation 4).

$$\text{If we assume that: } \frac{\text{Naturalisation}_{country,t}}{\text{Naturalisation}_{area,t}} = \frac{\text{Population}_{country,t}}{\text{Population}_{area,t}} \quad (4.2)$$

$$\text{,then: } \text{Population}_{country,t} = \frac{\text{Naturalisation}_{country,t}}{\text{Naturalisation}_{area,t}} \times \text{Population}_{area,t} \quad (4.3)$$

$$\% \text{ naturalised residents} = \frac{\text{Naturalisation}_{country,t}}{\text{Population}_{area,t}} \times 100 \quad (4.4)$$

The treatment variable, *Affected by Brexit* $_{c,t}$ , is dichotomous, turning from 0 to 1 after 2016 for all residents born in a EU country  $c$  affected by the referendum who live in the UK.

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<sup>7</sup>An analysis which breaks down the country level by age and gender can also be found in Appendix C.2.

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The DID estimator of interest in Equation 4.1 is  $\beta$ . It tests for heterogeneity effects by dual nationality restrictions and captures the effect of being affected by the referendum on the outcome. The mediating variable, *Dual nationality permitted*, is also binary, taking value 0 if taking British citizenship implies automatic loss of citizenship of origin country (restrictive policy) and 1 if there is no automatic loss of citizenship (permissive policy). I account for country  $\alpha_c$  and year  $\lambda_t$  fixed effects and control for the incumbent party in the UK. All models use clustered standard errors at the country of origin level.

Economic indicators help individuals decide whether to remain in the United Kingdom and naturalise or return to their home country in the face of uncertainty caused by the referendum. To control for such factors, I use data from the OECD (2021) which contains unemployment rate indicators by gender and age group for all OECD countries and six selected non-OECD economies (Brazil, China, India, Indonesia, Russia and South Africa). The focus on OECD countries stems from a desire to compare similar labour markets through reliable statistics. Non-OECD countries can have huge unemployment rates (as we see in this case also through the introduction of South Africa in the sample), more bias and less transparent reporting of official statistics. Therefore, the decision was made in favour of trading sample size for reduced bias. Thus the number of distinct countries drops from 176 in the previous model to just 29. However, the unit of analysis shifts from country-year to country–age–group–gender–year<sup>8</sup>, thus increasing the sample size from 6 to 36 observations per country. Furthermore, the time span of this data set is also restricted, unemployment values having only been released for the years 2005, 2010 and 2015-2018.

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<sup>8</sup>Continuing with country-year as unit of analysis does not make sense statistically or theoretically. The statistical advantages are obvious (174 vs 1354 observations). Theoretically, it is misleading to assume that everyone has equal unemployment opportunities within a country. Age and gender are among the most prominent and cross-nationally relevant dimensions of unemployment variability. Ideally other measures such as ethnic or religious minority status in both the country of origin and the UK would have also been good proxies. In the absence of such data, the current breakdown is the best possible alternative.

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The unemployment data was released for the following age-groups: 15-24, 25-54 and 55-64.

I use the OECD data to construct a measurement of relative economic performance, capturing the difference in unemployment between UK and the country of birth. This is a continuous variable, normally distributed and centred around zero. It was calculated by subtracting the unemployment level (%) for each age and gender in the country of birth from the equivalent unemployment level in the UK for each available year. A negative value on this indicator implies that the unemployment in the UK is lower than in the country of birth, while a positive percentage score suggests the opposite. A value of 0 implies equal unemployment levels. The unemployment differentials distribution is fairly similar among the full OECD +6 country sample and the EU OECD countries, as reflected in Figure 4.5 below. The unemployment differentials range from -48% (South Africa in 2016) to +14.2% (Switzerland in 2010) for the full sample and between -42.3% (Greece in 2016) to +12.4% (Austria in 2010) for the EU countries impacted by Brexit.

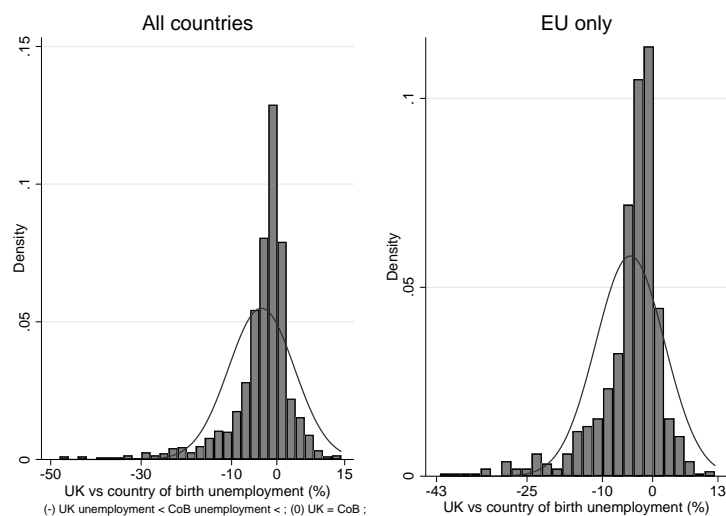


Figure 4.5: Distribution of UK unemployment relative to unemployment in the country of origin

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Once again a difference-in-differences design is implemented:

$$Y_{g,a,c,t} = \alpha_c + \lambda_t + \rho_g + \sigma_a + \beta \text{Affected by Brexit}_{g,a,c,t} \times \\ \times \text{Dual nationality permitted}_{c,t} \times \text{Unemployment(UK vs CoB)}_{g,a,c,t} + \varepsilon_{g,a,c,t} \quad (4.5)$$

where  $Y_{g,a,c,t}$  are the two outcomes of interest (the absolute number of successful citizenship applications and the estimated percentage of residents who naturalised in any given year) for residents of gender  $g$  and age group  $a$ , born in country  $c$ , who naturalise in year  $t$ . I account for country  $\alpha_c$ , year  $\lambda_t$ , gender  $\rho_g$  and age group  $\sigma_a$  fixed effects, control for the incumbent party in the UK and use cluster standard errors at the gender-age-country level.

The parallel trends for both samples are in Appendix C.3.

## 4.4 Results

I start by implementing the model proposed in Equation 4.1. Table 4.2 shows how dual nationality restrictions interact with the referendum treatment in encouraging or suppressing naturalisation. I start with a simple univariate linear regression model and show that there have been on average 438 more successful citizenship applications from Brexit affected nationals than unaffected ones. However, this translates into 2.10% fewer residents born in the European Union deciding to naturalise during the entire study period (2005 and 2018) compared to non-EU residents.

Speaking to *H1c* and *H1d*, Model 2 compares naturalisation rates of “new” and “old” member states before and after the referendum. I find that before the referendum Eastern European migrants naturalised more than Western Europeans (on average 199 more

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successful applications in the average year, equivalent to 0.3% more of the residential population). After the referendum however, the difference between the two groups becomes statistically indistinguishable in absolute numbers. When comparing percentages, Western European migrants actually overtook Eastern European ones by 0.62%, a statistically significant difference at all conventional confidence levels. I conclude that citizens of new EU member states did indeed see a higher value in the British passport prior to the referendum results, but old EU member states closed the gap and even overtook the first group in proportional terms after the referendum once more incentives to acquiring citizenship arose. These findings once again highlight the importance of differentiating between migrant groups and echo the findings in the existing literature that Eastern European migrants face greater challenges (linguistic, financial) to acquiring citizenship than their Western counterparts.

Treatment interacts with dual nationality permissiveness (Model 2) in interesting ways. The difference between the simple interaction (Model 3) and the one including country and year fixed effects, as well as clustered standard errors at the country level (Model 4) are striking. Both the direction and the magnitude of the effect change, confirming the fact that there is a lot of unaccounted heterogeneity in the basic model. Furthermore, Model 4 does a far better job in predicting variability in naturalisations than Model 3 ( $R^2=0.3$  vs 0.07, respectively 0.48), although the model explaining percentage of naturalised residents is better predicted by the chosen model specifications than the absolute number of successful citizenship applications ( $R^2=0.48$  vs 0.07). A lot of this variation is explained by the introduction of the year fixed effects, as already insinuated in the second model.

Table 4.2: DID effects of Brexit referendum on naturalisations (Full sample)

	(1)		(2)		(3)		(4)	
	N	%	N	%	N	%	N	%
Affected by Brexit	438.32	-2.10***	1048.20**	0.69***	-579.58	-2.14***	981.46**	2.62***
	(1.48)	(-8.91)	(3.16)	(4.13)	(-1.07)	(-4.91)	(3.11)	(19.45)
New member state			199.38*	0.30***				
			(2.06)	(5.95)				
Affected by Brexit × New member state			160.67	-0.62***				
			(0.70)	(-5.33)				
Unrestricted dual nationality					-852.75***	-0.05	-64.03	0.26
					(-7.81)	(-0.55)	(-0.50)	(1.07)
Affected by Brexit × Unrestricted dual nationality					1443.43*	0.06	393.34	-0.26
					(2.24)	(0.11)	(0.86)	(-1.54)
Observations	2493	2476	404	387	2493	2476	2493	2476
R-squared	0.00	0.03	0.23	0.44	0.03	0.03	0.07	0.48
Country fixed effects	✗	✗	✗	✗	✗	✗	✓	✓
Year fixed effects	✗	✗	✓	✓	✗	✗	✓	✓
Clustered standard errors	✗	✗	✗	✗	✗	✗	✓	✓

N is the total number of successful applications for British citizenship. % is the estimated share of residents from any given nationality who successfully naturalise every year. *t* statistics between parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Accounting for country and year specific shocks (Models 4), the referendum impact on both naturalisation measurements is positive, irrespective of dual nationality restrictions. However, surprisingly, the effect is only significant for those EU-born residents who are not permitted to have dual citizenship. Dual nationality permissiveness has a negative albeit non-significant effect on the number of applications for British citizenship among non-affected residents, but a positive overall effect among those affected by Brexit. Among nationals of countries who allow dual citizenship, we see an excess of 1045 successful

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applications from EU nationals compared to nationals of countries unaffected by Brexit. This translates into 2.62% increase of the resident population from the average country. A similar increase, albeit smaller is registered among nationals of countries who do not allow dual citizenship. Here, EU nationals registered 981 more applications than those unaffected by the referendum result, which once again translates into 2.62% of the resident population fitting this criteria. The differences are not statistically significant. The main takeaway is that the decision to become a British citizen is heavily shaped by Brexit. Obstacles posed by the country of birth are not, on their own, sufficiently strong deterrents.

Turning our attention to economic factors, I use OECD data to approximate employment opportunities in the UK versus country of birth for each age and gender group. I investigate how economic indicators interact with other factors to determine naturalisation outcomes (Equation 4.5). The key indicator in this analysis is the three-way interaction between the treatment (being impacted by Brexit), dual nationality permissiveness of the country of birth and difference in unemployment level in the UK compared to the country of birth.

We see for the control group that the worse off UK's employment rate fares in comparison to the sending country, the more people will naturalise. When unemployment differentials are interacted with EU nationality, we see a mirrored image – the number of applications for British citizenship from EU nationals decreases significantly as employment opportunities in their origin country increase relative to the UK (Model 2). Overall, EU nationals seem to have different naturalisation strategies than other migrant groups. They evaluate risks (losing pre-existing nationalities) and opportunities (chance of becoming and staying employed) differently. This can partially be explained by the fact that travel back to the

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origin country is easy and affordable, making this a more viable option for them than it is for other migrant groups. Furthermore, EU nationals who are allowed to keep dual nationality naturalise less than non-EU residents in similar circumstances, although the number of those who do naturalise constitutes a significantly higher share of the resident population compared to their peers (Model 3). This indicates that, when they have to choose sides, many residents affected by Brexit probably left the United Kingdom but the ones that stayed were more likely to apply for a British passport.

The findings from Model 4 are presented in a more intuitive manner in Figures 4.6 and 4.7. First, holding dual citizenship permissiveness constant, we see that actually EU and non-EU nationals respond similarly to unemployment shocks: the higher the UK unemployment is relative to their country of birth, the more people will naturalise. However, holding Brexit status constant, dual nationality permissiveness seems to create some diverging patterns, although not statistically significant. When having two nationalities is not permitted, unemployment conditions do not make a big difference in naturalisation numbers, although the share of population who naturalises grows when unemployment in the UK is larger than in the country of birth. When dual nationality is permitted, we see more applications from nationals of countries where the unemployment rate is higher than in the UK. As the unemployment differentials turn in favour of the sending country, fewer residents naturalise. The share of residents however stays constant irrespective of economic performance among nationals of states with permissive citizenship laws.



Table 4.3: DID effects of Brexit referendum on naturalisations (OECD sample)

	(1)		(2)		(3)		(4)	
	N	%	N	%	N	%	N	%
Affected by Brexit	252.15***	2.24***	229.08***	2.24***	288.48***	2.20***	285.21***	2.25***
	(7.09)	(15.42)	(6.19)	(14.62)	(6.08)	(15.58)	(5.18)	(14.49)
UK unemployment vs CoB	9.25**	-0.02	10.60**	-0.02	9.77**	-0.02	4.97	-0.04**
	(2.62)	(-1.88)	(3.05)	(-1.67)	(2.71)	(-1.92)	(1.22)	(-2.81)
Dual nationality allowed	30.63	-0.34**	19.90	-0.34**	62.61	-0.38***	104.14*	-0.18
	(1.17)	(-3.32)	(0.76)	(-3.29)	(1.88)	(-3.45)	(2.59)	(-1.35)
Affected by Brexit × UK unemployment vs CoB			-4.69*	-0.00			-1.18	0.01
			(-2.05)	(-0.11)			(-0.39)	(0.85)
Affected by Brexit × Dual nationality allowed					-104.58	0.12*	-122.97	0.06
					(-1.78)	(2.08)	(-1.77)	(0.73)
Dual nationality allowed × UK unemployment vs CoB							13.78	0.06**
							(1.94)	(3.20)
Affected by Brexit × Dual nationality allowed × UK unemployment vs CoB							-4.30	-0.01
							(-0.91)	(-0.96)
Observations	1330	1330	1330	1330	1330	1330	1330	1330
R-squared	0.10	0.37	0.10	0.37	0.10	0.37	0.11	0.38
Country, age, gender fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Clustered standard errors	✓	✓	✓	✓	✓	✓	✓	✓

N is the total number of successful applications for British citizenship. % is the estimated share of residents from any given nationality who successfully naturalise every year. *t* statistics between parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

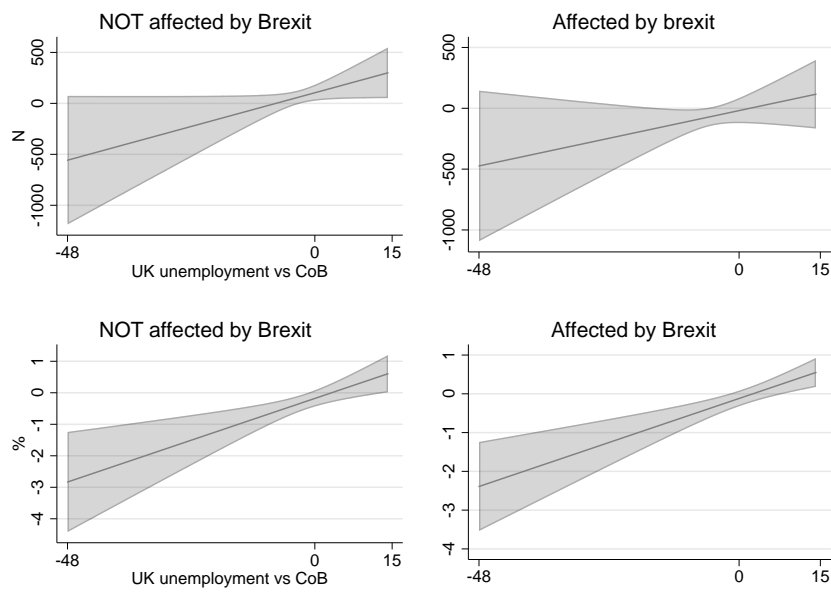


Figure 4.6: Average marginal effects of unemployment differentials on naturalisations by treatment status

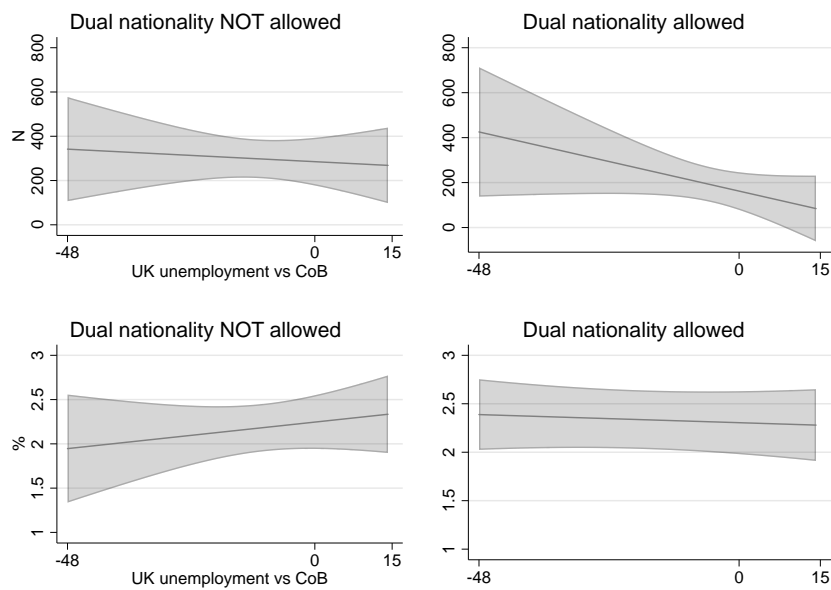


Figure 4.7: Average marginal effects of unemployment differentials on naturalisations by dual nationality restrictions

All in all, these findings have consistently showed two things: 1) the way in which naturalisation uptake is presented (be it in absolute numbers or share of the total population) impacts the conclusions drawn, and 2) the referendum on leaving the European Union

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created new incentives to naturalise among those affected by the change in status-quo (H1a). Looking back at the remaining theoretical expectations set in Section 4.1.1, I do not find any evidence to support the fact that those who have to renounce previously held citizenship(s) in order to get a British one will naturalise less than those who do not face such restrictions. To the contrary, for the group of migrants who have to make such big sacrifices, the decision making process shifts from whether to apply for citizenship or not, to whether to remain in the UK or not. I also uncovered supporting evidence that Western and Eastern migrants behave differently, although sample size restrictions did not permit a more in-depth investigation into the two groups.

## **4.5 Conclusion**

This paper identified and addressed a real gap in the literature concerning the naturalisation preferences of EU-born migrants residing in the United Kingdom. This is a crucial first step in understanding how new citizens will shape electoral results in the future. The Brexit referendum forced EU-born residents to reconsider the trade-offs between financial considerations (the application costs) and patriotism (belongingness in British society, citizenship loss of the country of birth). This shift of perspective could have triggered profound political consequences, especially if the referendum produced large numbers of new citizens who do not feel particularly loyal or included in the host country.

In fact these concerns seem unfounded, as the new electors show signs of deeper engagement and commitment to the country than previous waves of naturalised citizens. The results confirm the original expectation that the referendum pushed many EU nationals who might have otherwise not applied for British citizenship to make the big step. How-

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ever, the profile of the average naturalised EU citizen did not change significantly after 2016. We continue to see interest in British passports from both Eastern and Western Europeans, albeit the gap in successful naturalisation applications between the two groups is narrower. Barriers to naturalisation (e.g. dual nationality restrictions or unfavourable economic conditions) do not deter individuals from applying. If anything, they serve as a sorting mechanism for those who are not fully committed to staying in the United Kingdom and nudges those who are committed to naturalise more than their counterparts who do not face such difficult decisions. The fact that the average naturalised EU citizen comes from countries with higher electoral turnout than the UK can be interpreted as further evidence that new citizens have great potential to mobilise in future elections and make a real difference in politics.

While the paper does not tackle directly the political consequences of the recent spike in naturalisation applications, future academic and policy reports are encouraged to use the evidence presented here to reconsider the effect of EU migration on local and national politics, beyond the simple indirect effect left on the local community. In order to do so, consistent data collection and targeted research are necessary. Having national statistics like the ones released quarterly by the Home Office is useful, although a more geographically disaggregated data is desirable since migrants are more likely to become a salient electoral power in large cities, where they settle disproportionately.<sup>9</sup> Investigating the factors leading to mass EU naturalisation at a local level was not possible in this study due

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<sup>9</sup>38 percent of the London population is foreign-born (Vargas-Silva and Rienzo 2018) and approximately one in ten eligible voters in London during the 2015 General Election was a migrant (Ford and Grove-White 2015). Furthermore, 9 of the 20 seats with the largest migrant voter shares are in Greater London (Ford and Grove-White 2015: p.3)

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to data limitations.<sup>10</sup> However, the growing body of literature that focuses on migrants in the Greater London area reinforces the message that data collection at a more granular level is necessary in order to capture the changing demographics and preferences of residents in metropolitan areas. Furthermore, greater integration of European residents in national election survey studies is also desirable in order to draw more informed conclusions about their electoral behaviour in General Elections.

The findings have widespread policy implications. Many political parties still lack experience in targeting this previously disenfranchised group. First and foremost, the current research facilitate a targeted engagement with the new electorate and equips policy makers with detailed sociological profile of these potential voters: working age males and females, who chose British citizenship in order to secure their rights in the country they call home, despite the high financial and emotional costs behind the decision. The common misconception is that migrants are less well-off than natives and lack political sophistication. This is not true for EU migrants living in the United Kingdom. More and more scientific studies find that this expanding electorate is as varied as the native population, they contribute to the economy more than the average migrant and even more than the average native. They hold clearly defined policy preferences which they often suppress in favour of political loyalties. If political parties can understand the motivations of new citizens and win them over, the political landscape in many parts of the country can change drastically in the near future.

The new electorate can also benefit from the research. Acknowledging the electoral power they hold and learning from history can help them pave the way for themselves and future

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<sup>10</sup>The Home Office could not provide a break-down of naturalised citizens by country of birth for smaller geographical regions, mentioning that the British ceremony data (local authority level) comes from a different source to the wider citizenship data (national level).

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migrants in British politics. If EU nationals place their full economic and electoral power behind their vote, they can promote a new era of British politics where no person is left behind because of their country of birth. The paper also contains a warning message to the new electorate. It reminds them that there is a history in this country of policy makers overlooking and mistreating migrants, even when they are legal citizens and enjoy full voting rights. The only way to mediate this is by using their newly gained voting rights to engage with British politics, mobilise and turn out to vote in future elections. This can make a real difference for how they and other immigrants are perceived and treated by elected officials.

## **Chapter 5**

### **Conclusion**

Demographic changes have been posing explosive challenges to welfare states worldwide. While the list of demographic changes experienced in recent decades is a long one, the essays in this manuscript have focused on just two of them: “ageing populations” and “immigration”. These transformative policies changed the very structure of public opinion in Britain in two ways. Firstly, they alienated those directly affected and created deeply rooted cleavages between the majority and minority population. Secondly, they opened the political stage for far-right parties who capitalised on the fears of those “left behind”. This thesis provided an innovative, data driven analysis of how immigration and pension reforms have been shaping the attitudes of those affected and the wider community.

In this final chapter, I summarise the key findings and broad research themes which dominated the narrative of the thesis. I will then discuss some implications of the research and highlight the contributions it makes to the theoretical and empirical literature, and the discipline more widely. I conclude by outlining potential avenues for further research.

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## 5.1 Summary of main findings

Can voters hold politicians accountable when policies are implemented and information is disseminated with a considerable time lag? Chapter 2 showed that mishandled policies and extended time lags lead to erosion in political trust. While no evidence of partisan backlash has been found, the research indicates a type of voter sophistication which is often overlooked. In this case, affected women did not punish the incumbent for sacrificing their short-term individual well-being for long-term welfare gains. While they may not have liked this trade-off initially, the dissatisfaction did not persist across time. What did persist was a sense of political representation vacuum which no party could fill even decades after the law was implemented. The political alienation feeling was undoubtedly amplified by numerous governments overlooking to formally notify affected women of changes in their State Pension age until the last moment. As a House of Commons report concludes, *We will never know how many women did not know, or could not be reasonably expected to know, that their state pension age was increasing* (House of Commons, March 2016). The fundamental problem at hand however is not whether affected women knew about the change before the official notification letter reached them, but whether they can trust policy makers to keep them informed about such policies in a timely and transparent manner. The answer in this case was “no”. This avoidance or oversight of policy makers damaged citizens’ trust in the democratic process. For this reason, the main policy message of the paper is that mishandled information dissemination campaigns should be avoided at all costs in the future. Policy makers are encouraged to communicate honestly and openly with those affected, as no evidence has been found even in this extreme case that voters tend to “shoot the messenger” of bad news. Adverse side-effects of similar reforms can be managed if policy-makers recognise the threat of social and political fractionalisation



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posed by prevaricating tactics.

How did “hypermobile” migration affect electoral support for far-right parties? Chapter 3 showed that the spatial predictability of migrant settlement which stemmed from pre-existing transport infrastructure has a large positive effect on changes in support for far-right anti-immigrant parties. The beginning of the millennium has brought two significant changes in Europe: freedom of movement for millions of citizens across the continent (EU “Eastern enlargement” in 2004 and 2007) and the liberalization of travel markets. The boom in low-cost travel allows citizens from the “new” EU member states to travel and work in the “old” ones, while maintaining strong family and social ties with their home countries. This travel flexibility and affordability led to different integration trajectories for this group of migrants compared to previous migratory waves who typically had infrequent post-migration travel to the country of birth (e.g. refugees). Mobility is normally seen as a desirable characteristic. However, in the paper we reveal some unintended side-effects of low-cost travel. Areas experiencing a high increase in hyper-mobile residents voted significantly more for far right parties compared to areas with more consistent population composition. Furthermore, the new methodological approach developed - the proximity to travel hubs instrument (an alternative to the commonly used shift-share instrument) - can help deal with endogenous settlement choices among new migrants in cases where historical data is not available. Overall, we know very little about how this unprecedented mobility impacts the social and political norms of the host society. However, by being aware of the spatial predictability that we uncover, policy makers can proactively target support at affected communities.

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How did the Brexit referendum affect the composition of the foreign-born population with voting rights in the United Kingdom? Chapter 4 explored some of the implications of the Brexit referendum for electoral turnout and voting outcomes in future elections. It revealed a significant increase in naturalisation numbers among EU migrants and showed that constraining factors have little effect on the decision to become a British citizen when the very right to reside in the country is threatened. With millions of residents previously uninterested in becoming citizens now joining the eligible electorate in an attempt to retain their freedom of movement, we may be facing a political shift in upcoming years. The paper showed that deterrent factors such as dual nationality restrictions or expensive and bureaucratic application procedures had no effect on decisions to naturalise. What ultimately mattered for EU migrants was the threat to the freedom of movement and right to reside within the UK. All challenges and opportunities encountered throughout their citizenship journey have great potential to influence how the new citizens engage with political parties and how they vote in future elections. The paper identified the most prevalent socio-economic characteristics of successful applicants for British citizenship and connected them with the corresponding voting behaviour literature and survey data in order to develop hypothesis about their participation in future elections. The policy message is that political actors need to pay close attention to this previously unenfranchised group. Simultaneously, EU migrants need to understand the consequential role they play in British politics and mobilise to vote. If the new citizens become active voters, a new electoral map could emerge; one where electoral outcomes in urban constituencies will be decided by non-natives and where EU migrants will become a valued electoral commodity targeted by many political entrepreneurs.

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### 5.1.1 Themes

#### 1. Demographic change reforms

Pension reform, the expansion of the European Union and the Brexit referendum are among the most salient policy decisions implemented over the past three decades in the United Kingdom. They all reflect or are reflective of demographic changes in Britain. This collection of essays analysed their impact on ordinary voters, and how these policies have shaped the social and political scene in the country. Whether we talk about immigration policies or retirement interventions, these are demanding policies with an unknown initial cost. Their impact on the economy and society are not immediately obvious. Understanding how these processes are communicated to voters and, in turn, how voters assess them is essential for preparing future responses to contemporaneous challenges. Demographic changes will continue to dominate the political agenda and we have to be better prepared to deal with the consequences. The three papers highlighted some unexpected side-effects of these policies and encouraged greater communication, cooperation, and more targeted approaches in our efforts to create sustainable welfare states.

#### 2. Delayed materialisation of policy outcomes

All policies discussed are characterised by a significant time lag between the enactment moment and voter's responses to the policies. In the case of the pension reform, the time lag spans from 1995 and 2009-2011; for the EU enlargement, the time lag is between 2004/2007 and 2012; while the effects of the Brexit referendum which took place in 2016 still remains unclear to this day. All these evidence challenge

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the notion that accountability should be studied in the context of the next election alone. The thesis was centered around generating and testing a set of hypotheses about how these policies have shaped the long-term evolution of politics in Britain. Emphasis was placed on voters' ability to evaluate policy outputs and use them to hold policy makers accountable. Furthermore, the three papers challenge the idea of voter short-sightedness, revealing that responses to policies are oftentimes forming in a slow paced framework and gain momentum long after the implementation of the reform. If mishandled, policies can lead to social fractionalisation and resentment towards members of the out-group.

### 3. Linking administrative and public opinion data

By combining public opinion data with administrative/behavioural data, new research opportunities emerge. This technique provides a much wider scope for robust and relevant policy evaluations than it would be possible if these data-sets were used in isolation. The empirical narrative across the three paper is the combination of various high-quality sources of data in the pursuit of studying drivers of behaviours in response to given policies. Such data linkage techniques allow not only the exploration of new research avenues, but also revisiting old research with new data and tools.

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## **5.2 Broader implications and future directions**

The long-term challenges stemming from demographic shifts are not a new phenomena. They entered public awareness more recently due to global warming sustainability debates. Thanks to climate change activists, the general public is more accustomed today to hearing arguments about the urgency of responding now to threats that will affect future generations, although there is limited evidence to suggest that the public is capable of evaluating such complex policies. In policy circles on the other hand, such narratives have been circulating for decades already. For example, even before the start of the millennium, governments worldwide have been planning in advance how to mediate the problem of ageing populations. Some of the same governments also experimented and learned how best to frame the immigration debate to advance their political career. Now that complex and long-spanning policies appear more frequently on the political agenda, it is time to ask ourselves how policy makers and the average voter respond to the challenge. The concluding paragraphs will state the broader lessons learned from the findings and a road map for future research directions.

Changes in the composition of the electorate or the wider population have major implications for political elites, the new citizenry and the wider society. This manuscript explored some of these consequences and advanced our knowledge of the topic in three ways. Firstly, it raised awareness to the role of time lags in policy evaluations. Accountability is often seen as a process which manifests itself in the voting booth, in the context of the next election. However, when studying political responses to demographic changes, these transformations are subtle and slow in nature, which is ultimately reflected in the accountability process too. The paper therefore encourages the review of existing policy feedback

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mechanisms to reflect the growing importance and reliance on time-lags. Secondly, it provides an interdisciplinary approach to understanding how diverse actors respond to demographic changes. The translation of policy preferences into political outcomes remains a highly debated topic, generating contradictory evidence. In spite of the rising research interest in the field, the circumstances under which voters are capable to connect their policy preference to political parties' agendas remain unclear. This thesis contributes to the debate by presenting instances of high voter sophistication and by bringing to light some adverse electoral side-effects of interventions seen broadly as positive changes. Lastly, the thesis encourages the concomitant use of public opinion and administrative data to study policy implications. It developed new empirical tools which can be deployed across a wide range of settings and disciplines to analyze the electoral consequences of various reforms.

This thesis raised awareness to two understudied theoretical avenues which should be explored further. Firstly, the political behaviour literature needs to develop new accountability models to explain how citizens judge political parties for the performance of policies which span multiple administrations. Secondly, the role of information needs to be revised. We have seen numerous instances of political figures using prevaricating tactics and misinformation to advance their agendas. While these tactics may prove lucrative in the short-term, the three papers previously discussed showed that the long term effects can be detrimental to the democratic process. The effects of such complex policies cannot be conclusively stated and explored in a handful of studies. Using these first evidence, the political behaviour field should strive to move past studying the effects of these policies just in the context of the next election. Simultaneously, policy makers should enable ease of accessibility to appropriate information regarding policy interventions. These steps are crucial

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for re-building trust in the democratic process and promoting more reliable administrations.

What does this thesis tell us about other policies and even other countries? Due to space constraints, the analyses could not be extended in detail for more countries or policy fields, but the arguments put forward travel to other European reform trajectories. Ageing populations and immigration integration issues are not unique to the United Kingdom; they are challenges many countries face. The average life expectancy in the United Kingdom increased from about 70 years in the 1950s to about 81 years in 2020. This is on par with the growth experienced by other wealthy democracies. The United States increased its life expectancy by 10 years from 1950 (69 years old) to 2020 (79 years), while France and Germany raised the bar by 13 years in the same period (from 67 to 80 years). The net migration rate<sup>1</sup> in the UK also follows similar patterns to those recorded in other western democracies between 1950 and 2020. The average annual net number of migrants per 1,000 residents changed from 1.1 to 2.9 in the United States, from -0.1 to 6.6 in Germany and from -1.6 to 3.9 in the United Kingdom in the same period (all statistics from United Nations 2019). While the implementation and handling of policies may differ from country to country, the essence of the reform is the same. Many governments face similar challenges to fulfilling their social insurance commitments due to an aging population. Therefore, the implications of the findings revealed throughout the thesis can be removed from the UK electoral system and the British society, but they would have to be tested to ensure generalisability.

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<sup>1</sup>It is expressed as average annual net number of migrants per 1,000 population and calculated based on the formula  $\frac{(\text{immigrants}_t - \text{emigrants}_t)}{\text{population of country}_t}$ ; The number of immigrants minus the number of emigrants over a period, divided by the person-years lived by the population of the receiving country over that period.

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The message of the first paper was that mishandled policies and extended time lags are fertile grounds for political disengagement and alienation. These findings partially contradict previous studies which have shown that changes in State Pension age could not be linked to electoral costs (Macnicol 2015; Vickerstaff and Loretto 2017). While no direct partisan cost was linked to the policy in this case either, I have identified an indirect effect, one that damages trust in politics more generally. It remains to be determined which element of the policy produced these results: the large time lags or the information mismanagement that took place. To determine the driving factors behind the political attitudes discovered, one would need to study other similar policies. Luckily, the reform was not an isolated case and future research can draw on the 2007, 2011 and 2014 Pension Acts in the UK, as well as pension reforms from other European states to determine whether delaying retirement is indeed associated with higher likelihood of political disengagement. Furthermore, while the paper argued that time-lags in the information dissemination process are common in policy cycles, the case of the 1995 Pension reform remains unique in its circumstances both within and outside the United Kingdom. The lack of public interest in the topic allowed this policy to slip unnoticed from the public eye for decades. If this was a different, more “news-worthy” policy area, maybe mentions of the reform would have gone beyond business and finance magazines and maybe more political parties would have weighed in on the debate. Furthermore, similar changes in State pension age have been far more scrutinised in the media in other European countries. As it stands, it is difficult to speak to the external validity of these findings.

The second paper speaks to the effect of hypermobile migration on the host community’s political preferences. In 2015, approximately 4% of the EU’s population (nearly 20 million



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people) lived in a European country other than the one they were born in (United Nations 2015). The UK was among the top three most popular destinations for EU migrants (2.9 million), following Germany (5.3 million) and ahead of France (2.3 million). However, the share of EU migrants in the UK (about 4% of its population) is relatively small compared to that of Luxembourg (39%), Ireland (12%), Austria, Belgium, Cyprus (all of them 8%) or Germany (7%). All these statistics suggest that there is scope to study the role of EU migrants in other contexts and test whether the rise of populist parties in some of these destinations can also be traced back to the settlement patterns of foreign born EU residents. If this is the case, our fundamental understanding of the European integration project needs to be revised. As there are significant inferential challenges in determining whether and how new arrivals affect domestic politics, more data is necessary to try to understand the mechanisms behind the vote response of the natives. Further research should investigate and account for sustained connections with the home country, as this may impede efforts to integrate or prevent full language and cultural immersion. Simultaneously, the host community may see the close connection with the sending society as an economic or cultural threat and could become more hostile towards highly mobile migrants. Furthermore, the alternative instrument proposed in this paper can be used in numerous contexts in which historical settlement records are unavailable and can provide an additional test for those context where constructing a shift-share instrument is indeed possible. In this case, the estimates on the vote share using the standard shift-share instrument is not that different from the newly proposed instrument on hypermigration. Applying the instrument in a variety of settings will help determine whether this is simply a viable alternative to the widely used instrument or whether it can also lead to different findings.

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The electoral channel through which migrants affect voting outcomes in the receiving country covers not only the role of community perceptions of foreign born presence, but also the role of migrants' participation in elections. Chapters 3 and 4 explored both avenues for EU migrants residing in the United Kingdom. The key message of the final paper is that the limits to hypermobility that the post-Brexit arrangements imposed led to a surge in citizenship applications. Prior political experiences of new citizens will undoubtedly shape their political preferences moving forward. Cases of mass enfranchisement are not uncommon (Koukal and Eichenberger 2017; Kroth, Larcinese and Wehner 2016; Vernby 2013), although they are less frequent in this day and age. Occasionally, new instances of mass enfranchisement and new studies resurface when, for instance, the minimum voting age is lowered in a country (Bronner and Ifkovits 2019; Franklin 2004; Hernæs 2013). This study therefore speaks to the wider literature on the role of direct democracy in public policy provisions. Chapter 4 focused on the idea of mass enfranchisement in national elections, but one element unexplored in the paper is the possible mass *disenfranchisement* in local elections of the remaining EU nationals who did not naturalise. This phenomenon, too, has the potential to shape the profile of the median voter and can lead to further fractionalisation within the British society. Another take-away from the final two papers is the idea that EU migrants are a very different entity from other migrants and should be treated separately in research. The all-encompassing "migrant" variable used in many models needs revision. Especially when data is abundant, researchers need to unpack this black box and think about the different categories of migrants relevant in the context they are interested in.

To complement the theoretical and empirical contributions, this thesis also makes a significant data contribution. It brings together various reputable data sources which are difficult

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to access, makes them publicly available whenever possible and combines them to provide a more robust analysis of societal responses to policy change. The data released by the Department of Work and Pensions and the Home Office via the Freedom of Information requests filed are now in the public domain and other researchers can make use of it. Some of the Census statistics used in the second paper were not publicly available when we started the project. The data was commissioned from the Office for National Statistics and is now available online, free of charge. Obtaining access to the secure level British Household Panel Study and Understanding Society data was a lengthy process. While the data can only be accessed from authorised, secure locations, the analysis conducted and the outputs will enter the public domain.

Another important contribution of this book is that it calls attention to data gaps and, more importantly, to their cumulative impact on research capabilities. For example, the British Household Panel Study and Understanding Society suited well the purposes of the first study and remain, to my knowledge, the most adequate data source to answer the research question at hand. Together, they provide high-quality longitudinal data which allows researchers to evaluate policy interventions since 1991. However, the political battery of questions asked in every wave is limited, which constrained the analysis and the scope of the research. Furthermore, there are no comparable longitudinal political studies. More recently, the British Election Study began tracking political preferences of the same respondents via an internet panel. The study began in February 2014 and thus it does not match the timeline of the 1995 Pension act. Nevertheless, this is a welcomed initiative which will hopefully encourage more policy evaluation research in the future. The under-sampling of hypermobile European migrants in British election surveys on the

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one hand and the release of electoral results at a highly aggregate geographic area on the other hand, are just some of the other problematic aspects which impede UK-focused researchers to study the mechanisms driving the causal effect between the arrival of new immigrants and the change in vote shares of anti-immigration parties. This explains why a large proportion of similar published research studies are based in countries where votes and turnout statistics are released at the polling station level.

All in all, this thesis can be seen as a first attempt to unify our interdisciplinary knowledge on the consequences of demographic changes. The upcoming years will set the tone for how long-spanning ageing population policies are received by the public and how well we, as a society, integrate these changes into our social and political lives. Further delays in pension age are already announced globally, migration will continue to dominate the political agenda in Europe and beyond, and the Brexit challenges are only now beginning to unravel in the UK and across the EU. Demographic changes are here to stay and to challenge our willingness to overcome such challenges as a united society.

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# Appendix A

## Accountability and time lags: The electoral consequences of pension reform in the UK

### A.1 Further information about the SPA and various Pension Acts

There are two caveats to receiving State Pension which are particularly relevant for the research design. First of all, for a large proportion of the labour force, the State Pension Age is not necessarily an indicator of their actual retirement date. Due to age discrimination legislation implemented in 2011 which abolished the mandatory retirement age and prohibits age discrimination on the labour market, people can now remain economically active for as long as they wish. Thus, some people decide to retire early, while others keep working even after they reach their SPA. Those who opt for early retirement tend to be either at the top or at the very bottom of the income distribution. The wealthy retire early due to private or occupational pensions, while the poor may decide to stop working and claim income support or disability benefits until they become eligible to claim State Pension (Banks and Smith 2006). Other traits correlated with higher propensity of pursuing employment after retirement are education, health, having a working spouse or dependable children (Kim and Feldman 2000). Decisions over when to retire also vary based on individual circumstances, employment sector or industry and general job satisfaction.

The second caveat worth mentioning is that the State Pension is not the only type of pension available. Most individuals will have an additional source of income from occupational pensions or other private pension schemes. However, some reports show that a surprisingly large number of people still rely heavily on this basic source of income. A recent report published by the Pensions Policy Institute in March 2018 reveals that *the State Pension is an important part of retirement income for all pensioners except the lucky few with the*

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*highest retirement incomes* (PPI 2018). The analysis was conducted with "private pension" as control and the results did not change.

The 1995 Pension Reform was only the first in a series of state pension age interventions implemented in the post-war era in the United Kingdom. Another layer of complexity arises if multiple retirement delays experienced by the same individuals filter in the sample. In the UK context, many people were affected multiple times by changes in State Pension age. For example, as Figures A.1 and A.2 show, women born between 06 April 1959 and 05 April 1969 saw their minimum retirement age change three times (by the '95, '07 and '14 Pensions Acts). Similarly, women born between 06 April 1953 and 05 April 1959 or after 06 April 1969 and men born between 06 April 1959 and 05 April 1969 experienced two such derailments. The perpetual postponement of retirement could have adverse effects on political, economic and societal behaviour. Further research should strive to uncover the long term effects of these policies by bridging the gap between the various relevant disciplines (economics, sociology and political science). Only by addressing these multidisciplinary challenges simultaneously, can we understand how these policies affected preferences, and, in turn, how these preferences manifest themselves in social interactions, in the labour market and in the political arena.

The extent of the intervention is always calculated based on the date of birth, as explained in Figure A.3. The treatment therefore differs in terms of number of days added to the SPA based on the date of birth of each woman (As detailed in Figure A.3). Subsequent pension age reforms were enacted in 2007, 2011 and 2014. Special attention was paid to both the date of birth of the survey respondent and the date when the interview is held to ensure that individuals treated in subsequent reforms are not part of either the control or the treatment groups. For the enactment time, the post-treatment period ends before 26 July 2007 in order to ensure that individuals treated by the 2007 Pensions Act (enacted on this date) do not contaminate the results. For the notification analysis, similar considerations are given to the notification letters related to the changes in State Pension Age imposed by the 2011 Pension Act. Therefore, all individuals interviewed on or after 01 January 2012 are excluded, as that is when notifications letters related to the 2011 Pension act were sent.

Other aspects of the Pension Reform which can interfere with voters ability to assess the reform have also been considered, although data availability prevents studying their effects. For example, changes in SPA were delivered together with other pension related information which could appeal to some voters - an increase in the amount of state pension paid or the abolishing of mandatory retirement. These compensation packages could explain the mixed findings.

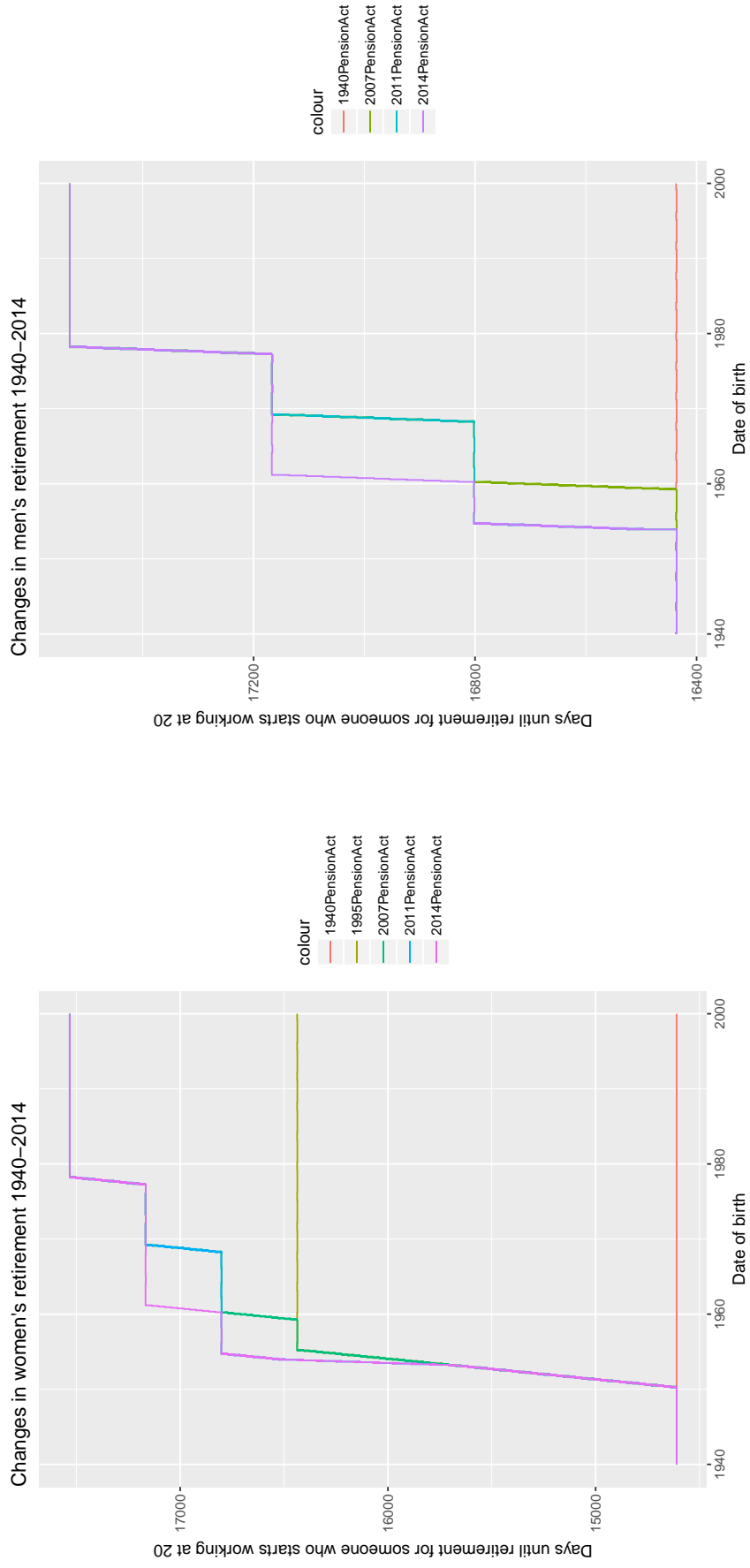


Figure A.1: **Women's** state pension age as changed by different pension reforms

Figure A.2: **Men's** state pension age as changed by different pension reforms



*Status: This is the original version (as it was originally enacted).*

- (3) A woman born on any day in a period mentioned in column 1 of the following table attains pensionable age at the commencement of the day shown against that period in column 2.
- (4) A woman born after 5th April 1955 attains pensionable age when she attains the age of 65.

TABLE

<i>(1) Period within which woman's birthday falls</i>	<i>(2) Day pensionable age attained</i>
6th April 1950 to 5th May 1950	6th May 2010
6th May 1950 to 5th June 1950	6th July 2010
6th June 1950 to 5th July 1950	6th September 2010
6th July 1950 to 5th August 1950	6th November 2010
6th August 1950 to 5th September 1950	6th January 2011
6th September 1950 to 5th October 1950	6th March 2011
6th October 1950 to 5th November 1950	6th May 2011
6th November 1950 to 5th December 1950	6th July 2011
6th December 1950 to 5th January 1951	6th September 2011
6th January 1951 to 5th February 1951	6th November 2011
6th February 1951 to 5th March 1951	6th January 2012
6th March 1951 to 5th April 1951	6th March 2012
6th April 1951 to 5th May 1951	6th May 2012
6th May 1951 to 5th June 1951	6th July 2012
6th June 1951 to 5th July 1951	6th September 2012
6th July 1951 to 5th August 1951	6th November 2012
6th August 1951 to 5th September 1951	6th January 2013
6th September 1951 to 5th October 1951	6th March 2013
6th October 1951 to 5th November 1951	6th May 2013
6th November 1951 to 5th December 1951	6th July 2013
6th December 1951 to 5th January 1952	6th September 2013
6th January 1952 to 5th February 1952	6th November 2013
6th February 1952 to 5th March 1952	6th January 2014
6th March 1952 to 5th April 1952	6th March 2014
6th April 1952 to 5th May 1952	6th May 2014
6th May 1952 to 5th June 1952	6th July 2014
6th June 1952 to 5th July 1952	6th September 2014
6th July 1952 to 5th August 1952	6th November 2014
6th August 1952 to 5th September 1952	6th January 2015

Figure A.3: Detailed treatment assignment (Pensions Act 1995, pp. 148-149)

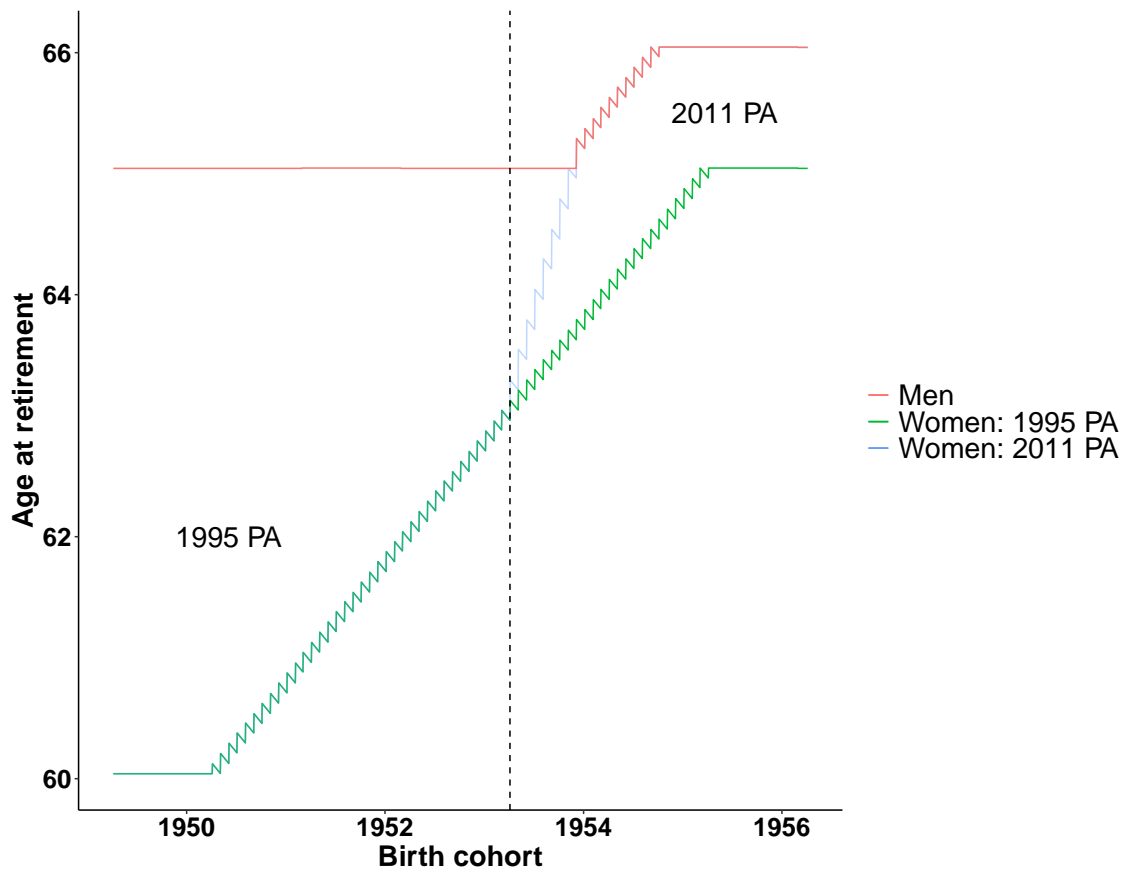


Figure A.4: Extent of SPA intervention of the 1995 Pensions Act (and further interventions by the 2011 Act)

## A.2 Knowledge of the treatment and the information dissemination exercise

It is impossible to determine the extent to which affected women would have been aware of the reform prior to the letter. The paper assumes that the most interested in politics and the highly educated would have been aware of the changes in the State Pension age prior to the notification treatment. However, we know that between April 2009 and November 2013 almost 7 million letters were sent to population affected by the 1995 and the 2011 pension acts.

Figure A.5 shows a sample letter sent to those affected by the 1995 Pension Act. This particular letter was sent to someone born on 5th November 1951. Their original State Pension age would have been 5th November 2011. However, in April 2010 they get notified that their new pension age is now 06th May 2013. Thus, they get one year and seven months notice for a one year and six months increase in their State Pension.

April 2010

Dear [redacted]

### Important information about your State Pension age

This letter gives you some important information about changes to the State Pension age for women and how these changes affect you - please read it carefully.

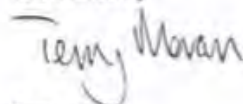
You may be aware that the law was changed in 1995 to increase the State Pension age for women from 60 to 65, so that it will be the same as for men. This will happen gradually between 2010 and 2020 and will affect all women born on or after 6 April 1950. This means your State Pension age is unlikely to fall on your birthday. You may have seen general information about this change, but this letter now explains how the change will affect you personally.

Our records show that your date of birth is 05/11/1951. Based on this information, the earliest date from which you could be entitled to any State Pension is 06/05/2013.

### More changes to State Pensions

To make State Pensions more widely available, especially for women and carers, we are introducing other changes in 2010. The enclosed leaflet will tell you whether you might benefit and also how you can get a free forecast of your State Pension.

Yours sincerely



Terry Moran  
Chief Executive

### What do I do now?

You do not need to do anything just yet. If the date of birth we have for you is wrong, please phone us straight away on [0845 3000 168](tel:08453000168).

We will write to you again before you reach State Pension age to tell you more about how to claim.

For more information about the changes to State Pensions, visit our website at [www.direct.gov.uk/statepension](http://www.direct.gov.uk/statepension).

Figure A.5: Sample letter - 1995 Pension Act, Source: DWP

Understanding the level of information prior to the notification exercise could speak to the validity of the results. To address this gap, I first look for evidence that affected voters were actively using search engines to look for more information on the reform. There is a growing trend across time for searches of terms such as "pension age", "pension reform", although the number of searches is small (it never exceeds 100 searches per day). There is also no unusual spike in interest for the reform around the information treatment, which suggest that people became more interested in state pensions reform after the notification treatment. Due to temporal trends in the use of the internet as a tool and platform, this method has obvious limitations.

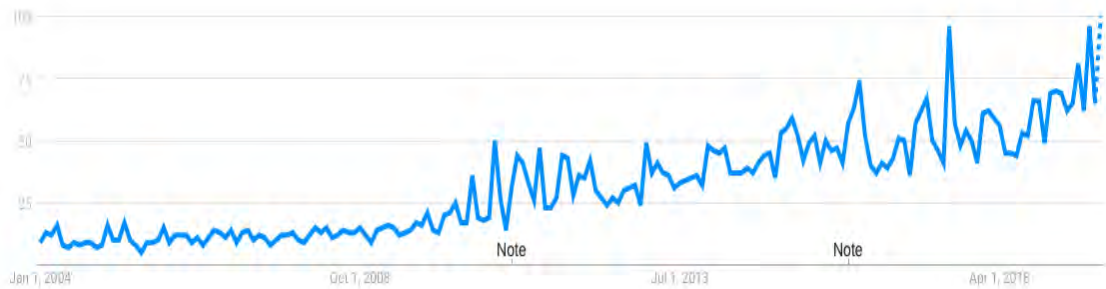


Figure A.6: Google trends for the search term “pension age”

Next, I turn my attention to mentions of the reform in the media prior to the information treatment. Once again, the transition from print to online media impedes an in-depth analysis. From the articles dating back to the 90s available online, I find that mentions of the 1995 Pension Reform are mainly resumed to business and finance magazines.

Political campaigns also vastly neglected mentioning the reform. None of the parties in the current analysis mentioned State Pension age in their electoral manifestos. I could only find evidence of three non-mainstream parties which included statements about SPA in their 2015 General Election Manifesto. The Party of Wales directly opposed increases in SPA, while the Scottish National Party proposed a review of the plans to increase the State Pension age beyond 66. The UK Independence Party opposed short-notice changes to retirement policies and also proposed “a flexible state pension window, which will widen over time, so even when the state pension age increases to 69, pensioners will still be able to take a slightly lower weekly state pension from the age of 65” (Manifesto Project Corpus). Analysis of the effects of such promises was not possible due to insufficient observations. All these avenues point to a general lack of awareness and salience of the policy.

### A.3 Political preferences (DV) coding strategy

Figure A.7 shows the four questions used to construct the dependent variables, as well as their sequence. The coding strategy is as follows: Respondents which follow the third sequence (3) are coded as weak supporters of the selected party, such that someone who selects "not very strong" in the final question receives a score of 1, while those who select "very strong" receive a score of three. Respondents following the second branch (2) receive scores between 4 and 6, while those who pursue the first avenue (1) are coded between 7 and 9. Respondents who report ineligibility to vote are excluded from the sample. The variable “political alienation” is coded as 1 if respondents spontaneously offer "none" as a response to the third question on avenue (3).

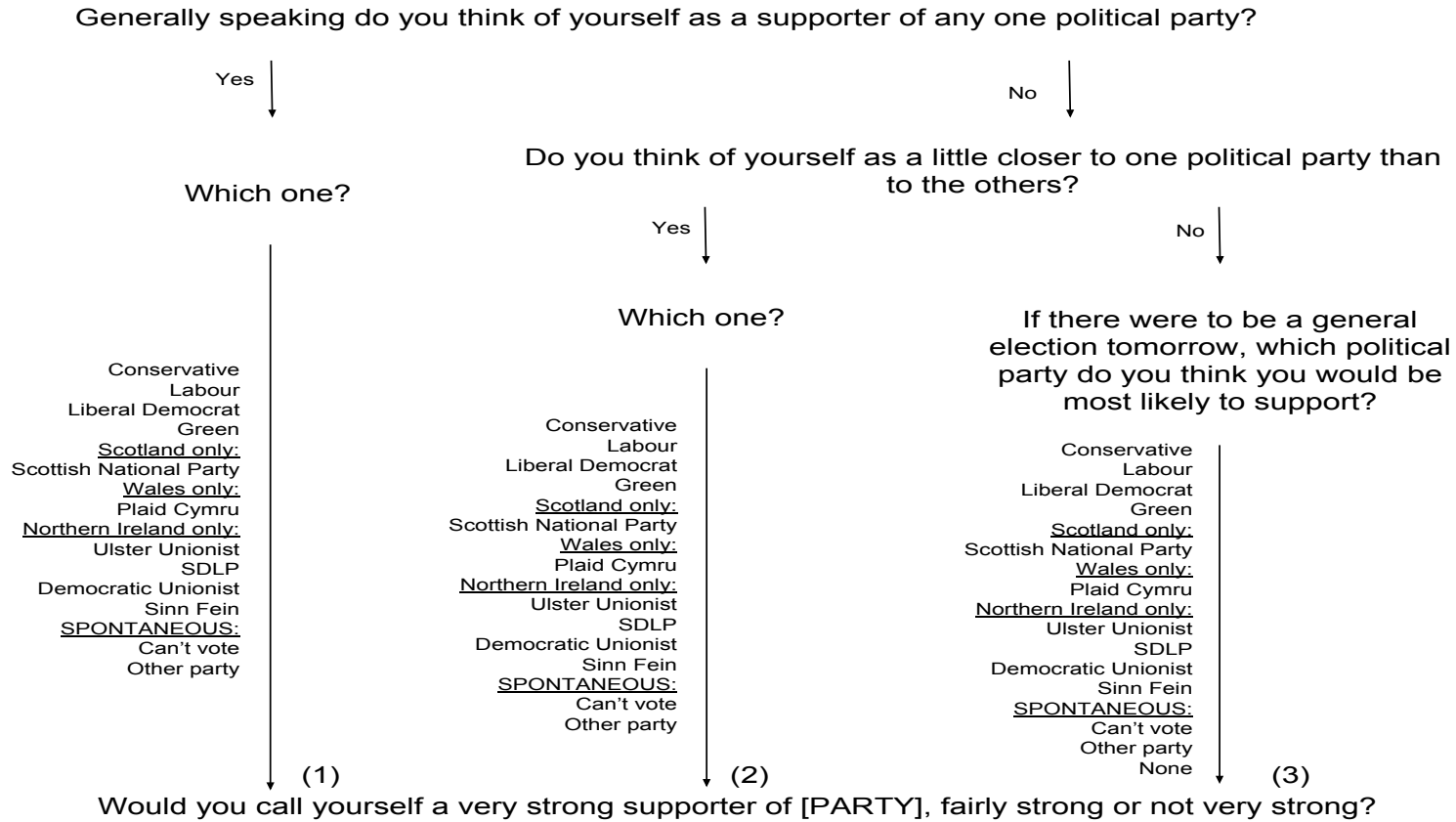


Figure A.7: Dependent variable - question sequencing map

## A.4 Further specifications - Enactment time

The final sample at the enactment time is as follows:

Table A.1: Sample size - enactment time

	Unaffected by 1995 Pension Act	Affected by 95 PA
Individuals	19,959	9,775
	Individual-wave observations: 147,461	Individual-wave observations: 52,828
Individuals in affected households	11,973	17,761
	Individual-wave observations: 106,032	Individual-wave observations: 94,257

Table A.2: The effect of being impacted by the 95' PA on political preferences among different groups at enactment time

	Labour	Conservative	Liberal Democrats	Abstention
Model 1: Affected women vs. everyone else				
Affected by '95 PA	-0.03 (-0.57)	-0.05 (-1.26)	0.06* (2.04)	0.04*** (6.84)
Observations	118,224	121,653	120,303	125,014
R-squared	0.03	0.03	0.01	0.04
Model 2: Women only - affected vs. unaffected				
Affected by '95 PA	-0.03 (-0.46)	-0.12* (-2.44)	0.03 (0.72)	0.05*** (8.52)
Observations	63,293	65,195	64,342	67,021
R-squared	0.03	0.03	0.01	0.04
Model 3: Everyone - living in a treated vs. untreated households				
Treated household	-0.04 (-1.34)	-0.05 (-1.69)	0.01 (0.46)	0.02*** (5.16)
Observations	118,224	121,653	120,303	125,014
R-squared	0.03	0.03	0.01	0.04
Model 4: Men only - living in a treated vs. untreated households				
Treated household	-0.04 (-1.04)	-0.02 (-0.49)	-0.01 (-0.46)	0.00 (0.34)
Observations	54,931	56,458	55,961	57,993
R-squared	0.03	0.03	0.01	0.04
Individual FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Month FE	✓	✓	✓	✓
Clustered SE	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Linear models with individual and time (year and month) fixed effects. The standard errors are clustered by individual. The outcome for the first three models is coded from 0 (Not a supporter) to 9 (Very strong supporter). The outcome in the final model is binary, taking value 1 if individual spontaneously says they do not support any party and 0 otherwise. All models control for age, education, income and interest in politics. *t* statistics in parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

### A.4.1 Enactment - robustness checks

The sample is restricted to include all individuals interviewed prior to the enactment of the law (19 July 1995) and those interviewed up to 365 days after the law was enacted.

Table A.3: DID effects of 95PA on Labour support

	(1)	(2)	(3)	(4)	(5)
Affected by '95 PA	0.43*** (10.42)	-0.02 (-0.41)	-0.02 (-0.38)	-0.00 (-0.02)	-0.00 (-0.07)
Age				0.07* (2.18)	-0.01 (-0.22)
Education				-0.00 (-0.02)	0.00 (0.00)
Income				-0.00 (-0.99)	-0.00 (-1.07)
Interest in politics				0.31*** (14.54)	0.31*** (14.53)
Observations	39,265	39,265	39,265	38,695	38,695
Individual FE	✓	✓	✓	✓	✓
Year FE	✗	✓	✓	✓	✓
Month FE	✗	✗	✗	✗	✓
Clustered SE	✗	✗	✓	✓	✓
R-squared	0.00	0.03	0.03	0.04	0.04

Linear models with support for Labour party as the outcome variable. The outcome is coded from 0 (Not a supporter) to 9 (Very strong supporter). All models compare untreated individuals with treated individuals interviewed up to 365 days after the 1995 Pension Act was enacted. *t* statistics in parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Table A.4: DID effects of 95PA on Conservative support

	(1)	(2)	(3)	(4)	(5)
Affected by '95 PA	-0.32*** (-8.49)	-0.02 (-0.41)	-0.02 (-0.44)	-0.00 (-0.07)	-0.00 (-0.00)
Age				-0.04 (-1.51)	0.05 (1.08)
Education				0.03 (0.55)	0.03 (0.63)
Income				0.00 (1.13)	0.00 (1.20)
Interest in politics				0.22*** (11.15)	0.21*** (11.12)
Observations	40,538	40,538	40,538	39,958	39,958
Individual FE	✓	✓	✓	✓	✓
Year FE	✗	✓	✓	✓	✓
Month FE	✗	✗	✗	✗	✓
Clustered SE	✗	✗	✓	✓	✓
R-squared	0.00	0.04	0.04	0.05	0.05

Linear models with support for Conservative party as the outcome variable. The outcome is coded from 0 (Not a supporter) to 9 (Very strong supporter). All models compare untreated individuals with treated individuals interviewed up to 365 days after the 1995 Pension Act was enacted. *t* statistics in parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$



Table A.5: DID effects of 95PA on Liberal Democrat support

	(1)	(2)	(3)	(4)	(5)
Affected by '95 PA	-0.01 (-0.27)	0.06 (1.95)	0.06* (2.02)	0.07* (2.33)	0.07* (2.32)
Age				-0.02 (-1.09)	-0.03 (-0.90)
Education				-0.05 (-1.32)	-0.05 (-1.33)
Income				-0.00 (-1.47)	-0.00 (-1.42)
Interest in politics				0.06*** (4.83)	0.06*** (4.84)
Observations	40,240	40,240	40,240	39,663	39,663
Individual FE	✓	✓	✓	✓	✓
Year FE	✗	✓	✓	✓	✓
Month FE	✗	✗	✗	✗	✓
Clustered SE	✗	✗	✓	✓	✓
R-squared	0.00	0.01	0.01	0.01	0.01

Linear models with support for Liberal Democratic party as the outcome variable. The outcome is coded from 0 (Not a supporter) to 9 (Very strong supporter). All models compare untreated individuals with treated individuals interviewed up to 365 days after the 1995 Pension Act was enacted. *t* statistics in parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Table A.6: DID effects of 95PA on vote abstention

	(1)	(2)	(3)	(4)	(5)
Affected by '95 PA	0.05*** (8.74)	0.02*** (3.34)	0.02** (2.98)	0.02* (2.46)	0.02* (2.42)
Age				0.01* (2.27)	0.00 (0.43)
Education				0.00 (0.49)	0.00 (0.43)
Income				0.00* (2.55)	0.00* (2.56)
Interest in politics				-0.03*** (-10.01)	-0.03*** (-10.01)
Observations	41,328	41,328	41,328	41,104	41,104
Individual FE	✓	✓	✓	✓	✓
Year FE	✗	✓	✓	✓	✓
Month FE	✗	✗	✗	✗	✓
Clustered SE	✗	✗	✓	✓	✓
R-squared	0.00	0.04	0.04	0.05	0.05

Linear models with vote abstention as the outcome variable. The outcome is coded as 1 if individual spontaneously says they do not support any party and 0 otherwise. All models compare untreated individuals with treated individuals interviewed up to 365 days after the 1995 Pension Act was enacted. *t* statistics in parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

## A.5 Further specifications - Notification time

Table A.7: Sample size - notification time

	Not notified about changes in SPA caused by the 95 PA	Notified about 95 PA
Individuals	54,866	1,238
	Individual-wave observations: 148,122	Individual-wave observations: 1,636
Individuals in affected households	53,601	2,503
	Individual-wave observations: 146,626	Individual-wave observations: 3,132

Table A.8: The effect of being impacted by the 95' PA on political preferences among different groups at notification time

	(1)	(2)	(3)	(4)
	Labour	Conservative	Liberal Democrats	Abstention
Model 1: Affected women vs. everyone else				
Notified about '95 PA	-0.13 (-1.28)	0.08 (0.91)	0.02 (0.22)	0.03 (1.88)
Observations	98,467	99,031	99,383	100,994
R-squared	0.03	0.01	0.01	0.03
Model 2: Women only - affected vs. unaffected				
Notified about '95 PA	-0.16 (-1.53)	0.07 (0.79)	0.01 (0.18)	0.04* (2.10)
Observations	54,435	54,864	54,958	55,888
R-squared	0.03	0.01	0.01	0.03
Model 3: Everyone - living in a treated vs. untreated households				
Notified household	-0.03 (-0.38)	0.00 (0.02)	-0.03 (-0.49)	0.00 (0.32)
Observations	98,467	99,031	99,383	100,994
R-squared	0.03	0.01	0.01	0.03
Model 4: Men only - living in a treated vs. untreated households				
Notified household	0.01 (0.04)	-0.06 (-0.55)	-0.10 (-0.82)	0.00 (0.18)
Observations	44,032	44,167	44,425	45,106
R-squared	0.02	0.01	0.01	0.03
Individual FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Month FE	✓	✓	✓	✓
Clustered SE	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Linear models with individual and time (year and month) fixed effects. The standard errors are clustered by individual. The outcome for the first three models is coded from 0 (Not a supporter) to 9 (Very strong supporter). The outcome in the final model is binary, taking value 1 if individual spontaneously says they do not support any party and 0 otherwise. All models control for age, education, income and interest in politics. *t* statistics in parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

### A.5.1 Labour incumbent

In this section I restrict the post-notification period to include only the period of Labour incumbency (up to 6 May 2010). There were not enough observations to run a similar analysis for the Coalition period only.

Table A.9: The effect of being notified about changes in SPA on political preferences for the period when Labour was the incumbent

	(1)	(2)	(3)	(4)
	Labour	Conservative	Liberal Democrats	Alienation
Notified about '95 PA	0.10 (0.29)	-0.20 (-0.52)	-0.20 (-0.83)	0.06 (1.57)
Observations	57,026	57,245	57,285	58,522
Individuals	34,785	34,870	34,841	35,120
IndividualFE	✓	✓	✓	✓
YearFE	✓	✓	✓	✓
MonthFE	✓	✓	✓	✓
ClusteredSE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
R-squared	0.02	0.01	0.01	0.02

Linear models with individual and time (year and month) fixed effects. The standard errors are clustered by individual. The outcome for the first three models is coded from 0 (Not a supporter) to 9 (Very strong supporter). The outcome in the final model is binary, taking value 1 if individual spontaneously says they do not support any party and 0 otherwise. All models control for age, education, income and interest in politics. The period of focus is from the beginning of the study period (2005) to 5 May 2010. *t* statistics in parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

## A.5.2 Notification model robustness checks

The sample is restricted to include all individuals interviewed prior to receiving the notification letter (date varies from household to household, as described in the paper) and those interviewed up to 365 days after the notification date.

Table A.10: DID effects of notification letter on Labour support

	(1)	(2)	(3)	(4)	(5)
Notified about '95 PA	0.22*	0.05	0.05	-0.13	-0.13
	(2.33)	(0.49)	(0.47)	(-1.21)	(-1.17)
Age				0.03	0.04
				(1.33)	(1.02)
Education				-0.08	-0.08
				(-1.58)	(-1.58)
Income				0.00	0.00
				(0.35)	(0.29)
Interest in politics				0.32***	0.32***
				(19.19)	(19.21)
Observations	101,198	101,198	101,198	80,808	80,808
Individual FE	✓	✓	✓	✓	✓
Year FE	✗	✓	✓	✓	✓
Month FE	✗	✗	✗	✗	✓
Clustered SE	✗	✗	✓	✓	✓
R-squared	0.00	0.01	0.01	0.02	0.02

Linear models with support for Labour party as the outcome variable. The outcome is coded from 0 (Not a supporter) to 9 (Very strong supporter). All models compare untreated individuals with treated individuals interviewed up to 365 days after they receive the notification letter. *t* statistics in parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Table A.11: DID effects of notification letter on Conservative support

	(1)	(2)	(3)	(4)	(5)
Notified about '95 PA	0.20** (2.82)	0.14 (1.96)	0.14 (1.78)	0.10 (1.00)	0.09 (0.97)
Age				0.04* (1.98)	-0.02 (-0.56)
Education				0.03 (0.80)	0.03 (0.75)
Income				-0.00 (-1.69)	-0.00 (-1.67)
Interest in politics				0.17*** (13.58)	0.17*** (13.56)
Observations	101,781	101,781	101,781	81,183	81,183
Individual FE	✓	✓	✓	✓	✓
Year FE	✗	✓	✓	✓	✓
Month FE	✗	✗	✗	✗	✓
Clustered SE	✗	✗	✓	✓	✓
R-squared	0.00	0.00	0.00	0.01	0.01

Linear models with support for Conservative party as the outcome variable. The outcome is coded from 0 (Not a supporter) to 9 (Very strong supporter). All models compare untreated individuals with treated individuals interviewed up to 365 days after they receive the notification letter. *t* statistics in parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Table A.12: DID effects of notification letter on Liberal Democrat support

	(1)	(2)	(3)	(4)	(5)
Notified about '95 PA	-0.04 (-0.69)	-0.02 (-0.29)	-0.02 (-0.30)	0.01 (0.07)	0.01 (0.12)
Age				-0.04* (-2.34)	-0.02 (-0.93)
Education				-0.01 (-0.37)	-0.01 (-0.34)
Income				-0.00 (-1.69)	-0.00 (-1.70)
Interest in politics				0.05*** (5.30)	0.05*** (5.27)
Observations	102,096	102,096	102,096	81,363	81,363
Individual FE	✓	✓	✓	✓	✓
Year FE	✗	✓	✓	✓	✓
Month FE	✗	✗	✗	✗	✓
Clustered SE	✗	✗	✓	✓	✓
R-squared	0.00	0.01	0.01	0.01	0.01

Linear models with support for Liberal Democratic party as the outcome variable. The outcome is coded from 0 (Not a supporter) to 9 (Very strong supporter). All models compare untreated individuals with treated individuals interviewed up to 365 days after they receive the notification letter. *t* statistics in parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Table A.13: DID effects of notification letter on vote abstention

	(1)	(2)	(3)	(4)	(5)
Notified about '95 PA	0.01 (0.49)	0.02 (1.40)	0.02 (1.40)	0.02 (1.34)	0.02 (1.32)
Age				-0.01 (-1.17)	-0.00 (-0.43)
Education				0.00 (0.22)	0.00 (0.22)
Income				-0.00 (-1.17)	-0.00 (-1.14)
Interest in politics				-0.07*** (-22.80)	-0.07*** (-22.79)
Observations	103,625	103,625	103,625	82,769	82,769
Individual FE	✓	✓	✓	✓	✓
Year FE	✗	✓	✓	✓	✓
Month FE	✗	✗	✗	✗	✓
Clustered SE	✗	✗	✓	✓	✓
R-squared	0.00	0.01	0.01	0.03	0.03

Linear models with vote abstention as the outcome variable. The outcome is coded as 1 if individual spontaneously says they do not support any party and 0 otherwise. All models compare untreated individuals with treated individuals interviewed up to 365 days after they receive the notification letter. *t* statistics in parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$



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### A.5.3 Different coding strategy for the notification results

A different coding approach for the notification treatment is also tested and the results do not change. Instead of dropping all interviews conducted after 2011, I drop individuals affected by the 2011 Pension act. This means that I drop all individuals who either receive a notification letter in January 2012, February 2012 or May 2013, or individuals who live in a household with someone who receives a letter on one of the three dates. Individuals living in untreated households get randomly allocated one of the remaining 7 notification dates to determine whether the interview is conducted pre or post treatment.

Thus, in the main analysis, individuals affected by the 2011 Pension Act and notified in 2012 and 2013 are kept in the analysis, but their post-treatment (post-notification) interviews are dropped. For cross-temporal comparison purposes, all interviews conducted after 2012 are also dropped. On the other hand, in this sample, all interviews ever conducted with individuals affected by the 2011 pension act are dropped from the analysis, but interview-wave observations for all other individuals are kept. The new time-frame is 2005-2019 (2005-2009 BHPS wave 14-17; 2009 - 2019 UKHLS wave 1-9), instead of 2005-2011 in the main analysis. The new sample size is as follows:

Table A.14: Sample size - notification time (second coding approach)

	Not notified about changes in SPA caused by the 95 PA	Notified about 95 PA
Individuals	58,585	1,345
	Individual-wave observations: 290,225	Individual-wave observations: 6,731
Individuals in affected households	57,168	2,762
	Individual-wave observations: 284,691	Individual-wave observations: 12,265

I will now replicate the analysis produced in Tables A.8 with the new sample. The magnitude and direction of the effects remain largely unchanged across the two samples.

Table A.15: The effect of being impacted by the 95' PA on political preferences among different groups at notification time

	(1)	(2)	(3)	(4)
	Labour	Conservative	Liberal Democrats	Alienation
Model 1: Affected women vs everyone else				
Notified about '95 PA	-0.10 (-1.12)	0.07 (0.89)	-0.00 (-0.04)	0.02 (1.46)
Observations	163,916	168,051	171,759	175,218
R-squared	0.03	0.02	0.01	0.03
Model 2: Everyone - living in a treated vs untreated hh				
Notified household	0.01 (0.08)	0.02 (0.33)	-0.04 (-0.85)	-0.00 (-0.13)
Observations	163,916	168,051	171,759	175,218
R-squared	0.03	0.02	0.01	0.03
Model 3: Women only - affected vs unaffected				
Notified about '95 PA	-0.14 (-1.52)	0.06 (0.76)	0.01 (0.10)	0.02 (1.65)
Observations	90,489	93,330	95,141	97,247
R-squared	0.03	0.02	0.02	0.04
Model 4: Men only - living in a treated vs. untreated households				
Notified household	0.06 (0.58)	-0.00 (-0.05)	-0.11 (-1.42)	0.00 (0.04)
Observations	73,427	74,721	76,618	77,971
R-squared	0.02	0.02	0.01	0.03
Individual FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Month FE	✓	✓	✓	✓
Clustered SE	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Linear models with individual and time (year and month) fixed effects. The standard errors are clustered by individual. The outcome for the first three models is coded from 0 (Not a supporter) to 9 (Very strong supporter). The outcome in the final model is binary, taking value 1 if individual spontaneously says they do not support any party and 0 otherwise. All models control for age, education, income and interest in politics. *t* statistics in parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

# Appendix B

## Migration and Election Outcomes: Evidence from London

### B.1 UKIP and the BNP campaigns

UKIP 2012 campaign - relevant points:

- Create more jobs for Londoners by saying 'No' to open-door immigration
- Fight EU red tape strangling London businesses
- Priority for Londoners whatever their ethnic origin for jobs and housing, over migrants and asylum seekers



**Lawrence Webb**   
**A Fresh Choice for London**

**Only UKIP offers a truly Fresh Choice for London.**  
Millions of Londoners fear for their livelihoods because of punishing EU regulations which threaten up to half a million jobs that rely on a thriving financial services industry.  
This is not just banking, it is the support services, like secretaries, IT workers, cleaners, bar and restaurant staff - even cabbies.  
Only UKIP will fight to protect those jobs by saying NO to EU regulation of the City.  
Vote UKIP on May 3rd.  
*Lawrence Webb*

**Fresh Ideas for London**

- ✓ **Create more jobs** for Londoners by saying 'No' to open-door immigration.
- ✓ **Cut rates for local businesses** employing local people.
- ✓ **Fight EU red tape** strangling London businesses.
- ✓ **Priority for Londoners** whatever their ethnic origin for jobs and housing, over migrants and asylum seekers.
- ✓ **Zero tolerance** on gangs, knife crime and anti-social behaviour. Offend on Saturday, face court on Monday.
- ✓ **20 minutes free parking** across London.
- ✓ **Allow taxis** to use Olympic VIP lanes.
- ✓ **Cut Council house waiting lists** in half by filling empty properties from a central register and prioritising the needs of long-term Londoners.
- ✓ **Stop spending public money** on public sculpture when pensioners can't pay their bills.
- ✓ **Give landlords the power** to decide if they want smoking rooms in pubs and clubs.
- ✓ **5% VAT** on beer and cider.

**UK Independence Party** 

**0800 587 6 587** [ukipmayor.com](http://ukipmayor.com)  
Published & promoted by Peter Staveley on behalf of UKIP 156 Pentonville Rd, London N1 9JL

BNP 2012 campaign - relevant points:

- British people must be housed first
- No amnesty for illegal immigrants
- British jobs for British workers



**I LONDON** visit [www.BNP.org.uk](http://www.BNP.org.uk)

**Think commonsense**  
**Vote BNP**

Dear Londoner,

I've lived in London since 1989 and I have worked for the BBC and then at the Commonwealth Office. I am of Italian descent which, I guess, makes me the most 'cosmopolitan' candidate standing for **London Mayor in 2012**.

I was outraged to find some immigrant communities refusing to respect the British people and their way of life so I joined the British National Party because I want to preserve the traditions, freedoms and identity of the country that has been so good to me and my family.

London is a diverse city and although there are some benefits, Multiculturalism has clearly led to division and confrontation instead of integration. The shocking looting and rioting last August is a prime example of this failed policy.

London needs a strong Mayor who is fair and decisive with the vision to make this great city of ours dynamic, safe and prosperous. I am that man!

Vote for me on 3rd May and together we will make London a city to be proud of again.

*Carlos Cortiglia*  
**Carlos Cortiglia**  
British National Party  
Mayoral Candidate

**Want to see Carlos take on the other mayoral candidates live on TV?**  
Call BBC London on 020 8743 8000 and demand that they invite the British National Party to all televised debates

**Call 0844 809 4581** **BRITISH NATIONAL PARTY**

**People like you voting BNP:**

- ✓ British people must be housed first
- ✓ No amnesty for illegal immigrants
- ✓ Build a better NHS
- ✓ Zero tolerance on crime and anti-social behaviour
- ✓ British jobs for British workers
- ✓ Abolish the Congestion Charge, CPZ and LEZ
- ✓ Reduce council tax
- ✓ Free weekend Tube and train travel

“ I support the British National Party all the way because I want to make a difference and have a no-nonsense British government that puts British people first. ”  
**Mike Jones, London Cabbie**

“ I'm backing the British National Party because they support our traditional Christian faith. We need strong leadership to protect our national identity from the threat of Islam. ” **Reverend Robert West**

“ We always vote for the British National Party because we want streets that are safe to walk on. We want to see local bobbies on the beat and we want to feel part of a caring British community again. ”  
**Penny McCulley, Pensioner**

**CORTIGLIA Carlos**  
British National Party

First choice

Prepared by election agent Clive Jefferson of PO Box 1244, Enfield EN1 9UF

British National Party - London

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## B.2 Airlines and destinations

WizzAir was one of the first airlines to capitalize on the Eastern enlargement of the EU. The company was established in 2003 and had its first flight on May 19, 2004 (only two weeks after enlargement) from Katowice, Poland to London Luton. At the time of writing, the company operated regular flights between London and various Central and Eastern European countries, flying to a mix of primary, secondary and regional airports. The variety in destination airports makes the company attractive for migrants who also want to reduce commuting time in their home countries. WizzAir's main competitor in Central and Eastern Europe is Ryanair. Founded in November 1984, Ryanair moved its main London base to Stansted in 1991.

The flight networks of WizzAir (Figure B.4) and Ryanair (Figure B.5) contrast with that of EasyJet (Figure B.6), another low-cost airline, which adopted a different strategy. Unlike its two competitors, EasyJet expanded less aggressively into Central and Eastern European countries, where it tends to fly only to the capital cities. Moreover, and importantly in the context of our paper, EasyJet's largest base of operation is London's Gatwick airport.

While these maps show flight routes at the time of writing, they closely reflect historical patterns. Dobruszkes (2009) highlights the role of WizzAir and Ryanair in expanding west-east routes in direct response to the Eastern enlargement of the EU, in contrast to EasyJet's continued focus on Western Europe. His study reports route networks for these and other airlines as of 2008. To assess trends across London's airports more specifically, we obtained historical flight and passenger data from the UK's Civil Aviation Authority for the period 2000 to 2015. Our analysis is presented in Figures B.3, B.8, and B.9. The data show how quickly after accession EU8+2 passenger traffic through London's airports expanded, and how Gatwick was overtaken by Stansted and Luton in terms of the number of EU8+2 flight destinations and passenger volumes. This shows low-cost flights to and from the region quickly became concentrated in the two airports located to the north of London.

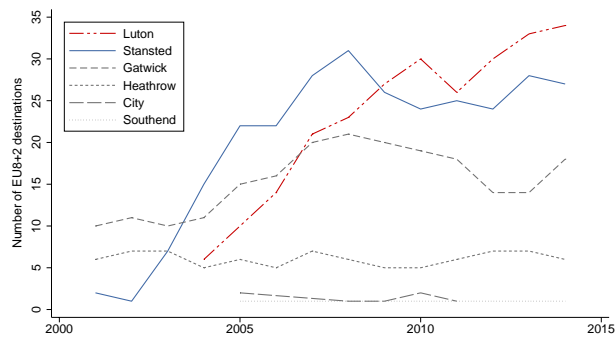


Figure B.1: Number of EU8+2 destinations by London airport, 2001-2014. Source: Own calculations based on annual data from the Civil Aviation Authority (2020), Table 12.1, downloaded on December 28, 2020.

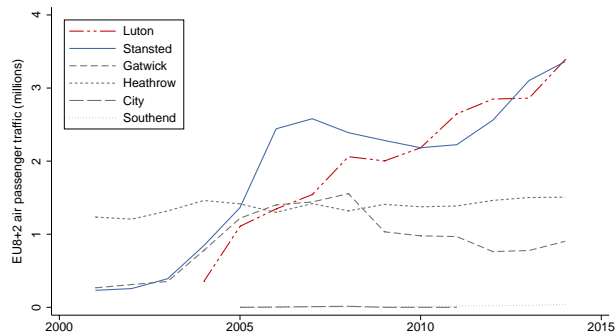


Figure B.2: Passenger traffic to/from EU8+2 countries by London airport, 2001-2014. Source: Own calculations based on annual data from the Civil Aviation Authority (2020), Table 12.1, downloaded on December 28, 2020.

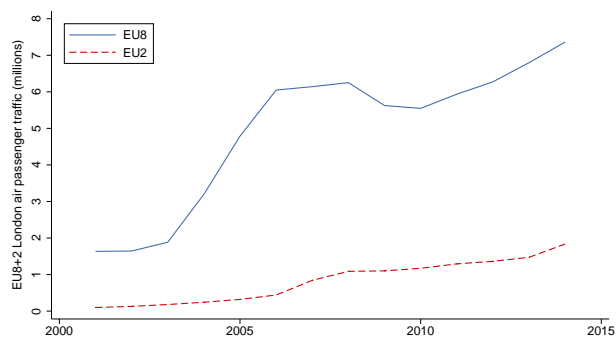


Figure B.3: Total number of passengers to/from London and EU8/EU2 countries, 2001-2014. Source: Own calculations based on annual data from the Civil Aviation Authority (2020), Table 12.1, downloaded on December 28, 2020.



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### **B.3 Bus stops and their distance from ward centroids**

To identify the pre-existing bus route infrastructure to Luton and Stansted airports, we first consulted travel guides from around the period of EU enlargement or earlier. These consistently identified two main bus routes: Green Lines bus number 757 from Victoria to Luton Airport, and the National Express Airbus A6 to from Victoria Coach Station to Stansted Airport. Both routes have a history that predates the period we examine. Just prior to the 2004 enlargement of the EU, the 757 departed from central London towards the airport about 30 times on weekdays, and the A6 about 40 times (Lonely Planet 2004). Both providers dominated their respective routes. For example, according to data in Luton Borough Council's 2006 Provisional Bus Strategy the 757 accounted for about two thirds of busses connecting the airport to central London. Both the 757 and the A6 followed a similar route into and out of London, with some shared stops and others in close proximity. The smaller rival operators to these dominant services, too, tended to follow these routes.

Next, we confirmed the stops on these routes, where travellers to or from the airport were able to board, at the time of EU enlargement in 2004. Although there is substantial continuity in stops used over the years, there were also some changes, and we wanted to be certain to capture the pre-existing ones only. Hence, we submitted requests under the UK's Freedom of Information Act to Transport for London (TfL), which is responsible for the approval of coach stops. In response, TfL confirmed when it first approved the existing stops to operate under a London Service Permit, a regulatory regime for any bus, coach, or tours service outside of the TfL network. Prior to this, such services operated under London Local Service licences, a separate regulatory regime. Stops with the earliest approval date were likely also approved at an earlier date, but TfL does not hold any earlier information. TfL sent us a full listing of current bus stops in both directions and the dates of their approval under the current regulatory regime. We coded only those in one direction, since the corresponding stops in the other direction are typically in very close proximity, often on the opposite side of the road.

According to TfL, the following current 757 stops for travel from Luton and towards Victoria were approved to operate under a London Service Permit from 1 August 2002 (bus stop codes in brackets): Brent Cross (BP4663), Childs Hill (9358), Lord's Cricket Ground (4804), Baker Street Station (4789), Baker Street (1588), Oxford Street (179), Marble Arch (29908), Hyde Park Corner (36857), and Victoria (BP4469). For the A6, the following current stops for travel from Luton and towards Victoria Coach Station were approved under a London Service Permit from 24 May 2004: Golders Green Bus Station (RO802), Finchley Road (BP4403), St Johns Wood Wellington Road (4804), and Baker Street Station (4879). The Finchley Road bus stop is for travel in the opposite direction, but we use it as a proxy as the corresponding one in the direction of central London has a more recent approval date,



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suggesting some minor adjustment in its location over the years. We also added Victoria Coach Station as the terminus for the A6, as indicated in various travel guides we consulted.

Next, we used this information to determine the distance of wards to each of these transport nodes. We first obtained the locations of TfL's more than 19,000 bus stops from the London Datastore (<https://data.london.gov.uk>) and identified the above A6 and 757 stops. In a few instances TfL's listing did not include the bus stop, which was the case when it did not serve any TfL buses. In these cases, we searched the TfL website (<https://tfl.gov.uk>) for the location of these stops and used the closest TfL stop or tube station for which we had information on its precise location. Our listing of TfL tube station locations is also from the London Datastore. We then calculated the distance between each ward centroid and each A6 and 757 bus stop. These operations were carried out with QGIS version 3.4. This provided the data for our instrument, which for each ward gives the distance (in kilometres) between its centroid and the closest A6 or 757 bus stop approved in 2004 or earlier.

## **B.4 Data and variables**

We started with data from the Office for National Statistics (ONS) Open Geography Data Portal (ONS 2019) to match lower layer super output areas (LSOA) used in the census and electoral wards/divisions used for election outcomes. Two documents provided by the ONS assisted with the task: "WD10\_LAD10\_EW\_LU", containing the new 9 character ward codes and the old 6 character ward codes; and "WD11\_CMWD11\_LAD11\_EW\_LU", containing the lookup between the census merged wards (E36) to electoral wards (E05). We then added information on electoral outcomes, on population counts by country of birth, and other ward-specific data.

Next, we checked for electoral border changes. Table B.1 shows that most electoral changes took place before the 2004 election or after the 2012 one. The only electoral changes in the period investigated in this paper are targeted at the City of London. We exclude City of London from the analysis. This does not impact the results significantly, as "the City" is a business centre and has a negligible number of residents. The 2001 Census counted between 1061 and 3003 residents in each of its wards and the 2011 Census between 1434 and 2782 residents.

Table B.1: Electoral changes orders since 2001

File ref	SI Number	SI Title	District Code	District/UA Name	Year
W327	N/A	City of London (Ward Elections) Act 2002	00AA	City of London	2007
W327B	N/A	City of London Act	00AA	City of London	2013
W52	780/2000	The London Borough of Barking and Dagenham Order 2000	00AB	Barking and Dagenham	2002
W42	333/2000	The London Borough of Barnet Order 2000	00AC	Barnet	2002
W48	312/2000	The London Borough of Bexley Order 2000	00AD	Bexley	2002
W483	481/2017	The London Borough of Bexley Order 2017	00AD	Bexley	2018
W72	1846/2000	The London Borough of Brent Order 2000	00AE	Brent	2002
W71	1764/2000	The London Borough of Bromley Order 2000	00AF	Bromley	2002
W70	1765/2000	The London Borough of Camden Order 2000	00AG	Camden	2002
W55	781/2000	The London Borough of Croydon Order 2000	00AH	Croydon	2002
W506	1125/2017	The London Borough of Croydon Order 2017	00AH	Croydon	2018
W26	334/2000	The London Borough of Ealing Order 1999	00AJ	Ealing	2002
W73	1845/2000	The London Borough of Enfield Order 2000	00AK	Enfield	2002
W77	1977/2000	The London Borough of Greenwich Order	00AL	Greenwich	2002
W56	782/2000	The London Borough of Hackney Order 2000	00AM	Hackney	2002
W391	2795/2013	The Hackney Order 2013	00AM	Hackney	2014
W74	1844/2000	The London Borough of Hammersmith and Fulham Order 2000	00AN	Hammersmith and Fulham	2002
W57	783/2000	The London Borough of Haringey Order 2000	00AP	Haringey	2002
W27	316/2000	The London Borough of Harrow Order 1999	00AQ	Harrow	2002
W49	313/2000	The London Borough of Havering Order 2000	00AR	Havering	2002
W69	1766/2000	The London Borough of Hillingdon Order 2000	00AS	Hillingdon	2002
W28	317/2000	The London Borough of Hounslow Order 1999	00AT	Hounslow	2002
W58	784/2000	The London Borough of Islington Order 2000	00AU	Islington	2002
W59	785/2000	The Royal Borough of Kensington and Chelsea Order 2000	00AW	Kensington and Chelsea	2002
W401	25/2014	The Kensington and Chelsea Order 2014	00AW	Kensington and Chelsea	2014
W68	1767/2000	The Royal Borough of Kingston upon Thames Order 2000	00AX	Kingston upon Thames	2002
W29	319/2000	The London Borough of Lambeth Order 1999	00AY	Lambeth	2002
W61	1236/2000	The London Borough of Lewisham Order 2000	00AZ	Lewisham	2002
W30	318/2000	The London Borough of Merton Order 1999	00BA	Merton	2002
W67	1768/2000	The London Borough of Newham Order 2000	00BB	Newham	2002
W31	335/2000	The London Borough of Redbridge Order 1999	00BC	Redbridge	2002
W484	609/2017	The London Borough of Redbridge Order 2017	00BC	Redbridge	2018
W50	314/2000	The London Borough of Richmond upon Thames Order 2000	00BD	Richmond upon Thames	2002
W60	786/2000	The London Borough of Southwark Order 2000	00BE	Southwark	2002
W474	1202/2016	The London Borough of Southwark Order 2016	00BE	Southwark	2018
W75	1847/2000	The London Borough of Sutton Order 2000	00BF	Sutton	2002
W54	787/2000	The London Borough of Tower Hamlets Order 2000	00BG	Tower Hamlets	2002
W390	1786/2013	The Tower Hamlets Order 2013	00BG	Tower Hamlets	2014
W32	336/2000	The London Borough of Waltham Forest Order 1999	00BH	Waltham Forest	2002
W51	315/2000	The London Borough of Wandsworth Order 2000	00BJ	Wandsworth	2002
W53	788/2000	The City of Westminster Order 2000	00BK	Westminster	2002
P325	5008/2013	The City of Westminster Order 2013	00BK	City of Westminster	2014

Source: Data in the current format was provided by the ONS in private correspondence. It is also available online on The Local Government Boundary Commission for England website: <https://www.lgbce.org.uk/resources/database-of-local-government-orders/greater-london>.

May 4, 2000	London elections
<b>April 29, 2001</b>	<b>Census</b>
<b>May 1, 2004</b>	<b>EU enlargement (EU8 join)</b>
<b>June 10, 2004</b>	<b>London elections and EU Parliament election</b>
<b>January 1, 2007</b>	<b>EU enlargement (EU2 join)</b>
May 1, 2008	London elections
<b>March 27, 2011</b>	<b>Census</b>
<b>May 3, 2012</b>	<b>London elections</b>

Table B.2: Timeline of key events

Table B.2 presents the timeline of key events and helps to clarify several aspects of our data. First, we do not use the 2000 elections because ward and borough data were not collected. The unit of analysis for which the 2000 election results are available (London Assembly constituency) is too large to conduct any meaningful analysis. Instead, we use the 2004 results. Second, as the timeline suggests, the 2004 elections took place one month after the EU’s enlargement in that year. We assume this is a short enough period to make it unlikely for new EU migrants to arrive in London and leave an impression on the local population, in a way that substantially affects their electoral behavior. The Civil Aviation Authority reports approximately 100,000 more travellers on all routes (back and forth) between London airports and EU8 destinations in May 2004 (263,322 travellers) compared to the same month in the previous year (160,859 travellers). See also Figure B.7 for a monthly breakdown of relevant air travel in 2004. Finally, we use the 2012 election results because they are temporally closest to the 2011 Census. Therefore, this election is the one for which we have the most accurate data related to the number of new EU residents.

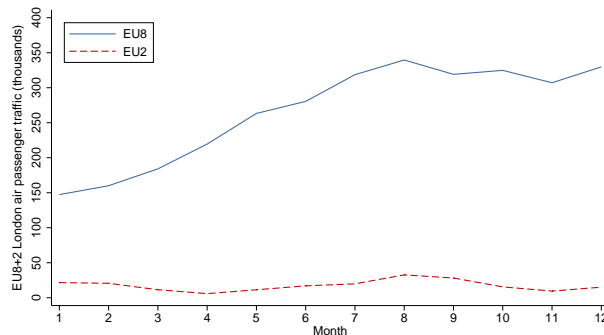


Figure B.7: Monthly total number of passengers to/from London and EU8/EU2 countries, 2004. The EU8 countries joined the EU in month 5. Source: Own calculations based on monthly data from the Civil Aviation Authority (2020), Table 12.1, downloaded on February 6, 2021.

Data used to calculate the share of votes cast for UKIP and the BNP were obtained from the London Datastore (Greater London Authority 2004; 2012). We focus on the

London Member election and calculate the percentage point change in votes for party  $p \in \{BNP, UKIP\}$  in each ward  $w \in \{1 : 620\}$  according to the formula:

$$\Delta \text{Votes}_{p,w} = \frac{\text{Member}_{p,w,2012}}{\text{Member}_{\text{TotalValidVotes},w,2012}} \times 100 - \frac{\text{Member}_{p,w,2004}}{\text{Member}_{\text{TotalValidVotes},w,2004}} \times 100 \quad (\text{B.1})$$

The 2004 elections for London coincided with the European Parliament elections. This affects the patterns we observe in our data on electoral outcomes. Far-right parties are known to fare better in second-order elections such as these. Holding EU elections on the same day would have prompted individuals to vote in higher numbers for these parties in the London elections, too, thus increasing their levels of support in 2004. The data depicted in the maps below confirm that support largely declined in 2012, relative to 2004. However, since the coincidence of EU and London elections in 2004 affects all of London, it does not distort our ward-level analysis of the link between EU8+2 migration and support for these parties.

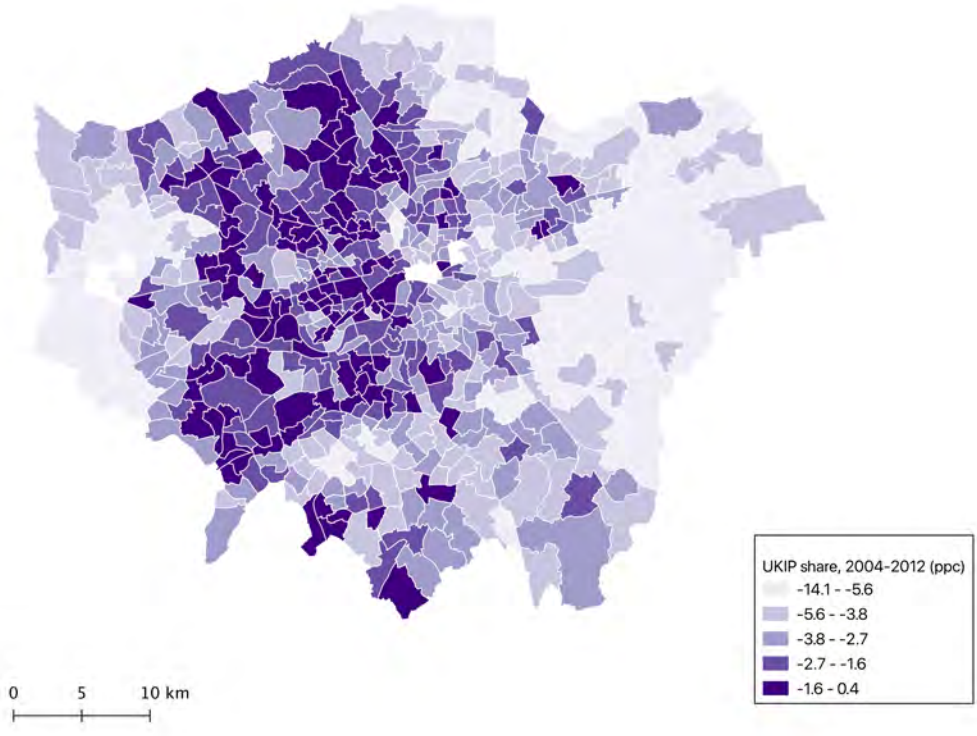


Figure B.8: Percentage point change in votes for **UKIP**, 2004-12 by ward

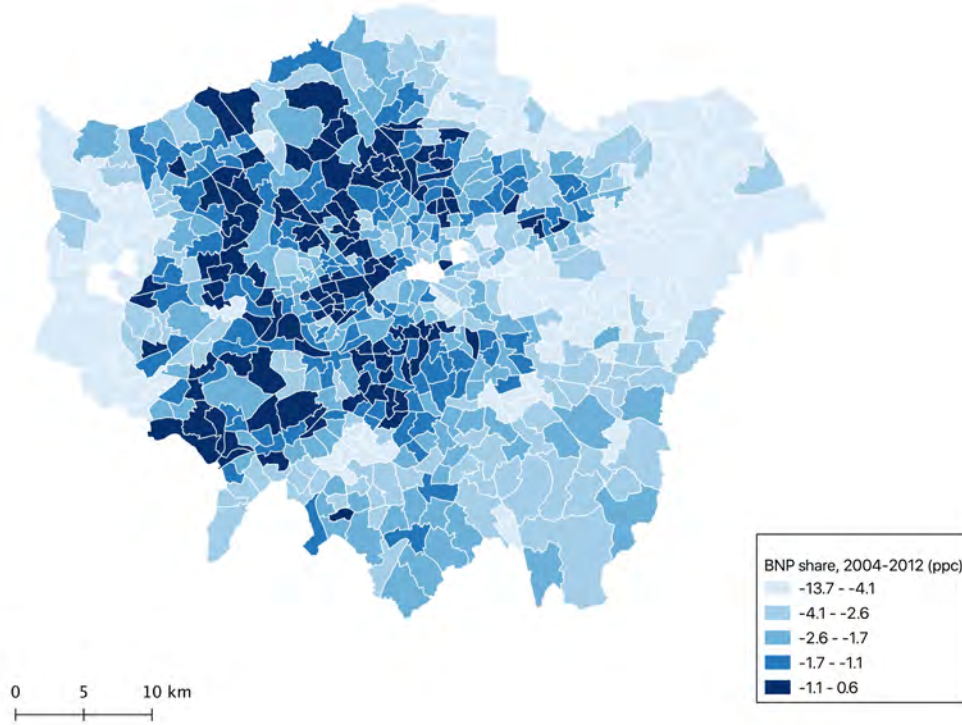


Figure B.9: Percentage point change in votes for the **BNP**, 2004-12 by ward

To construct the percentage point change in new EU residents and in all other foreign-born residents, we use commissioned data from the Office for National Statistics: Census 2001 Commissioned Table C1397 and Census 2011 Commissioned Table CT0226 (<https://data.london.gov.uk/census/tools/country-of-birth-ward-tool/>, last accessed November 19, 2020).

$$\Delta Residents_{c,w} = \frac{Residents_{c,w,2011}}{Residents_{Total,w,2011}} \times 100 - \frac{Residents_{c,w,2001}}{Residents_{Total,w,2001}} \times 100 \quad (\text{B.2})$$

For the main independent variable, the percentage point change in new EU residents, subscript  $c$  in equation B.2 refers to residents born in Bulgaria, Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, Romania, Slovakia or Slovenia who resided in ward  $w$  at the date of the census. Our calculation of the percentage point change in all other foreign-born residents involves subtracting the number of residents born in an EU8+2 country from the total number of non-UK born residents.

The remaining control variables are obtained from two sources available on the London Datastore website. We obtain information on the proportions of unemployed, retired, and economically inactive students for each ward released by the Census Information Scheme (2011) and data on household income and median house prices released by the Greater London Authority (2013). The former data are already released in percentage

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format, so no transformations are needed for the variables  $\Delta Unemployed\ residents\ (pp)$ ,  $\Delta Retired\ residents\ (pp)$  and  $\Delta Student\ residents\ (pp)$ . The median house price data (in pound sterling) come from the Land Registry, which publishes full postcode price paid data on their website. The median household income (also in pounds) was calculated by the Greater London Authority and released for 2001/02, 2007/08, and 2012/13. We then construct the variable  $\Delta Median\ household\ income\ (\text{£}000s)$  by subtracting the median income in 2001/02 from that in 2012/13 and re-scaling to thousands of pounds. Table B.3 below shows the summary statistics for all variables.

Table B.3: Descriptive statistics

	Min	Mean	Max	Std.Dev.	Obs
BNP votes 2004	13	130.09	480	94.33	620
BNP votes 2004 (pct.)	0.69	5.17	24.97	4.01	620
BNP votes 2012	7	58.89	251	41.37	620
BNP votes 2012 (pct.)	0.21	2.36	14.95	2.02	620
Absolute change BNP votes	-272	-71.20	14	57.75	620
$\Delta$ BNP votes (pp)	-13.70	-2.81	0.55	2.26	620
UKIP votes 2004	27	221.15	790	136.06	620
UKIP votes 2004 (pct.)	2.44	8.50	23.14	4.62	620
UKIP votes 2012	22	123.50	491	75.31	620
UKIP votes 2012 (pct.)	0.88	4.72	15.69	2.79	620
Absolute change UKIP votes	-367	-97.64	7	72.75	620
$\Delta$ UKIP votes (pp)	-14.09	-3.78	0.35	2.52	620
Votes 2004	978	2651.05	5042	650.69	620
Turnout 2004	33.38	36.93	41.49	2.51	620
Votes 2012	1263	2740.03	4693	618.77	620
Turnout 2012	19.30	34.15	51.72	5.34	620
$\Delta$ Turnout	-18.83	-2.78	18.34	5.58	620
UK-born residents in 2001	3504	8374.77	15178	1941.89	620
UK-born residents in 2011	3383	8288.62	14942	2031.53	620
Absolute change native population	-3284	-86.15	3231	700.65	620
EU2 residents in 2001	0	9.73	65	8.74	620
EU8 residents in 2001	0	59.11	431	51.89	620
New EU residents 2001 (pct.)	0.00	0.59	3.38	0.44	620
EU2 residents in 2011	3	115.91	857	136.59	620
EU8 residents in 2011	13	402.99	2265	341.71	620
New EU residents 2011 (pct.)	0.31	3.80	15.79	2.85	620
$\Delta$ New EU residents (pp)	-0.06	3.20	15.28	2.71	620
All other foreign-born residents 2001 (pct.)	3.99	26.34	57.60	12.23	620
All other foreign-born residents 2011 (pct.)	4.69	32.21	63.74	12.45	620
$\Delta$ All other foreign-born residents (pp)	-1.88	5.87	18.69	3.69	620
$\Delta$ Unemployed residents (pp)	-0.42	0.43	1.62	0.32	620
$\Delta$ Retired residents (pp)	-0.41	-0.01	0.68	0.12	620
$\Delta$ Student residents (pp)	-0.49	0.43	2.84	0.38	620
Median household income 2001/02 (£000s)	17.01	27.31	46.11	5.03	620
Median household income 2012/13 (£000s)	25.09	39.27	88.33	7.40	620
$\Delta$ Median household income (£000s)	5.61	11.95	42.22	2.89	620
Median house price in 2001 (£000s)	73.50	173.03	730.00	70.07	620
Distance from closest A6/757 bus stop (km)	0.28	10.65	29.72	6.29	620
Distance from closest train station (km)	0.37	10.96	25.61	6.16	620
2001 initial shares	0.00	0.16	1.05	0.13	620
Shift-share, 2001 initial shares	0.00	450.39	2763.76	355.64	620
1991 initial shares	0.01	0.16	1.65	0.18	620
Shift-share, 1991 initial shares	15.17	450.78	4174.61	501.21	620

## B.5 Estimates of EU8+2 voter registration rates

Between November 2018 and May 2019, we contacted the electoral services managers of London's 32 boroughs and requested information on the number of electors by nationality in each ward. Eleven boroughs included in Figure B.10 were able to supply this information for 2011. The figure summarizes the ward-level distributions of EU8+2 nationals on the electoral registers of these boroughs, relative to the number of residents aged 18 or older

and born in an EU8+2 country (from Census 2011 Commissioned Table CT0796 compiled by the Office for National Statistics). We omit the Harold Wood ward in Havering, where our estimate is 133% (201 electors against 151 residents). Several factors introduce inaccuracies. First, the census and registry data refer to different dates. Second, the census data used here refer to country of birth instead of nationality. Third, the electoral registers may not capture recent arrivals or departures. Finally, there is a margin of error in census counts.

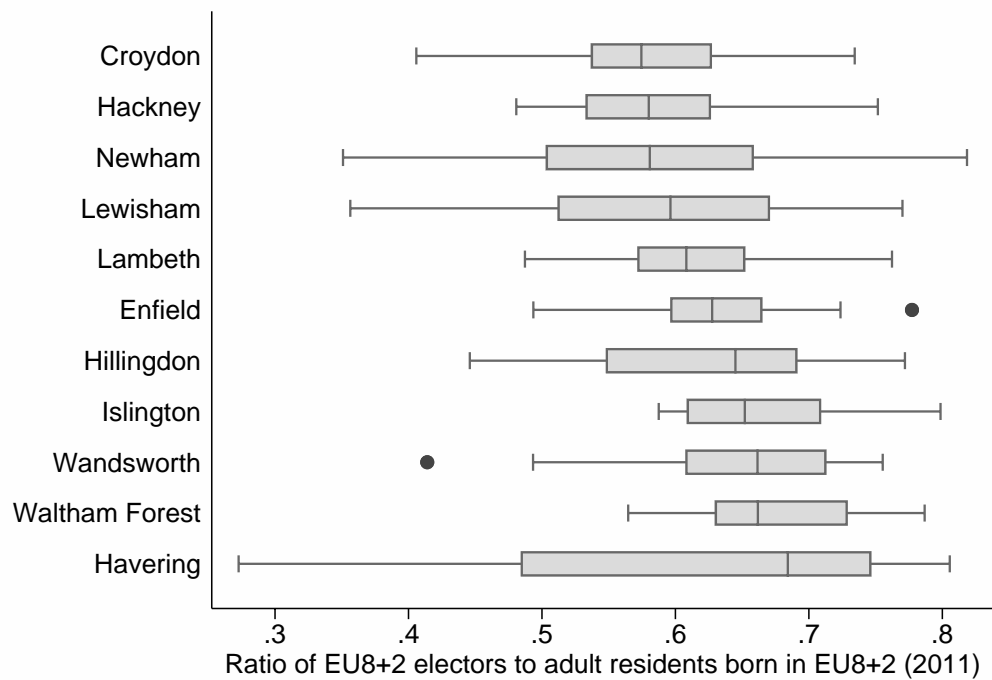


Figure B.10: Estimated EU8+2 voter registration rates across wards in selected boroughs (2011)

## B.6 Alternative instrumental variables

Before creating various alternative instruments, we first had to adjust the 1991 census data. In generating estimates of the initial distribution of the population from the EU8+2 countries across wards in London in 1991, we faced two obstacles. First, six of these ten countries did not exist until 1991 or later and hence the populations born in them and residing in London were not directly captured in the 1991 UK census. The affected countries are Estonia, Latvia, and Lithuania (which were part of the USSR until their independence in 1991); Slovenia (which was part of Yugoslavia until its independence in 1991); and the Czech Republic and Slovakia (which became independent states in 1993 following the dissolution of Czechoslovakia). Second, the ward structure at the time of the 1991 census is different from the 2004 ward structure that underpins our empirical analysis.



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To use the 1991 census data thus required a process for imputing the 1991 distribution of the population from the later EU8+2 countries using 2004 ward boundaries.

We started by compiling 1991 total population estimates for the EU8+2 countries, which we obtained from the World Bank. The World Bank data draw on United Nations population estimates, national census data, as well as data from various regional and other statistical agencies. We cross-checked these estimates against those of the United Nations Population Division's "World Population Prospects: 2019 Revision" data and found differences to be small. We also obtained total population estimates for the USSR (from the 1989 census) and Yugoslavia (from the 1991 census) as reported on Wikipedia. For the USSR, the 1989 census is the last undertaken before the union's dissolution and provides the closest available estimate.

Next, we used this information to construct weights for the population from later EU8+2 countries who in the 1991 UK census report their country of birth as USSR, Yugoslavia, or Czechoslovakia. This entailed dividing the World Bank 1991 population estimates for Estonia, Latvia, Lithuania, Slovenia, the Czech Republic, and Slovakia by the relevant total population of the entity of which they were part before the end of the Cold War. For example, we divided the World Bank's estimate of the 1991 population of Slovenia (1999429) by the total population in the 1991 Yugoslav census (23229846) to estimate the share of the Slovenian population in the total population of Yugoslavia at the time, about 0.0861. We repeated this for the three Baltic countries using the 1989 Soviet census, while for Czechoslovakia we derived the denominator by summing the World Bank's 1991 population estimates for the Czech Republic and Slovakia.

At the end of this process, we thus had a set of weights that we could use to back out estimates of the population from the later EU8+2 countries in London at the time of the 1991 UK census. We obtained the number of people recorded in the 1991 census as born in the USSR, Yugoslavia, Czechoslovakia, as well as Bulgaria, Hungary, Poland, and Romania. For the latter four, we made no adjustments. For the former three, we backed out estimates for the relevant EU8+2 country using the weights described above. For instance, to obtain an estimate of the Slovenian population in a given ward in London in 1991, we multiplied the census count for those giving Yugoslavia as their country of birth in that ward by 0.0861, and equivalent for the five other countries. This left us with 1991 population estimates across all wards in London of those born in all entities later included in the new EU accession countries. The steps involved in this calculation are summarized in Table B.4.

Table B.4: Estimating EU8+2 ward populations from the 1991 UK census

Country	1991 population	Our weight is constructed by dividing the 1991 population by the population of:	Our 1991 estimate is the product of the weight and the ward count for the relevant COB category
Bulgaria	8632367	Bulgaria	1 x COB Bulgaria
Czech Republic	10308578	Czechoslovakia	0.660304 x COB Czechoslovakia
Estonia	1561314	USSR	0.005445 x COB USSR
Hungary	10373400	Hungary	1 x COB Hungary
Lithuania	3704134	USSR	0.012919 x COB USSR
Latvia	2650581	USSR	0.009244 x COB USSR
Poland	38246193	Poland	1 x COB Poland
Romania	23001155	Romania	1 x COB Romania
Slovak Republic	5303294	Czechoslovakia	0.339696 x COB Czechoslovakia
Slovenia	1999429	Yugoslavia	0.086072 x COB Yugoslavia
USSR (1989)	286730819		
Yugoslavia (1991)	23229846		
Czechoslovakia (1991)	15611872		

Sources: The country of birth (COB) categories for the 1991 UK census were defined by the Office of Population Censuses and Surveys (Annex A of “1991 Census: Definitions Great Britain”). EU8+2 population estimates for 1991 are from the World Bank DataBank (series SP.POP.TOTL downloaded on April 1, 2020); the USSR census total is from [https://en.wikipedia.org/wiki/Soviet\\_Census\\_\(1989\)](https://en.wikipedia.org/wiki/Soviet_Census_(1989)) and the total for Yugoslavia from [https://en.wikipedia.org/wiki/Demographics\\_of\\_the\\_Socialist\\_Federal\\_Republic\\_of\\_Yugoslavia](https://en.wikipedia.org/wiki/Demographics_of_the_Socialist_Federal_Republic_of_Yugoslavia).

It is important to acknowledge our underpinning assumptions. Lacking any other information, we assumed that emigration from the USSR, Yugoslavia and Czechoslovakia was uniformly distributed. Hence, we assumed that Slovenians were as likely to leave Yugoslavia for the UK as were Serbs or Croats, for example; or that someone from the territory of Estonia was as likely to emigrate to the UK as someone in, say, Irkutsk. This may or may not hold in individual circumstances, but it is a reasonable approach given that we lack systematic data on the composition of those emigrating to the UK from the different regions of these countries.

Our final step in making the 1991 data usable for our study was their conversion into 2004 ward boundaries. We could not commission the 1991 data in 2004 ward boundaries from the Office for National Statistics. Thus, we carried out a spatial join using the 1991 and 2004 ward shapefiles, to refit the 1991 data to the 2004 boundaries. We did so with the “proportional sum” operation that assumes individuals are uniformly distributed in any given ward, so that the proportion of the 1991 ward that falls into a 2004 ward can be used to attribute individuals to the new ward. Without other information about the distribution of these immigrants within wards, this was the most neutral assumption. This operation was carried out with QGIS version 3.4.

We then used these data to construct two alternative instruments that are commonly used in the migration literature. First, we calculate initial shares:

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$$z_{2w,t} = \frac{\sum_{c \in EU8+2} Migrants_{c,w,t}}{\sum_{c \in EU8+2} TotalMigrants_{c,t}} \quad (B.3)$$

Here,  $z_{2w,t}$  represents the initial share of all new EU migrant residents in ward  $w$  at time  $t$ , which can be either 1991 or 2001. It is calculated as the sum of the number of migrants from each of the ten Eastern enlargement countries  $c$  that joined the EU in 2004 or 2007 and who resided in a given ward  $w$ , divided by the total number of migrants from these countries across all wards at that time.

Next, we construct an alternative variable that is a version of the widely used shift-share instrument:

$$z_{3w,t} = \sum_{c \in EU8+2} Share_{c,w,t} \times Shift_{c,2011-2001} \quad (B.4)$$

The variable  $z_{3w,t}$  distributes the London-wide inflow of migrants from an EU8+2 country (the “shift”) using weights depending on an initial spatial distribution of immigrants from that country (the “share”). For either 1991 or 2001,  $Share_{c,w,t}$  is the initial share in ward  $w$  of immigrants across all wards in our dataset who were born in country  $c \in \{EU8+2\}$ .  $Shift_{c,2011-2001}$  is the total flow of migrants from country  $c$  into all wards between 2001 and 2011.<sup>1</sup>

One concern related to shift-share instruments in the context of our study is that using pre-accession census data of Central and Eastern European residents may only be partly related to subsequent migration flows. This is because these data may more accurately capture specific sub-categories of these migrants, such as highly skilled workers, students, or older immigrants (Becker and Fetzer 2016). Furthermore, the methodological debate about shift-share instruments highlights the importance of the exogeneity of the initial shares (Goldsmith-Pinkham, Sorkin and Swift 2020). In our case, this is more plausible in 1991. However, the data for that year suffer from other inaccuracies due to the transformations required to make them usable for our study.

We further considered instruments based on proximity to alternative transport links, especially train stations. This is calculated in the same fashion as our bus stop instrument, focusing on three train stations with direct connections to the two airports: King’s Cross/St

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<sup>1</sup>As we examine the impact of changes in the share of EU8+2 migrants, we also compiled an alternative version where we scale the instrument by 2001 ward population. The results are very similar, although the scaled versions of our shift-share instruments are slightly weaker in the first-stage regressions. We report results with the non-scaled versions in this appendix and include models with the alternative scaled versions in the replication package.

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Pancras for Luton; and Tottenham Hale and Liverpool Street for Stansted. However, this instrument is less convincing, because these stations provide valuable travel links to other destinations and hence a wider group of migrants. For instance, St Pancras provides direct train links to Paris and Brussels and adjacent King’s Cross offers a direct underground link to Heathrow Airport. Also, as we discuss in the paper, trains are more expensive than busses, which make them less appealing to those seeking the most affordable journey to the airport. Overall, this makes the exclusion restriction less plausible.

The first-stage results presented in Table B.5 show a strong and positive relationship between the train station, initial share, or shift-share instruments, and the increase in the share of migrants from new EU countries. The instruments using 1991 data are weaker than the alternatives, but close to the conventional cut-off as indicated by the F-statistics. The second-stage estimates presented in Table B.6 are much larger than their OLS baselines. The coefficients with the transport instrument are similar to those with the other instruments for both UKIP and the BNP.

In Table B.7, we present second-stage estimates for a selection of multiple instruments. We combine the bus stop instrument with the initial share instrument described in Equation B.3. For comparison, the first two columns show the 2SLS results presented in the main analysis, columns three and four combine the transport instrument with the 1991 shares of migrant residents from new EU countries and columns five and six use the 2001 distribution of migrants as the “initial” share of EU8+2 residents. The results are almost identical.

Table B.5: First-stage regressions

	(1)	(2)	(3)	(4)	(5)	(6)
Distance from closest A6/757 bus stop (km)	-0.20*** (-5.62)					
Distance from closest train station (km)		-0.16*** (-5.04)				
Shift-share, 1991 initial shares (000s)			0.87** (3.22)			
1991 initial shares				2.01** (2.81)		
Shift-share, 2001 initial shares (000s)					1.82*** (4.53)	
2001 initial shares						4.45*** (4.16)
$\Delta$ All other foreign-born residents (pp)	-0.07 (-1.73)	-0.08 (-1.88)	-0.08 (-1.77)	-0.07 (-1.67)	-0.06 (-1.55)	-0.06 (-1.48)
$\Delta$ Unemployed residents (pp)	1.00* (2.49)	0.89* (2.21)	0.60 (1.51)	0.56 (1.42)	0.84* (2.18)	0.76 (1.96)
$\Delta$ Retired residents (pp)	-1.38 (-1.63)	-1.29 (-1.56)	-1.31 (-1.64)	-1.33 (-1.64)	-1.18 (-1.48)	-1.22 (-1.53)
$\Delta$ Student residents (pp)	-0.03 (-0.15)	-0.07 (-0.30)	0.06 (0.26)	0.04 (0.21)	0.06 (0.30)	0.05 (0.25)
$\Delta$ Median household income (£000s)	-0.09 (-1.22)	-0.08 (-1.09)	-0.14* (-2.09)	-0.12 (-1.74)	-0.09 (-1.18)	-0.10 (-1.35)
Median house price in 2001 (£000s)	-0.01*** (-4.73)	-0.01*** (-4.63)	-0.01*** (-4.33)	-0.01*** (-4.16)	-0.01*** (-4.35)	-0.01*** (-4.32)
Observations	620	620	620	620	620	620
Borough fixed effects	✓	✓	✓	✓	✓	✓
F-test on excluded instrument	31.56	25.37	10.38	7.92	20.55	17.32
R <sup>2</sup>	0.61	0.60	0.59	0.59	0.61	0.60

Notes: OLS estimates with t-statistics based on robust standard errors. The outcome is the percentage point change ( $\Delta$  pp) in residents from new EU member states between 2001 and 2011. For presentational purposes, we report the coefficient for our shift-share instruments in units of thousands. The instrument using distance to the closest train station with direct connections to the relevant airports refers to King's Cross/St Pancrass for Luton; and Tottenham Hale and Liverpool Street for Stansted. All  $\Delta$  in the control variables refer to the percentage point change between 2001 and 2011, apart from  $\Delta$  Median household income, which refers to the period 2001/02 to 2012/13, the closest available data. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Table B.6: Comparison between different instruments

	OLS	Transport instrument		1991 instrument		2001 instrument	
		Bus	Train	Shift-share	Initial shares	Shift-share	Initial shares
Outcome: $\Delta$ UKIP votes (pp)							
$\Delta$ New EU residents (pp)	0.01 (0.21)	0.67*** (3.53)	0.65** (3.13)	0.54* (2.19)	0.71* (2.14)	0.70*** (3.30)	0.76** (3.04)
Outcome: $\Delta$ BNP votes (pp)							
$\Delta$ New EU residents (pp)	0.06 (1.54)	0.61*** (3.70)	0.55** (3.00)	0.58** (2.66)	0.63* (2.30)	0.62*** (3.37)	0.60** (3.11)
Observations	620	620	620	620	620	620	620
Borough fixed effects	✓	✓	✓	✓	✓	✓	✓
First-stage controls	✓	✓	✓	✓	✓	✓	✓

Notes: OLS or 2SLS estimates with t-statistics based on robust standard errors. The dependent variable for the first panel is the percentage point change ( $\Delta$  pp) in votes cast for the UK Independence Party between 2004 and 2012. The outcome variable for the second panel is the percentage point change in votes cast for the British National Party between 2004 and 2012. The first column shows OLS estimates and the remaining six columns report 2SLS estimates. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Table B.7: Second-stage estimates with single vs. multiple instruments

	Bus stops		Bus stops & '91 shares		Bus stops & '01 shares	
	UKIP	BNP	UKIP	BNP	UKIP	BNP
$\Delta$ New EU residents (pp)	0.67*** (3.53)	0.61*** (3.70)	0.68*** (3.77)	0.61*** (3.96)	0.71*** (4.05)	0.61*** (4.10)
$\Delta$ All other foreign-born residents (pp)	-0.09* (-2.12)	-0.07 (-1.76)	-0.08* (-2.09)	-0.07 (-1.76)	-0.08* (-2.02)	-0.07 (-1.80)
$\Delta$ Unemployed residents (pp)	-1.42*** (-3.41)	-1.46*** (-4.07)	-1.42*** (-3.42)	-1.46*** (-4.08)	-1.44*** (-3.38)	-1.46*** (-4.07)
$\Delta$ Retired residents (pp)	4.20*** (4.91)	3.11*** (4.08)	4.22*** (4.94)	3.12*** (4.13)	4.26*** (4.83)	3.11*** (4.08)
$\Delta$ Student residents (pp)	-0.67* (-2.08)	-0.92*** (-3.48)	-0.67* (-2.07)	-0.92*** (-3.48)	-0.67* (-2.05)	-0.92*** (-3.49)
$\Delta$ Median household income (£000s)	0.14* (2.05)	0.04 (0.75)	0.14* (2.00)	0.05 (0.74)	0.14* (2.04)	0.04 (0.74)
Median house price in 2001 (£000s)	0.01*** (4.31)	0.01*** (4.72)	0.01*** (4.32)	0.01*** (4.75)	0.01*** (4.39)	0.01*** (4.85)
Observations	620	620	620	620	620	620
Borough fixed effects	✓	✓	✓	✓	✓	✓
Cragg-Donald F-statistic	32.50	32.50	18.64	18.64	24.97	24.97

Notes: 2SLS estimates with t-statistics based on robust standard errors. The dependent variable for columns 1, 3, and 5 is the percentage point change ( $\Delta$  pp) in votes cast for the UK Independence Party between 2004 and 2012. Columns 2, 4, and 6 have as outcome the percentage point change in votes cast for the British National Party between 2004 and 2012. Columns 1 and 2 estimate Equation 3.3. The remaining columns instrument the percentage point change in new EU migrants between 2001 and 2011 using both the proximity to a A6 or 757 bus stop and the initial shares of new EU migrants in 1991 (columns 3 and 4) or 2001 (columns 5 and 6). All  $\Delta$  in the control variables refer to the percentage point change between 2001 and 2011, apart from  $\Delta$  Median household income, which refers to the period 2001/02 to 2012/13, the closest available data. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

## B.7 Supplementary results

Table B.8: Effect on the absolute change in the number of votes cast for UKIP and the BNP

	OLS		2SLS		Reduced Form	
	UKIP	BNP	UKIP	BNP	UKIP	BNP
Δ New EU residents (pp)	2.64** (2.81)	3.08*** (3.66)	27.18*** (4.29)	18.82*** (4.47)		
Δ All other foreign-born residents (pp)	-1.56* (-2.04)	-1.36 (-1.95)	0.52 (0.39)	-0.03 (-0.03)	-1.46* (-1.99)	-1.40* (-2.07)
Δ Unemployed residents (pp)	-40.79*** (-4.15)	-38.07*** (-4.89)	-52.08*** (-3.78)	-45.31*** (-4.66)	-24.93** (-2.59)	-26.52*** (-3.53)
Δ Retired residents (pp)	94.99*** (5.79)	63.19*** (4.97)	130.39*** (4.69)	85.89*** (4.33)	92.87*** (5.63)	59.92*** (4.81)
Δ Student residents (pp)	-5.36 (-0.76)	-16.84** (-2.98)	-5.91 (-0.66)	-17.20** (-2.60)	-6.81 (-0.98)	-17.82** (-3.09)
Δ Median household income (£000s)	0.10 (0.10)	-1.46 (-1.88)	3.16 (1.47)	0.50 (0.35)	0.67 (0.68)	-1.22 (-1.48)
Median house price in 2001 (£000s)	0.13* (2.56)	0.13*** (3.42)	0.31*** (3.71)	0.25*** (4.24)	0.03 (0.58)	0.06 (1.37)
Distance from closest A6/757 bus stop (km)					-5.56*** (-5.76)	-3.85*** (-5.56)
Observations	620	620	620	620	620	620
Borough fixed effects	✓	✓	✓	✓	✓	✓

Notes: OLS or 2SLS estimates with t-statistics based on robust standard errors. The dependent variable for columns 1, 3 and 5 is the absolute change in votes cast for the UK Independence Party between 2004 and 2012. Columns 2, 4 and 6 have as outcome the absolute change in votes cast for the British National Party between 2004 and 2012. Columns 1 and 2 estimate the OLS results. Columns 3 and 4 estimate the second-stage equation. The final two columns show the reduced form for the bus stop instrument. All Δ in the control variables refer to the percentage point change between 2001 and 2011, apart from Δ Median household income, which refers to the period 2001/02 to 2012/13, the closest available data. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Table B.9: Effect on turnout

	OLS	First Stage	2SLS	Reduced Form
$\Delta$ New EU residents (pp)	-0.76*** (-9.02)		0.38 (0.92)	
$\Delta$ All other foreign-born residents (pp)	-0.54*** (-8.04)	-0.07 (-1.73)	-0.45*** (-5.30)	-0.48*** (-6.23)
$\Delta$ Unemployed residents (pp)	0.87 (1.19)	1.00* (2.49)	0.31 (0.38)	0.70 (0.88)
$\Delta$ Retired residents (pp)	3.57* (2.35)	-1.38 (-1.63)	5.20* (2.46)	4.68** (2.63)
$\Delta$ Student residents (pp)	-1.98*** (-3.95)	-0.03 (-0.15)	-2.01*** (-3.75)	-2.02*** (-3.89)
$\Delta$ Median household income (£000s)	0.38* (2.41)	-0.09 (-1.22)	0.52** (2.63)	0.48** (2.61)
Median house price in 2001 (£000s)	0.01 (1.02)	-0.01*** (-4.73)	0.01* (2.18)	0.01 (1.78)
Distance from closest A6/757 bus stop (km)		-0.20*** (-5.62)		-0.08 (-0.96)
Observations	624	620	624	624
Borough fixed effects	✓	✓	✓	✓
F-test on excluded instrument		31.56		

Notes: OLS or 2SLS estimates with t-statistics based on robust standard errors. The dependent variable for columns 1, 3 and 4 is the percentage point change ( $\Delta$  pp) in turnout between 2004 and 2012. The outcome variable in column 2 is the percentage point change in residents from new EU member states between 2001 and 2011. Column 1 presents the OLS results. Column 2 estimates Equation 3.2, while column 3 estimates the second-stage equation. The final column shows the reduced form for the bus stop instrument. All  $\Delta$  in the control variables refer to the percentage point change between 2001 and 2011, apart from  $\Delta$  Median household income, which refers to the period 2001/02 to 2012/13, the closest available data. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$



Table B.10: Different control variables

	No controls		Baseline control		Fixed effects		Demographic controls		Deprivation Index	
	UKIP	BNP	UKIP	BNP	UKIP	BNP	UKIP	BNP	UKIP	BNP
Δ New EU residents (pp)	7.54*	6.28*	1.18***	1.08***	0.88***	0.85***	0.77***	0.74***	1.58**	1.37**
	(2.23)	(2.21)	(7.18)	(7.08)	(3.68)	(3.99)	(3.66)	(3.98)	(2.64)	(2.75)
Median house price in 2001 (£000s)			0.04***	0.03***	0.02***	0.02***	0.02***	0.02***		
			(11.98)	(10.37)	(6.12)	(5.58)	(5.82)	(5.15)		
Δ All other foreign-born residents (pp)							-0.11**	-0.09*	-0.23***	-0.20***
							(-2.69)	(-2.25)	(-3.72)	(-3.78)
Δ Retired residents (pp)							3.71***	2.78**	5.87***	4.57**
							(4.07)	(3.27)	(3.40)	(3.11)
Δ Student residents (pp)							-1.07**	-1.23***		
							(-3.22)	(-4.41)		
Δ Average deprivation index score									-0.07	-0.09
									(-0.57)	(-0.77)
Observations	620	620	620	620	620	620	620	620	620	620
Borough fixed effects	✗	✗	✗	✗	✓	✓	✓	✓	✓	✓
F-test on excluded instrument	5.35	5.35	84.69	84.69	27.65	27.65	28.05	28.05	9.19	9.19

Notes: 2SLS estimates with t-statistics based on robust standard errors. The dependent variable for columns 1, 3, 5, 7, and 9 is the percentage point change ( $\Delta$  pp) in votes cast for the UK Independence Party between 2004 and 2012. Columns 2, 4, 6, 8, and 10 have as outcome the percentage point change in votes cast for the British National Party between 2004 and 2012. All control variables refer to the percentage point change between 2001 and 2011, apart from the change in the average deprivation index score (2007 to 2010) and in median household income (2001/02 to 2012/13). The average deprivation index score serves as an alternative to our standard socio-economic controls. It is based on the Index of Multiple Deprivation (IMD), which is an official governmental measure of relative deprivation for small areas in England. Seven areas of deprivation are combined in this score: income, employment, education, health, crime, housing and living environment. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

# Appendix C

## Naturalisation Trends in post-Brexit Britain – a new citizen profile

### C.1 Discussion on the role of the incumbent in naturalisation uptake

The role of the incumbent in naturalisation decisions deserves a closer look. Data from the Home Office shows citizenship uptake during the period 1962-2019. I plot this against the incumbent parties for a visual inspection of the trends. Figure C.1 shows how successful and unsuccessful applications for British citizenship have changed since the late 80s. Labour administrations have received and approved more applications than Conservative ones, but they also declined more requests than Tory governments.

While the figure shows a sharp up-tick in successful citizenship applications since the late 90s, the available data does not allow for a causal investigation into the role played by the change in leadership around that time (Labour took power after a long period of Conservative leadership). A simple bivariate regression (Table C.1) however reveals the number of successful citizenship applications is lower under Conservative administrations and this difference is statistically significant at the 95% confidence level. Tory leadership is also associated with a decline in the two most popular methods of acquiring British citizenship: through residence and marriage, although only the latter is statistically significant. An increase in citizenship uptake can be seen as evidence of a global Britain, where immigrants do not just come to live and work in the country, but also form more meaningful connections with the local community. While speculating over the mechanisms behind and the decisions to naturalise is tempting, these results suffer from many omitted variable biases and should not be interpreted causally.

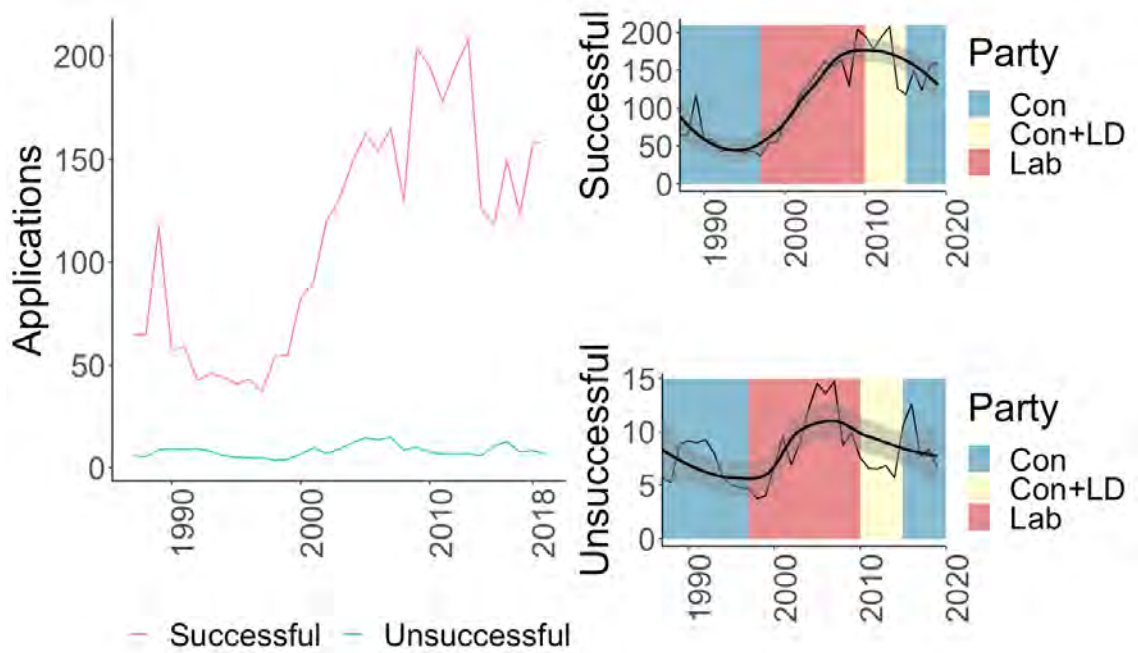


Figure C.1: Applications (Thousands) by outcome and incumbent

Table C.1: Incumbent influence on successful citizenship applications

	Overall	Residence	Marriage	Residence – Marriage
Conservative	-30,964*	-12,906	-8,153*	-4,753
	(-2.12)	(-1.70)	(-2.58)	(-0.92)
Observations	58	58	58	58

*t* statistics in parentheses; \*  $p < 0.05$

## C.2 Full sample - Breakdown by age and gender

The main analysis presents the results at the country level. However, the naturalisation data received from the Home Office is much richer and contains a breakdown of naturalisation numbers by gender and age group<sup>1</sup>. By using this disaggregated data, the number of observations increases significantly (from 2493 to 32,890). To avoid concerns over artificially inflating the sample size, this analysis was not included in the main paper. However, gender and age are important factors in the naturalisation decision, as they can serve as proxies for risk-taking behaviours. Age can also serve as a loose proxy for length of stay in the country or job security. For these reasons, I felt compelled to report the results. The average number of successful applications across these groups is 67, or roughly 3.22% of the resident population in the country which matches the same characteristics (Table C.2).

Table C.2: Descriptive statistics

	Mean	Std.Dev.	Obs
Naturalisation	67.23	251.19	32,890
Estimated % residents naturalised	3.22	2.10	32,553
Year	2010	4.66	32,890

I apply a difference in differences design:

$$Y_{g,a,c,t} = \alpha_c + \lambda_t + \rho_g + \sigma_a + \beta \text{Affected by Brexit}_{g,a,c,t} \times \text{Dual nationality permitted}_{c,t} + \eta \text{Incumbent}_{UK} + \varepsilon_{g,a,c,t} \quad (\text{C.1})$$

where  $Y_{g,a,c,t}$  are the two outcome of interest (the absolute number of successful citizenship applications and the estimated percentage of residents who naturalised in any given year) for residents of gender  $g$  and age group  $a$ , born in country  $c$ , who naturalise in year  $t$ . I account for country  $\alpha_c$ , year  $\lambda_t$ , gender  $\rho_g$  and age group  $\sigma_a$  fixed effects and control for the incumbent party in the UK. All models use cluster standard errors at the gender-age-country level.

<sup>1</sup>The age groups are Under 16, 16-17, 18-24, 25-34, 35-44, 45-54, 55-59, 60-64 and Over 65.

Table C.3: DID effects of Brexit referendum on naturalisation numbers

	(1)	(2)	(3)	(4)	(5)
Affected by Brexit	60.65*** (15.48)	41.10*** (5.68)	66.84*** (9.01)	66.84*** (8.58)	66.84*** (8.58)
Dual nationality permitted		-16.44*** (-4.36)	-5.89 (-1.26)	-5.89 (-1.17)	-5.89 (-1.17)
Affected by Brexit × Dual nationality permitted		29.25*** (3.38)	26.19** (3.05)	26.19* (2.16)	26.19* (2.16)
Labour incumbent					-7.46* (-2.03)
Constant	43.73*** (15.22)	55.11*** (14.24)	49.61*** (13.13)	49.61*** (11.12)	57.07*** (11.33)
Observations	32,890	32,890	32,890	32,890	32,890
R-squared			0.03	0.03	0.03
Country, age, gender fixed effects	✗	✗	✓	✓	✓
Year fixed effects	✗	✗	✓	✓	✓
Clustered standard errors	✗	✗	✗	✓	✓

The outcome is the total number of successful applications for British citizenship. *t* statistics between parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Table C.4: DID effects of Brexit referendum on percentage of residents naturalised

	(1)	(2)	(3)	(4)	(5)
Affected by Brexit	0.31*** (6.35)	0.41*** (4.59)	2.63*** (38.26)	2.63*** (67.39)	2.63*** (67.39)
Dual nationality permitted		-0.37*** (-8.85)	0.12** (2.67)	0.12 (1.59)	0.12 (1.59)
Affected by Brexit × Dual nationality permitted		-0.11 (-1.00)	-0.26** (-3.26)	-0.26*** (-5.35)	-0.26*** (-5.35)
Labour incumbent					1.94*** (39.61)
Constant	3.32*** (123.88)	3.58*** (90.52)	3.63*** (102.51)	3.63*** (73.07)	1.69*** (27.88)
Observations	32,553	32,553	32,553	32,553	32,553
R-squared			0.46	0.46	0.46
Country, age, gender fixed effects	✗	✗	✓	✓	✓
Year fixed effects	✗	✗	✓	✓	✓
Clustered standard errors	✗	✗	✗	✓	✓

The outcome is the estimated % residents naturalised. It uses the ONS annual population estimates by region of birth and naturalisation numbers by region and country of birth from the Home Office to calculate the share of residents by country of birth who successfully apply for British citizenship. *t* statistics between parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

### C.3 Parallel trends

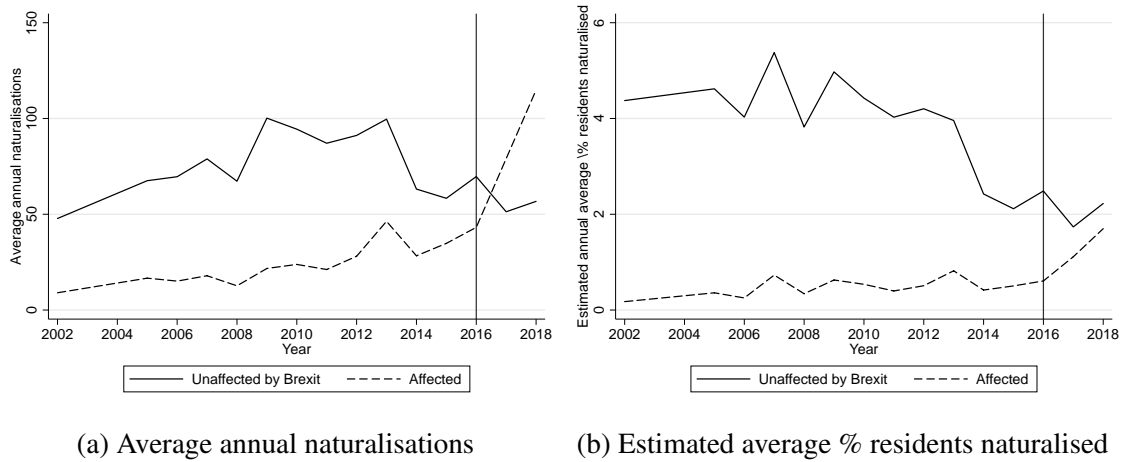


Figure C.2: Parallel trends for the basic model (1), full sample

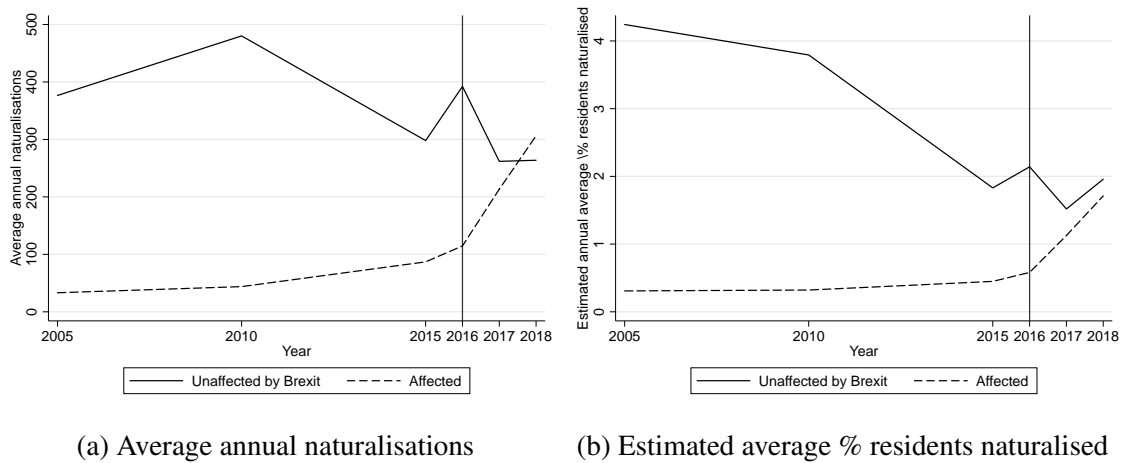


Figure C.3: Parallel trends for the basic model (1), OECD sample

Table C.5: Parallel trends (Naturalisation numbers), full countries sample

	2011	2012	2013	2014	2015	2016	2017
Placebo treatment, 5 year(s) before	227.07 [-61.30,515.44]						
Placebo treatment, 4 year(s) before		260.80 [-15.95,537.56]					
Placebo treatment, 3 year(s) before			278.42* [10.88,545.96]				
Placebo treatment, 2 year(s) before				151.69 [-158.68,462.07]			
Placebo treatment, 1 year(s) before					202.91 [-215.01,620.83]		
treated						609.04 [-274.05,1492.13]	978.49* [179.15,1777.84]
Observations	1632	1801	1972	1984	1992	496	330

Outcome variable is number of naturalisations. % Confidence intervals in parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Table C.6: Parallel trends (% naturalised), full countries sample

	2011	2012	2013	2014	2015	2016	2017
Placebo treatment, 5 year(s) before	-0.15 [-0.44,0.15]						
Placebo treatment, 4 year(s) before		-0.20 [-0.53,0.13]					
Placebo treatment, 3 year(s) before			-0.26 [-0.62,0.09]				
Placebo treatment, 2 year(s) before				-0.37 [-0.78,0.04]			
Placebo treatment, 1 year(s) before					-0.29 [-0.84,0.27]		
treated						-1.02*** [-1.23,-0.82]	-0.59*** [-0.77,-0.42]
Observations	1615	1784	1955	1967	1975	496	330

Outcome variable is the estimated percentage of residents who naturalise.% Confidence intervals in parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$