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RESEARCH ARTICLE

Optimal multi-action treatment allocation: A two-phase field experiment to boost immigrant naturalization

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Summary

Research underscores the role of naturalization in enhancing immigrants' socio-economic integration, yet application rates remain low. We estimate a policy rule for a letter-based information campaign encouraging newly eligible immigrants in Zurich, Switzerland, to naturalize. The policy rule assigns one out of three treatment letters to each individual, based on their observed characteristics. We field the policy rule to one-half of 1717 immigrants, while sending random treatment letters to the other half. Despite only moderate treatment effect heterogeneity, the policy tree yields a larger, albeit insignificant, increase in application rates compared with assigning the same letter to everyone.

KEYWORDS

immigrant naturalization, policy learning, randomized field experiment, statistical decision rules, targeted treatment

1 | INTRODUCTION

Policymakers frequently need to select among alternative treatment options. While one of the stated aims of empirical research is to provide new insights to inform decision-making processes, the primary focus is usually on estimating averages of treatment effects rather than providing direct guidance on how to design assignment mechanisms for alternative treatments. In practice, the empirical researcher specifies a statistical model and estimates the efficacy of each treatment using an experimental or observational sample, while the decision-maker assigns the treatment, interpreting the point estimates as if they were true. This approach, termed *as-if* maximization by Manski (2021), tends to yield one-size-fits-all rules assigning the same treatment to the wider population. Such one-size-fits-all policies seem inefficient given that treatment effects frequently exhibit relevant effect heterogeneity across observations and the increasing availability of administrative data providing rich individual characteristics.

Policy learning provides a framework for directly estimating statistical decision rules, so-called policy rules, which prescribe treatments to individuals based on their observed characteristics (also known as profiling or targeting). While its origins date back to statistical decision theory (Savage, 1951; Wald, 1950), the seminal work of Manski (2004) sparked a flourishing literature in econometrics which has developed methods for estimating statistical treatment rules, initially focusing on data drawn from randomized control trials (Hirano & Porter, 2009; Manski, 2004; Stoye, 2009; 2012) but subsequently also covering observational data under unconfoundedness assumptions (Athey & Wager, 2021; Manski, 2007; Zhou et al., 2022; see Hirano & Porter, 2020 for a review). While applied research using policy learning is still relatively scarce, previous work has revealed the potential for data-driven treatment allocation across a variety of domains, including active labor market programs (e.g., Frölich, 2008; Lechner & Smith, 2007), vaccines accounting for spill-over effects

(Kitagawa & Wang, 2023), deforestation-reducing policies (Assunção et al., 2022), antimalaria subsidies under budget constraints (Bhattacharya & Dupas, 2012), energy use information campaigns (Ida et al., 2022; Gerarden & Yang, 2022), and maximizing fundraising (Cagala et al., 2021).

In this preregistered study, we co-design and evaluate an individualized treatment allocation program with the goal of facilitating the naturalization of eligible immigrants in the City of Zurich, Switzerland. An expanding body of literature is utilizing close naturalization referendums or temporal discontinuities created by policy reform to enable credible comparisons between naturalized and nonnaturalized immigrants to demonstrate that acquiring host-country citizenship offers long-term integration benefits for immigrants and, indirectly, to host societies. These benefits span various integration dimensions, including employment and earnings (Gathmann & Keller, 2018; Hainmueller et al., 2019), political efficacy, knowledge, and participation (Hainmueller et al., 2015), as well as social incorporation and cooperation (Felfe et al., 2021; Hainmueller et al., 2017; Keller et al., 2015). Yet, despite these benefits, naturalization rates remain low in many countries, with a median annual naturalization rate (number of naturalized immigrants divided by number of immigrants) of 1.9% in Europe and 3.1% in the U.S. (Ward et al., 2019). Against this background, policymakers at the national level in Estonia, Ireland, Latvia, North Macedonia, Spain and at the local level in Germany, Italy, Switzerland, and the USA have deployed information campaigns to boost citizenship applications and fully reap the integration benefits of naturalization (Huddleston, 2013).

Informed by existing research (e.g., Bauböck et al., 2006; Bloemraad, 2002; National Academies of Sciences, et al., 2016) and the insights of integration and naturalization bureaucrats of the City of Zurich, this study considers interventions that address three specific hurdles blocking eligible immigrants' path to citizenship. These hurdles include the following: (i) the perceived complexity of the application process, (ii) knowledge gaps about the requirements for naturalization, and (iii) the feeling of not being welcome to naturalize. To address the first two hurdles, we co-designed specific information letters with the City of Zurich. For the third hurdle, a letter sent by the Mayor of City of Zurich encouraged immigrants to apply. In line with recent recommendations by Haaland et al. (2023), we opted for three separate treatment letters with accompanying flyers to ensure that each letter is short and easy to understand. Addressing all hurdles in a combined treatment letter with several flyers is likely counterproductive due to the limited time and attention that recipients devote when reading the letters.[2]

Since it is unknown which treatment letter is optimal for maximizing the individual application probabilities and given that the optimal treatment choice may differ among individuals, we derive a multi-action policy rule. This policy rule is structured as a decision tree, which is referred to as a "policy tree." Policy trees are introduced by Athey and Wager (2021) for binary and by Zhou et al. (2022) for multivalued treatments. In our context, the policy tree selects one treatment from a set of three treatment options for each eligible immigrant based on their individual characteristics including residency, nationality, and age. The treatment options are incorporated into three different letters with enclosed flyers sent out by the City of Zurich. Thus, by applying policy learning, we allow the optimal content and framing of the information provision to vary with observed immigrant characteristics. The policy rule is chosen to maximize the application rate for naturalization, the first step in the process of acquiring Swiss citizenship.

Policy trees possess several strengths that make them a particularly promising method for immigrant naturalization and other sensitive policy contexts. First, policy trees allow policymakers and researchers to select those variables that can be used to tailor treatment assignment and, more importantly, exclude those that should not be used (e.g., protected characteristics such as religion) and quantify the costs of exclusion in terms of foregone treatment efficacy. Second, policy trees make transparent which variables, and which variable values, guide treatment assignment. This is in contrast to black-box *plug-in* rules, providing no insights into what drives treatment allocation. Related to the second strength is the third: Policy trees are easy to visualize and easy to explain to users of the research—for example, policymakers, case officers, and subjects receiving treatment assignment—even if they lack training in statistics. Together, transparency and interpretability are important steps towards satisfying requirements for explainable artificial intelligence (AI), for example, as outlined in recent proposals for the regulation of AI by the European Commission (2021) and The White House (2022). Finally, from a practical perspective, the so-called offline approach of policy trees, which learns policies from a single data batch, is often easier to implement in a public policy context than adaptive approaches training policy rules dynamically over time (e.g., Caria et al., 2020).

After introducing the methodology of policy learning, we illustrate the practical feasibility of the targeted assignment rule and evaluate its benefits using a tailored, two-phase randomized controlled trial. In the first phase of our field exper-

[2]A large literature in behavioral economics stresses that information processing is costly and provides evidence that individuals often fail to translate all available information into optimal decisions (for recent reviews, see Handel & Schwartzstein, 2018; Gabaix, 2019; Maćkowiak et al., 2023).

iment, we randomly allocate 60% of our sample of 5145 citizenship-eligible immigrants to receive one of three letters addressing specific naturalization hurdles. Based on first-wave application outcomes and leveraging observed treatment effect heterogeneity, we estimate the optimal multi-action policy rule using the estimation framework of Zhou et al. (2022). In the second phase, we field the fitted policy rule on one-half of the remaining sample while sending random treatment letters to the other half. Adopting terminology from reinforcement learning, we refer to these two phases as the exploration phase (aimed at gathering knowledge about treatment efficacy) and the exploitation phase (aimed at implementing the reward-maximizing strategy), respectively. We evaluate the performance of the derived policy rule against random treatment allocation, one-size-fits-all policy rules assigning the same treatment to everyone, and a model-free *plug-in* rule assigning the treatment with the largest estimated treatment effect. We find that policy trees can capture the vast majority of treatment effect heterogeneity of the more flexible but less transparent and noninterpretable *plug-in* rule. Despite only moderate levels of heterogeneity, the policy tree yields a larger, albeit insignificant, increase in take-up than each individual treatment.

Our study relates to three fields of empirical research. First, sparked by methodological advances, especially the advent of causal forests (due to Wager & Athey, 2018), there is a burgeoning literature estimating heterogeneous treatment effects using machine learning (e.g., Davis & Heller, 2017; Knaus et al., 2022; Knittel & Stolper, 2021).[3] While studies in this literature emphasize the potential of estimating heterogeneous effects for improved targeting, they usually do not explicitly derive interpretable targeting rules. Second, we build on the expanding literature applying statistical decision rules. The vast majority of applied studies, including those discussed above (i.e., Assunção et al., 2022; Bhattacharya & Dupas, 2012; Frölich, 2008; Kitagawa & Wang, 2023; Lechner & Smith, 2007), only provide backtest results about the ex-post performance of policy targeting rules. Ida et al. (2022) propose a policy-learning framework that allows participants to self-select their treatment and apply their method to a residential energy rebate program. Closest to our study are Gerarden and Yang (2022) and Cagala et al. (2021). Gerarden and Yang (2022) follow the methodology of Kitagawa and Tetenov (2018) to estimate policy rules for a behavioral intervention targeted at reducing household electricity usage, but do not implement the derived policy rules. Similar to us, Cagala et al. (2021) consider policy trees in an application to maximizing fundraising and gauge the performance of the estimated policy tree on out-of-sample data. We add to this literature by fielding the estimated optimal policy rule in the second phase of our experiment, which allows us to directly evaluate the performance against other policy rules. Furthermore, both Cagala et al. (2021) and Gerarden and Yang (2022) focus on the choice between two treatment options, whereas we are concerned with the more challenging problem of multi-action policy learning. Third, we contribute to the larger literature on informational interventions aimed at increasing take-up of government services and subsidies among eligible people (e.g., Bhargava & Manoli, 2015; Finkelstein & Notowidigdo, 2019; Goldin et al., 2022; Hotard et al., 2019). Beyond contributing to these three strands, this article aims to make policy learning accessible to a wider audience by offering an introduction relevant both for randomized field experiments and applications relying on observational data.

This article proceeds as follows. Section 2 provides an introduction to policy learning. Section 3 turns to our application. We contextualize our application, describe the data, the treatments, and the study design in Sections 3.1–3.4. We summarize the results of the exploration and exploitation phase in Sections 3.5 and 3.6. Section 4 concludes the study.

2 | MULTI-ACTION POLICY LEARNING

In this section, we provide a brief review of (multi-action) policy learning, with a special focus on the policy learning framework of Zhou et al. (2022). While we rely on a randomized experimental design to learn the optimal policy rule in our application, we also discuss the setting where one has to rely on unconfoundedness assumptions, thereby illustrating the generality of the methodological framework.

The aim of policy learning is to formulate a policy rule $\pi(X)$ designed to maximize the expected value of Y, the outcome of interest. A policy rule assigns a treatment a from the choice set of treatment options $\mathcal{A} = \{1, 2, \dots, D\}$ to each individual based on their observed covariates X. Note that $\mathcal{A}$ may include the no-treatment option. Formally, $\pi(X)$ is a function mapping individual characteristics to one of the treatment options in $\mathcal{A}$. For example, a policy rule might assign treatment 1 to every person below age 30, treatment 2 to individuals aged 30–40, and treatment 3 to individuals older than 40.

[3]Other methods for estimating conditional average treatment effects using machine learning include Chernozhukov, Demirer, et al. (2018) and Künzel et al. (2019). For an overview, see Knaus et al. (2021) and Jacob (2021).

2.1 | Estimating optimal policies

Before we turn to the estimation of optimal policies, it is instructive to consider a candidate policy rule $\pi'(X)$ and assess its effectiveness. We assume that we have access to the sample $\{Y_i, A_i, X_i\}$ for $i = 1, \ldots, n$, which is drawn from the joint population distribution P. The sample data include the treatment received, A_i, the realized outcome, Y_i, as well as observed individual i's characteristics X_i. In our application, the data stem from the exploration phase of the randomized controlled trial, but the general approach also extends to observational data.

As typical in the causal effects literature, we assume the existence of the potential outcomes $\{Y_i(1), Y_i(2), \ldots, Y_i(D)\}$, which are the outcomes if individual i had received treatments 1, 2, ..., D (Imbens & Rubin, 2015; Rubin, 1974). This allows us to define the expected reward of $\pi'(X)$, which is the expected value of the potential outcomes if the policy rule had been followed, that is, $Q(\pi'(X_i)) = E[Y_i(\pi'(X_i))]$ where $E[\cdot]$ denotes the expectation with respect to the population P. In nonexperimental settings, the fundamental challenge of estimating the reward of a candidate policy $\pi'(X)$ is that we only observe $Y_i = Y_i(A_i)$ and that individuals might self-select into treatment options that optimize their expected pay-off.

The offline policy learning literature commonly imposes the following set of assumptions[4]:

Assumption 1.

(a) Unconfoundedness: $Y_i(1), \ldots, Y_i(D)\} \perp A_i | X_i$.
(b) Overlap: There exists some $\eta > 0$ such that $e_a(X_i) \geq \eta$ for any $a \in \mathcal{A}$ and X, where $e_a(X_i) \equiv P(A_i = a | X_i)$ is the (generalized) propensity scores for treatment a.
(c) Boundedness: The potential outcomes are contained on a finite interval in $\mathbb{R}^D$.

Under these assumptions, we can evaluate the reward of a candidate policy π' by taking the weighted average across observations that align with the candidate policy rule, that is,

$$\hat{Q}_{IPW}(\pi'(X_i)) = \frac{1}{n} \sum_{i=1}^{n} \frac{\mathbb{1}\left\{A_i = \pi'(X_i)\right\} Y_i}{e_{A_i}(X_i)}. \tag{1}$$

We inversely weight by the (generalized) propensity score $e_a(X_i)$ to allow for endogenous selection into treatment (Kitagawa & Tetenov, 2018; Swaminathan & Joachims, 2015).

Suppose that the policymaker considers a number of policy rules that depend on X_i, for example, $\Pi' = \{\pi'(X_i), \pi''(X_i), \pi'''(X_i)\}$ where Π' is the set of candidate policies.[5] The optimal policy is the policy in the candidate set Π' that maximizes the expected reward; formally, $\pi^\star(X_i) = \arg\max_{\pi \in \Pi'} Q(\pi(X_i))$. Accordingly, we can leverage our sample to estimate the optimal policy rule as

$$\hat{\pi}(X_i) = \arg\max_{\pi \in \Pi'} \hat{Q}_{IPW}(\pi(X_i)), \tag{2}$$

where $\hat{\pi}(X_i)$ is the policy from the set of candidate policies Π' that maximizes the estimated reward. The performance of policy learner $\hat{\pi}(X_i)$, which estimates $\pi^\star(X_i)$ from the data, is measured by its regret, $R(\hat{\pi}(X_i)) = Q(\pi^\star(X_i)) - Q(\hat{\pi}(X_i))$. The regret measures the difference between the reward of the unobserved optimal policy and the value of the estimated policy.

[4]The assumptions are standard in the causal inference literature (e.g., Imbens, 2004; Rosenbaum & Rubin, 1983) and have been adopted more recently in the literature on offline policy learning (e.g., Kitagawa & Tetenov, 2018; Zhou et al., 2022). Unconfoundedness in (a) states that we observe all necessary covariates, which allows us to account for selection biases. The overlap assumption in (b) uses the definition of the (generalized) propensity score $e_a(X_i)$, which generalizes the definition of propensity scores to accommodate multi-valued treatments (Imbens, 2000). Specifically, $e_a(X_i)$ denotes the propensity of taking up treatment a given observable characteristics X_i. The boundedness assumption in (c) serves the purpose of simplifying mathematical proofs but can be replaced by weaker assumptions (Zhou et al., 2022).

[5]Note that we leverage the same covariates X_i to adjust for selection effects in (1) and to form policy rules in (2). There may, however, be good reasons to use distinct covariate sets in each step. For example, legal or ethical concerns might mandate the exclusion of protected characteristics from the policy rule (e.g., gender, nationality, religion). Yet, the inclusion of these characteristics in the propensity score estimation could be necessary if prior evidence suggests their potential influence on treatment allocation. In randomized experiments, a consistent estimation of the reward does not require covariate adjustment but can enhance statistical precision.

2.2 | Cross-fitting and double-robust estimation

If the propensity scores $e_a(X)$ are known, the regret converges to zero at $\sqrt{n}$-rate (Kitagawa & Tetenov, 2018; Swaminathan & Joachims, 2015). If the exact assignment mechanism is not known, which is typically the case in nonexperimental settings, we have to estimate $e_a(X)$ from the data. One approach is to estimate $e_a(X)$ using the full sample and plug the estimates into (1). However, this approach generally yields suboptimal convergence rates unless we impose strong convergence rate requirements on the first-step estimator (Kitagawa & Tetenov, 2018). The suboptimal performance can be attributed to the own-observation bias, which arises if the first-step estimation error from the propensity score estimation is correlated with the error associated with estimating the reward. To allow for a general class of data-adaptive nonparametric estimators, including popular supervised machine learners such as random forests, which are more robust towards unknown data structures, Zhou et al. (2022) combine two strategies for policy learning: cross-fitting and double-robust estimation using augmented inverse-propensity weighting (AIPW). We discuss each strategy in turn.

To illustrate how cross-fitting addresses the own-observation bias, consider the simple case where we randomly split the data into two subsamples, referred to as auxiliary and main samples. In the first step, we leverage the auxiliary sample for the estimation of conditional expectation functions (e.g., the propensity scores). The second step uses out-of-sample predicted values from the first step on the main sample to estimate the reward. This sample-splitting approach resolves the own-observation bias since the second step is, after conditioning on the auxiliary sample, independent from the first-step estimation error.

Cross-fitting extends this sample-splitting approach by flipping the auxiliary and main samples, thus effectively using the full sample for both the first- and second-step estimation. Cross-fitting also allows the sample to be split into more than two partitions.[6] Specifically, to implement cross-fitting, we randomly split the sample into K folds of approximately equal size. We use $\hat{e}_a^{-k(i)}(X_i)$ to denote the *cross-fitted* (generalized) propensity score of observation i for treatment a. The cross-fitted predicted value is calculated as the out-of-sample predicted value from fitting an estimator on all folds but fold $k(i)$, which is the fold that observation i falls into. Similarly, we introduce $\hat{\mu}_a^{-k(i)}(X_i)$ which is the cross-fitted predicted value of the outcome under treatment a using predictors X_i, that is, it is a cross-fitted estimate of $\mu_a(X_i) \equiv E[Y_i(a)|X_i]$.

Double robust estimation of the reward allows for nonrandom treatment assignment under uncounfoundedness. The estimator adjusts for biases arising from selective treatment allocation by combining the reweighting approach of inverse-propensity weighting as used in (1) with outcome adjustment (as used in regression-based adjustment). The advantage over the IPW estimator is that the resulting double-robustness property guarantees consistency if either the propensity scores $e_a(X_i)$ or the conditional expectation of outcome given covariates, that is, $\mu_a(X_i)$, are correctly specified. Using the cross-fitted estimates $\hat{e}_a^{-k(i)}(X_i)$ and $\hat{\mu}_a^{-k(i)}(X_i)$, we can define the cross-fitted AIPW (CAIPW) estimator of the reward as[7]

$$\hat{Q}_{CAIPW}(\pi(X_i)) = \frac{1}{n}\sum_{i=1}^{n}\left(\frac{Y_i - \hat{\mu}_{A_i}^{-k(i)}(X_i)}{\hat{e}_{A_i}^{-k(i)}(X_i)}\mathbb{1}\{A_i = \pi(X_i)\} + \hat{\mu}_{\pi(X_i)}^{-k(i)}(X_i)\right). \tag{3}$$

The first term in (3) adjusts the observed outcome by subtracting the conditional expectation of the outcome under the observed treatment and by inversely weighting with the propensity scores if the observed treatment, A_i, is equal to the treatment recommended by the policy, $\pi(X_i)$. The second term adds the conditional expectation of the outcome under the treatment assigned by the policy. Using the double-robust estimator of the reward, we can estimate the optimal policy by evaluating $\hat{Q}_{CAIPW}(\pi(X_i))$ for all candidate policies in Π', that is, we calculate $\hat{\pi}(X_i)_{CAIPW} = \arg\max_{\pi\in\Pi'}\hat{Q}_{CAIPW}(\pi(X_i))$.

2.3 | Policy class

So far, we have assumed a predefined set of candidate policies. In many applications, however, we wish to learn policies flexibly from the data instead of relying on a predefined set of policy rules. A fully flexible approach could assign each individual to the treatment for which the estimated treatment effect is the largest. This *plug-in policy rule* requires no functional form restrictions but may be inappropriate when stakeholders wish to learn about the drivers of treatment efficacy and have hesitations to rely on a black-box treatment assignment mechanism.[8]

[6]The causal machine learning literature frequently relies on sample splitting approaches, such as cross-fitting; see for example Chernozhukov, Chetverikov, et al. (2018) for the estimation of average treatment effects and Wager and Athey (2018) for the estimation of CATE using causal forests.

[7]The function $\mathbb{1}\{\cdot\}$ denotes the indicator function.

[8]For formal results on plug-in rules, see Hirano and Porter (2009) and Bhattacharya and Dupas (2012).

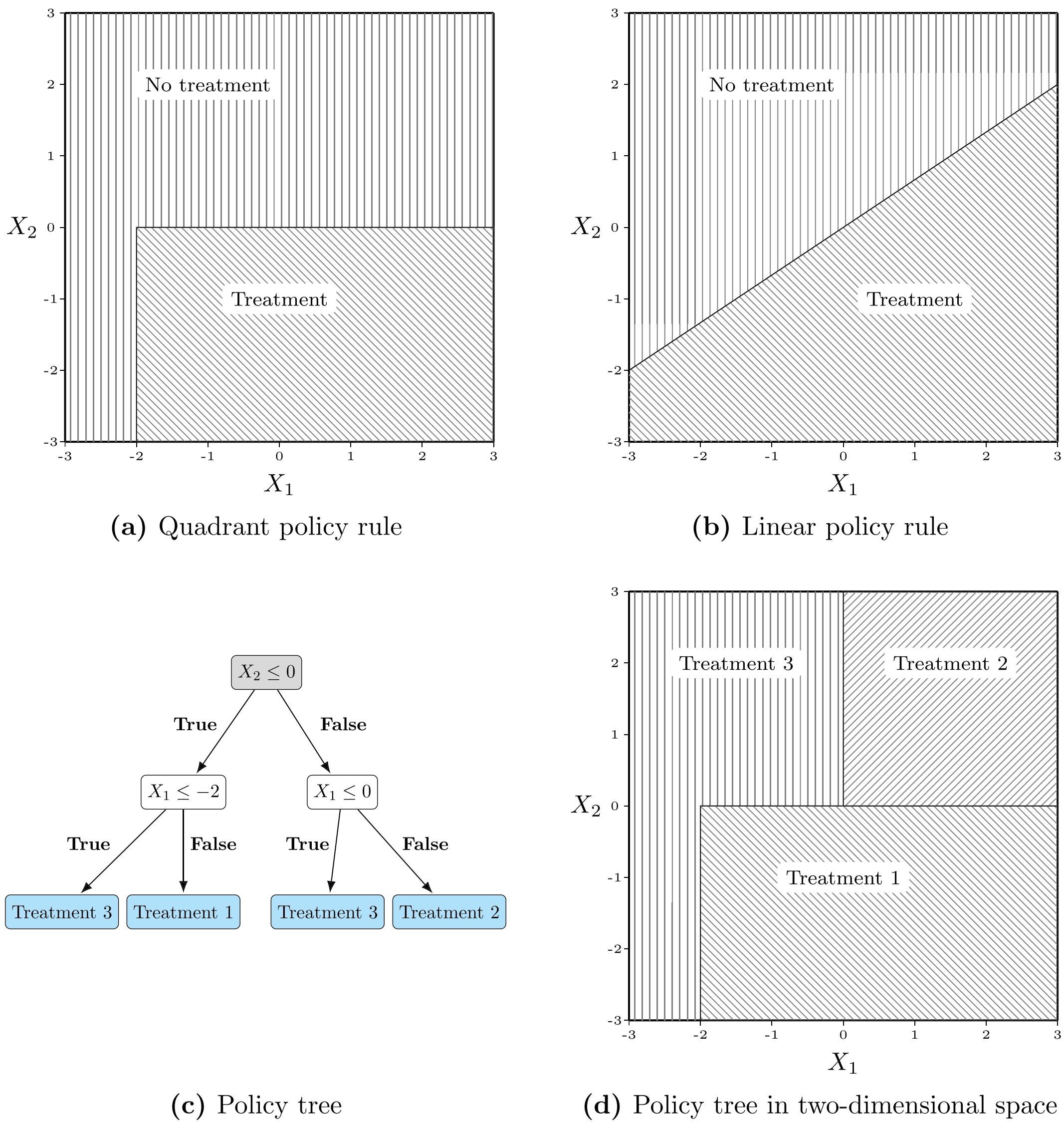

FIGURE 1 Illustrative examples of policy rules with different functional forms. *Note:* The figure shows three examples of policy rules that assign treatments based on two covariates X_1 and X_2. Panels (a) and (b) illustrate the quadrant and linear policy rule considered in Kitagawa and Tetenov (2018) for the case of a binary treatment. The quadrant rule is given by $\pi(X_i) = \mathbb{1}\{s_1(X_{1i} - \beta_1) \geq 0\}\mathbb{1}\{s_2(X_{2i} - \beta_2) \geq 0\}$. The linear policy rule is defined by $\pi(X_i) = \mathbb{1}\{\beta_0 + \beta_1 X_{1i} + \beta_2 X_{2i} \geq 0\}$. Panels (c) and (d) provide two alternative illustrations of the same policy tree with three treatment options.

Policy learning allows for the estimation of interpretable treatment rules from the data. To this end, we must choose a suitable policy class from which we estimate the optimal policy. Figure 1 illustrates several options. Figure 1a,b shows examples of the quadrant and linear policy rules considered by Kitagawa and Tetenov (2018) in an application to active labor market programs with a binary treatment and two covariates. Another possible policy class is given by trees. Trees are widely employed as predictive tools that construct predictions by splitting the feature space optimally into nonoverlapping regions. In the prediction context, classification and regression trees yield the same prediction for observations falling into the same region. In the policy context, observations falling into the same region are assigned the same treatment action. Athey and Wager (2021) suggest using trees for policy learning, and Zhou et al. (2022) generalize policy trees to settings where the treatment is multivalued.[9] Figure 1c shows an example of a multi-action policy tree, and Figure 1d visualizes the same tree in a two-dimensional space.

[9]Zhou et al. (2022) describe how the optimization of policy trees can be regarded as a mixed integer program.

3 | PERSONALIZING NATURALIZATION CAMPAIGNS

In this section, we apply the multi-action policy tree to an information campaign encouraging eligible immigrants residing in the City of Zurich to apply for Swiss citizenship. We introduce the policy context in Section 3.1 and discuss data and treatments in Sections 3.2 and 3.3. Section 3.4 summarizes the study design and estimation methodology. Results are presented in Sections 3.5 and 3.6.

3.1 | Background: Immigrant integration and citizenship

The integration of immigrants into the host-country fabric and economy is a central policy issue in many countries across the globe. One promising policy to foster integration is naturalization, that is, the process of awarding host-country citizenship to immigrants (Dancygier, 2010; Goodman, 2014). Observational studies relying on difference-in-difference models and regression discontinuity designs comparing similar naturalized and nonnaturalized immigrants show that acquiring host-country citizenship can positively impact the integration of immigrants by increasing their earnings and labor market attachment (Gathmann & Keller, 2018; Gathmann & Garbers, 2023; Govind, 2021; Hainmueller et al., 2019; Mazzolari, 2009; OECD, 2011; Vink et al., 2021), fostering political efficacy and knowledge (Hainmueller et al., 2015), spurring cultural assimilation and cooperation (Felfe et al., 2021; Keller et al., 2015), and reducing feelings of isolation and discrimination (Hainmueller et al., 2017).[10] This process can also benefit the host society by increasing immigrants' contributions to economic growth, lowering their dependency on welfare, and, by extension, reducing societal tensions and strengthening social cohesion (for reviews, see National Academies of Sciences, et al., 2016; Pastor & Scoggins, 2012).

Despite these potential benefits, naturalization rates remain low in many countries (Blizzard & Batalova, 2019). What explains this mismatch between the benefits of host-country citizenship and the low demand for naturalization? Previous evidence from surveys and qualitative studies suggest that uncertainty about the eligibility criteria such as residency and language requirements can prevent immigrants from applying (Bauböck et al., 2006; Gonzalez-Barrera et al., 2013). Other studies highlight that—particularly in hostile immigration environments—a lack of encouragement by politicians, public administration, or the general public might deter immigrants (Bauböck et al., 2006; Bloemraad, 2002; Bloemraad et al., 2008). Furthermore, in earlier research using a tailored survey, we find evidence for informational deficits and the feeling that an application is not welcome by the host society (Hangartner et al., 2023). Lastly, in countries that unlike Switzerland do not allow for dual citizenship, immigrants might not be willing to give up the passport from their origin country to obtain host-country citizenship.[11]

To boost naturalization rates, countries, states, and municipalities across Europe and the USA have begun to turn to information campaigns to overcome hurdles to citizenship acquisition for eligible immigrants. While the content and scope of these naturalization campaigns vary, they often combine information provision about the naturalization process and requirements with an encouragement to apply for citizenship. Yet, despite the growing popularity of these campaigns across Europe and the USA, there exists little experimental research to evaluate its effectiveness. An important exception is Hotard et al. (2019), who show that a low-cost nudge informing low-income immigrants about their eligibility for a fee waiver increased the rate of citizenship applications by 8.6 percentage points (from 24.5% in the control group to 33.1%). Most similar to our study is Hangartner et al. (2023), who evaluated previous versions of the naturalization campaign of the City of Zurich and showed that a similarly low-cost letter (about CHF 1.20 per person; see below) combining information and encouragement increased naturalization rates by about 2.5 percentage points (from 6.0% in the control group to 8.5%).

Past naturalization campaigns, including the one by the City of Zurich mentioned above, have typically relied on a one-size-fits-all approach—despite the substantial diversity of the immigrant population in terms of, for example, country of origin, language skills, and age. There are good reasons to suspect treatment effect heterogeneity along various dimensions: Immigrants' willingness to naturalize and their susceptibility to certain information letters might depend on their

[10] With the exception of Mazzolari (2009), who studies immigrants from Latin American countries in the USA, the studies referenced above focus on France, Germany, and Switzerland. This might limit external validity since we expect the benefits of naturalization to be context-dependent and generally decline with lower naturalization hurdles. While testing this hypothesis requires more comparative research, Vernby and Dancygier (2019) provide initial evidence from a correspondence test varying citizenship in fictitious applications in Sweden that is consistent with this conjecture.

[11] Whether a person is allowed to retain the previous citizenship when naturalizing in another country generally depends on the regulations of both the origin and host country. Switzerland has guaranteed the right to hold dual (or more) citizenship without restrictions since 1992. Hence, eligible immigrants seeking Swiss citizenship are subject only to restrictions of their origin countries. Among the 10 largest countries by nationality in our sample (which jointly amount to 72% of origin countries), only Austria and Spain generally do not allow for dual citizenship.

current nationality due to the specific dual citizenship regulations, the relative benefits in terms of visa requirements *vis-á-vis* third countries, the attachment to the home country, and the attitudes of native citizens towards specific immigrants groups. For example, immigrants who feel discriminated against might be more likely to be persuaded by a letter welcoming them to set roots and apply for citizenship in their host country. Furthermore, language requirements might be less of a concern for immigrants who speak the same language (such as Austrians and Germans in Switzerland). Thus, tailoring such campaigns to the specific needs of diverse immigrants promises to deliver both a deeper understanding of the different hurdles that immigrants face and to increase the effectiveness of the campaign.

3.2 | Data

We draw our data from administrative sources of the Canton of Zurich. The data include records of whether and when eligible immigrants submit an application for Swiss citizenship to the City of Zurich during the study period, which allows us to define the outcome variable of our analysis. The data also include additional covariates which we use to identify and leverage treatment effect heterogeneity. These covariates are age, gender, nationality, years of residency in Switzerland, and years of residency in Zurich. The data also include an address identifier which allows us to assign the treatment on a building level to minimize contamination by spill-over effects.

The study sample includes all immigrants in the City of Zurich who satisfy the following criteria:

1. They were born on or before June 30, 2003 (i.e., they must have been at least 18 years of age at the start of the study),
2. They arrived in Switzerland on or before June 30, 2011,
3. They arrived in Zurich City on or before June 30, 2019,
4. They must have possessed a permanent residence permit (C permit) at the time of randomization (August 2021), and
5. They must not have received any information or encouragement letter in the past.

The first criterion ensures that only adults are in the study. Criteria 2–4 ensure that the entire sample meets the current residency and permit requirements for citizenship. The sample includes 5145 individuals.

3.3 | Treatment letters

Combining insights from the existing literature and our own surveys, we identify three key barriers to naturalization: (i) perceived complexity of the naturalization process, (ii) perceived difficulty of and uncertainty about naturalization requirements, and (iii) perception that naturalization is not welcome. In collaboration with the City of Zurich, we developed three treatment letters, each of which puts emphasis on one of the hurdles. Each treatment involves the receipt of a letter sent by representatives of the City of Zurich. The treatments differ in the sender, content, wording and design of the letters. The per-unit costs of the three treatments range between 1.20 and 1.50 CHF and are thus negligible compared with the fiscal benefits of naturalization.[12] We chose to develop distinct letters to keep the letters brief and understandable, thus avoiding the risk of an informational overload (Haaland et al., 2023). The letters, including enclosed flyers, were written in German. Appendix A.2 in the supporting information contains copies of the original letters in German as well as an English translation.

The *Complexity letter* consists of a short informational cover letter written by the City Clerk of the City of Zurich (see Appendix A.2.1) and a flyer. The half-page cover letter informs recipients that they meet the basic requirements for Swiss citizenship and directs them to sources of further information about the citizenship application process. The flyer included in the *Complexity letter* (shown in Figure A.2.2 in the supporting information) attempts to tackle the perceived complexity of the naturalization process. The left-hand side of the flyer shows a video screenshot and a QR code that directs readers to the video, explaining the naturalization process in a simplified way. The right-hand side encourages readers to scan another QR code redirecting to the contact and advice webpage[13] of the City of Zurich's citizenship office.

[12]Hainmueller et al. (2019) quantify the long-term effect of naturalization on immigrants' earnings at CHF 4,500 per year, which implies an increase in tax revenues for Swiss municipalities of at least CHF 450 per year.

[13]The first QR code redirects to https://www.stadt-zuerich.ch/portal/de/index/politik_u_recht/einbuergerungen.html (last accessed on December 7, 2022). The second QR code redirects to https://www.stadt-zuerich.ch/portal/de/index/politik_u_recht/einbuergerungen/kontakt-und-beratung.html (last accessed on December 7, 2022).

The *Requirements letter* includes the same short informational cover letter as the *Complexity letter* but uses a different flyer addressing the perceived difficulty of the naturalization process (see Appendix A.2.3 in the supporting information). This flyer is also divided into two sections, each containing a descriptive text and a QR code. The QR code on the left-hand side redirects to the targeted, free-of-charge mobile application, which allows immigrants to study for the civics exam and test their knowledge with practice questions.[14] The section on the right lists the German language requirements for citizenship and the QR code redirects to a webpage containing more detailed information on the language requirements, exam costs, as well as a link to a practice language exam.[15]

The *Welcome letter* is an information and encouragement letter signed by the Mayor of the City of Zurich. The *Welcome letter* attempts to tackle the hurdle stemming from the perception that naturalization is not welcome (Hainmueller & Hangartner, 2013). The letter includes only a cover letter (shown in Appendix A.2.4 in the supporting information) that is a little less than one page long and contains three sections. The first section informs recipients that they meet the basic eligibility requirements for Swiss citizenship. The second section encourages them to play an active part in Zurich's political life by becoming a citizen. The last section briefly directs to sources for further information about the citizenship application process and states that the City hopes to see them at the next ceremony for new citizens. Hence, compared with the other two treatment letters, this letter puts more emphasis on the emotional and psychological aspects associated with naturalization and only provides minimal information.

3.4 | Experimental design and estimation methodology

This section summarizes the preregistered experimental design, estimation methodology, and evaluation strategy.[16] In the exploration phase of the project, we randomly divide the sample of 5145 eligible immigrants into two groups: Group A (60% of the sample) receives one of three treatment letters at random from the City of Zurich in October 2021, while Group B (40%) received no letter. The randomization design allocates one of the three treatment letters to individuals in Group A by building address and applied block randomization by nationality groups. The randomization by building address reduces the risk of spill-over effects among eligible immigrants living in the same or neighboring households. The block randomization by nationality group ensures that we have a roughly equal share of nationalities in Group A (including each subgroup receiving different letters) and Group B. We block on nationality groups given the importance of this effect moderator in earlier studies (Ward et al., 2019). The letters for this first wave were delivered on October 8, 2021.

The first-wage application outcomes enable us to estimate the average treatment effect of treatment letter d, that is, $E[Y_i(d) - Y_i(0)]$, and the conditional average treatment effect $E[Y_i(d) - Y_i(0)|X_i]$ where we use Y_i to denote the application outcome recorded at the end of March 2022 and $Y_i(d)$ its potential outcome under treatment d. The covariates X_i are country group of nationality, age, gender, years lived in Zurich, and years lived in Switzerland, which are constant over the sample period. We employ causal forests due to Wager and Athey (2018) and Athey et al. (2019), a nonparametric method for the estimation of heterogeneous treatment effects relying on random forests.

The main objective, however, is to leverage the first-wave application outcomes Y_i and individual characteristics X_i to fit a multi-action policy tree based on the estimation methodology of Zhou et al. (2022) outlined in Section 2. We opt for policy trees as they easily generalize to the multi-action settings. Furthermore, policy trees are transparent and simple to interpret, even without statistical training, making them attractive in a public policy context where users of the research vary in statistical literacy and often view black-box methods with skepticism. To select the tree depth, we consider a validation exercise: In each iteration, we randomly split the wave-1-data (including untreated) into training and test data with a 60/40 split and sample from each partition separately with replacement to construct bootstrapped training and validation data sets of sizes $n_1 = 4871$ and $n_2 = 1857$. We then fit a policy tree on the bootstrapped training data and estimate the difference in reward between alternative policy rules on the bootstrapped validation data.

[14]The mobile application is developed by the City of Zurich and named *Einbügerungstest Code Schweiz*, which translates to Naturalization Test Code Switzerland.

[15]The website, which the QR code redirected to, moved to https://www.stadt-zuerich.ch/portal/de/index/politik_u_recht/einbuergerungen/kenntnisse/sprachlicheanforderungen.html on October 21, 2022, due to a mistake by the website maintainers. As a consequence, the QR code broke more than 5 months after the letter was dispatched to wave two participants. We show in Table A.4 in the supporting information, where we only consider the naturalization applications recorded up to 5 months after letter dispatch, that our main results in Table 3 are not affected by this issue. We thus use, in line with the pre-analysis plan, application outcomes recorded 7 months after letter dispatch in the remainder of the study.

[16]The study was preregistered online (https://osf.io/9wf4t).

In the exploitation phase, we field the fitted policy tree on not-yet-treated individuals in Group B. Specifically, in order to evaluate the performance of the policy rule, we randomly subdivide Group B into two subgroups, referred to as Group B.1 and Group B.2, and send treatment letters to Group B.1 based on the estimated policy rule, while Group B.2 receive a random treatment letter (with one-third probability for each letter). We randomize by building address for the random division into Groups B.1 and B.2, as well as for the randomization of treatments within Group B.2. The City of Zurich delivered the letters for the exploitation phase on May 6, 2022.[17]

The evaluation compares the policy tree against no treatment, random treatment allocation, and conventional one-size-fits-all policy rules that always assign the same treatment to everyone, ignoring treatment effect heterogeneity. To this end, we estimate models of the form:

$$Y_{it} = W_{it}'\beta + f(X_i, \delta_t) + \varepsilon_{it} \tag{4}$$

where Y_{it} is the application outcome of eligible immigrant i at the end of wave $t \in \{1, 2\}$. We add the wave subscript t to accommodate the two-wave structure of the data. The outcomes for the evaluation analysis were recorded approximately 7 months after the date of letter dispatch t.[18] The time-invariant covariates X_i are defined above. δ_t is a dummy for wave $t \in \{1, 2\}$ and accounts for seasonal effects and other external shocks that may affect application rates. The vector W_{it} assigns individuals to treatment groups and is defined as $W_{it} = (Letter^1_{it}, Letter^2_{it}, Letter^3_{it}, Nothing_{it}, PolicyTree_{it})$ or $W_{it} = \left(Random_{it}, Nothing_{it}, PolicyTree_{it}\right)$, respectively, where $Letter^j_{it}$ is set to 1 if the individual i was randomly assigned to treatment letter $j \in \{1, 2, 3\}$ for wave t, 0 otherwise. $Nothing_{it}$ is set to 1 if the individual i has received no treatment in wave t, and $PolicyTree_{it}$ equals 1 if individual i has received the treatment letter assigned to them by the policy tree. Finally, $Random_{it}$ is set to 1 if individual i was randomly assigned to one of the three letters, 0 otherwise.

We estimate (4) by linear regression using only the elementary controls but also consider more flexible methods. Namely, we use post-double selection lasso (PDS Lasso Belloni et al., 2014) and double-debiased machine learning (DDML; Chernozhukov, Chetverikov, et al., 2018) where we extend the set of controls by interaction terms and second-order polynomials.[19] We cluster standard errors by building addresses, that is, the level at which the treatment was applied.[20]

3.5 | Results from the exploration phase: Learning the policy rule

We begin by analyzing the results from the exploration phase of the experiment using naturalization applications received by the end of March 2022 (i.e., wave 1). Descriptive statistics of the wave-1-data are provided in Table 1. We proceed in three steps: estimation of (conditional) averages of treatment effects, tuning policy trees using a validation exercise, and fitting the policy tree on the full wave-1-data.

First, we fit a multi-arm causal forest to estimate average treatment effects, as well as conditional average treatment effects by nationality group and years lived in Switzerland (Athey et al., 2019; Wager & Athey, 2018). Results are displayed in Figure 2.[21] The average treatment effects for the first-wave sample imply that the *Complexity letter* increases application rates by 1.08 p.p. ($s.e. = 0.91$), the *Requirements letter* by 4.33 p.p. ($s.e. = 1.04$), and the *Welcome letter* by 3.51 p.p. ($s.e. = 1.03$), relative to the control condition of no letter.[22]

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[17]Note that for practical reasons, there was a two-month time gap between measuring the application outcomes in March 2022 and sending out the letter in May.

[18]The application outcomes for the evaluation analysis were recorded in May 9 and December 9, 2022, respectively. In Table A.4 in the supporting information, we provide alternative results where we consider all application outcomes until March 21 and October 21, 2022, respectively (see fn. 15).

[19]For the Post-Double Selection Lasso, we use cluster-robust penalty loadings of Belloni et al. (2016). With regard to DDML, we use 10 cross-fitting folds, 5 cross-fitting repetitions and use stacking with a set of candidate learners including linear regression, lasso, ridge, random forests and gradient boosting (Ahrens et al., 2024)

[20]We note that the clustered standard errors do not account for sampling variability arising from the estimation of the policy rules. The issue is akin to the well-known generated regressor problem which occurs when a regressor is unobserved and replaced by a first-step estimate. The generated regressor problem is usually addressed using standard-error adjustments Pagan (1984) and Murphy and Topel (1985) or, most commonly, using bootstrapping; see e.g. review in Chen et al. (2023) or Wooldridge (2010). Neither of these approaches is feasible in our setting. Analytical standard errors are, to our knowledge, not available for this specific problem. Bootstrapping or other resampling techniques would require us to repeatedly field policy rules fitted on bootstrapped samples of the data in order to capture the variability in estimated policy rules, which is practically infeasible. We thus interpret the standard errors with caution.

[21]We removed 274 individuals who moved between October 2021 and March 2022, resulting in an estimation sample of 4871 individuals.

[22]See Hangartner et al. (2023) for a discussion the letters' efficacy in overcoming specific hurdles.

TABLE 1 Descriptive statistics of wave-1 data.

	Avg.	*St.dev.*	*Min*	*Max*	*Obs.*
Dependent variable:					
Naturalization application	0.07	0.25	0.00	1.00	4871
Covariates:					
Age	41.79	10.95	19.00	99.00	4871
Female	0.46	0.50	0.00	1.00	4871
Years in Switzerland	14.88	7.44	11.00	67.00	4871
Years in Zurich	9.08	4.06	3.00	20.00	4871
Regions:					
Americas & Caribbean	0.05	0.22	0.00	1.00	4871
Asia	0.06	0.25	0.00	1.00	4871
Central-East Europe	0.04	0.19	0.00	1.00	4871
Germany and Austria	0.37	0.48	0.00	1.00	4871
Italy	0.10	0.30	0.00	1.00	4871
Middle Easy and Northern Africa	0.01	0.12	0.00	1.00	4871
South-East Europe	0.12	0.32	0.00	1.00	4871
Spain and Portugal	0.12	0.33	0.00	1.00	4871
Stateless	0.00	0.04	0.00	1.00	4871
Sub-Saharian Africa	0.02	0.12	0.00	1.00	4871
Western Europe	0.11	0.32	0.00	1.00	4871

Note: The table shows summary statistics for covariates and dependent variables measured until March 2022.

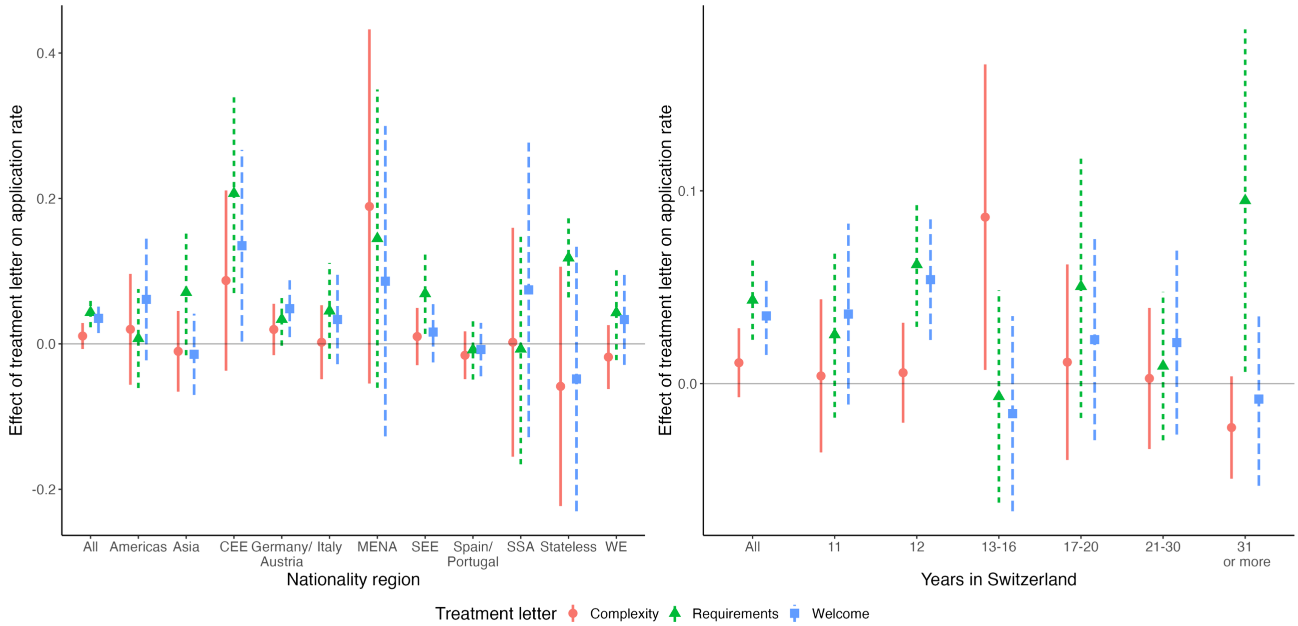

FIGURE 2 Average and conditional average treatment effects. *Note:* The figures shows the average and conditional average treatment effects by group where the groups are formed based on nationality and years of residence in Switzerland. The regions are the Americas, Asia, Central and East Europe (CEE), Germany and Austria, Italy, Middle East and Northern Africa (MENA), South-East Europe, Spain and Portugal, Stateless and Sub-Saharian Africa (SSA). The treatment effects are estimated using a multi-arm causal forest and using the R package `grf` (Athey et al., 2019; Tibshirani et al., 2022; Wager & Athey, 2018).

The left panel of Figure 2 shows only moderate heterogeneity in treatment effects by nationality. The *Welcome letter* appears to have slightly stronger effects for immigrants from Germany and Austria, consistent with the idea that Germans and Austrians do not perceive complexity and difficulty as major hurdles due to their cultural proximity and language. At the same time, the *Welcome letter* is also the most effective letter for immigrants from the Americas, which could indicate that this minority group does not feel very welcome in Switzerland. The relative effect size of the *Requirements letter* is particularly large for immigrants from Central-Eastern and South-Eastern Europe, as well as for stateless immigrants. The right panel of Figure 2 indicates that the *Complexity letter* has the largest effect on application rates among eligible

TABLE 2 Estimated reward of the policy rule compared to randomization, always the same treatment and no treatment.

	One-size-fits-all Complexity	Requirem.	Welcome	Random treatment	Policy tree $d = 2$	$d = 3$	$d = 4$	Plug-in rule
Nothing	0.917	4.096★★★	3.245★★★	2.755★★★	5.369★★★	5.545★★★	5.436★★★	6.396★★★
	(1.007)	(1.092)	(1.106)	(0.761)	(1.015)	(0.985)	(0.936)	(0.943)
Always 1		3.180★★	2.329★	1.838★★	4.452★★★	4.628★★★	4.520★★★	5.479★★★
		(1.295)	(1.291)	(0.736)	(1.174)	(1.115)	(1.080)	(1.071)
Always 2			−0.851	−1.342	1.273	1.448★	1.340★	2.299★★★
			(1.351)	(0.772)	(0.795)	(0.791)	(0.780)	(0.748)
Always 3				−0.491	2.124★★	2.300★★★	2.191★★★	3.150★★★
				(0.777)	(0.860)	(0.865)	(0.827)	(0.846)
Random					2.614★★★	2.790★★★	2.682★★★	3.641★★★
					(0.590)	(0.552)	(0.502)	(0.489)
Policy tree ($d = 2$)						0.176	0.067	1.027★★★
						(0.296)	(0.304)	(0.286)
Policy tree ($d = 3$)							−0.109	0.851★★★
							(0.246)	(0.248)
Hybrid tree ($d = 2$)								0.959★★★
								(0.221)

Note: The table reports the difference in estimated rewards between policy rules based on wave-1 data (including untreated immigrants of Group B). Specifically, each cell corresponds to the gain in reward of a specific policy rule (shown in columns) relative to alternative policy rules (listed in rows). The results are based on a resampling exercise where we randomly split the wave-1 data into training and test data using a 60/40 split and separately draw $n_1 = 4871$ and $n_2 = 1857$ observations with replacement from the training and test data. We use 500 repetitions and report the average difference in rewards and associated bootstrapped standard errors.***0.01. Significance level.**0.05. Significance level.*0.1. Significance level.

immigrants who have lived between 13 and 16 years in Switzerland. In contrast, eligible immigrants who have lived for more than 30 years in Switzerland are especially receptive to the requirements letter, suggesting that the perceived difficulty of the naturalization process may discourage some eligible immigrants from applying over long periods. This effect may also be partially driven by age since we also find the *Requirements letter* to have the largest effect among immigrants aged 46 and above (see Figure A.4 in the supporting information appendix). Finally, we find that men are slightly more receptive to the letter treatments overall than women, but the ranking of treatment letter efficacy is the same (see Figure A.4 in the supporting information).

Second, we conduct the validation exercise outlined above to assess the out-of-sample performance of various policy rules and to select the tree depth of the policy tree. We focus on policy trees with tree depths of 2 and 3. We also estimate a hybrid policy tree of depth 4. Hybrid policy trees rely on a computationally less costly but approximate optimization algorithm (Sverdrup et al., 2022). For comparison, we consider (i) one-size-fits-all rules that always assign one of the *Complexity*, *Requirements* or *Welcome* letters, (ii) random allocation of one of the three letters, and (iii) a model-free plug-in rule that assigns the treatment for which the estimated reward is the largest. We repeat the exercise 500 times and report average differences in rewards and bootstrapped standard errors in Table 2.[23] The table reports in each column the gain in reward of a specific policy choice compared to alternative policy rules (shown in rows). For instance, the coefficient of 0.917 ($s.e. = 1.007$) in the top-left entry corresponds to the gain in reward of a one-size-fits-all policy rule assigning the *Complexity letter* to everyone relative to a policy rule assigning no letter. We find that all three policy trees outperform each individual treatment letter as well as random treatment allocation. Among the three policy trees, the tree of depth 3 performs marginally better than trees of depths 2 and 4. As expected, the plug-in rule shows overall the best performance. However, the plug-in rule provides no insights into the drivers of treatment effects. The results thus highlight the trade-off between interpretability and performance but also show that, in this context, the best-performing policy tree is able to reach more than 85% of the performance of the plug-in rule.

Third, in light of the advantages and limited costs of policy trees in this setting, we opted for implementing the policy tree of depth 3. Following the approach of Zhou et al. (2022) as outlined in Section 2, we trained the policy tree on wave 1 data, including Group A (who received a letter in the first wave) and Group B (who did not receive a letter in the first wave). Since we randomized treatment assignment in the first wave, we did not need to estimate the propensity scores but

[23]We opted for this approach rather than K-fold cross-validation as it allows us to match the sizes of the training and validation data to the actual sample sizes. However, we obtain similar results when applying K-fold cross-validation.

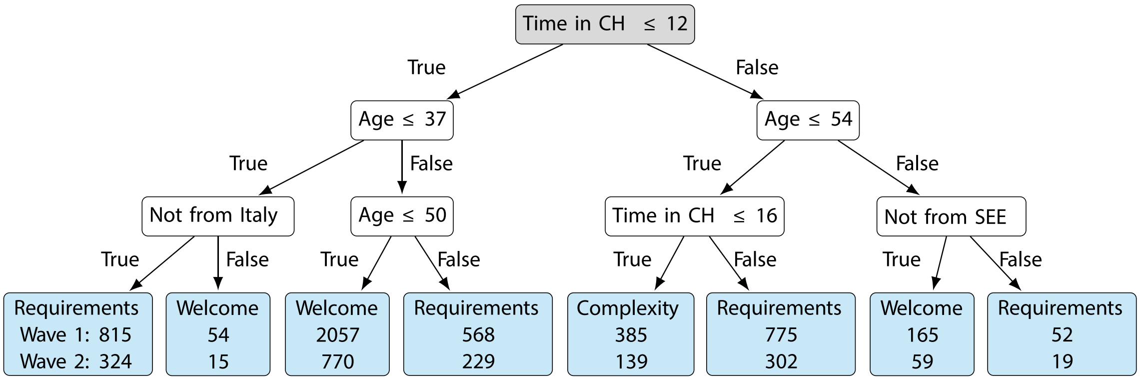

FIGURE 3 Fitted policy tree. *Note:* The figure shows the policy tree fitted to data from wave 1. The size of the training sample is 4871. The numbers at the bottom indicate the number of individuals assigned to each terminal node in the training sample and in Group B.

plugged the known treatment shares into (3).[24] We used multi-arm causal forests to estimate the double robust scores, although other estimators are possible. The fitted policy tree $\hat{\pi}$ of depth three is displayed in Figure 3. The boxes at the bottom of the tree show the assigned treatment for the wave-1 sample and the wave-2 sample (i.e., Group B) per terminal node. For instance, the very-left branch assigns individuals who have spent no more than 12 years in Switzerland are aged 37 years or younger and who are not from Italy to the requirements treatment. A total of 815 individuals in total and 324 individuals from Group B fall into that category. In total, 139 individuals of Group B are assigned to the *Complexity letter*, 874 individuals to the *Requirements letter*, and 844 to the *Welcome letter*.[25] The splits in the tree are based on years in Switzerland, age, and only two nationality indicators, but no split is based on gender confirming that the relative performance of each letter is the same for women and men. It is also noteworthy that no individuals were assigned to receive no letter, which suggests that at least one of the three letters has a positive effect for every individual.

3.6 | Results from the exploitation phase: Evaluating the policy rule

Table 3 shows the results of the evaluation based on estimating versions of (4) using ordinary least squares (OLS; see columns 1–3), PDS lasso (columns 4 and 5), and DDML (columns 6 and 7). The sample includes only wave 2 in column 1 and both waves in the remaining columns. The reference group in column 1 is random treatment allocation, while the base group in columns 2–7 is no treatment. Panel A reports the coefficient estimates, and Panel B compares the policy rule using policy trees against each individual treatment letter and random treatment allocation.

According to the OLS results in columns 1–3, the treatment assignment by policy tree increased the application rate by 1.79 (*s.e.* = 1.36) to 1.90 p.p. (1.36) relative to random treatment and by around 5.13 p.p. (1.61) compared with no treatment. Random allocation is associated with an application rate increase of approximately 3.23 p.p. (0.82). Turning to the individual treatments, we find that the *Welcome letter* yields overall the largest increase in application take-up with an effect size around 3.79 p.p. (1.07), closely followed by the *Requirements letter* with an effect size around 3.65 p.p. (1.10). The *Complexity letter* performs substantially worse in comparison, with an effect size of 2.23 (*s.e.* = 1.04). Panel B shows that the policy tree performs better than random treatment or each individual treatment option. The take-up increase compared with the best-performing individual treatment (the *Welcome letter*) is 1.03 p.p. but statistically insignificant. The PDS lasso estimates are almost identical, and the DDML estimator yields effect sizes only marginally smaller.[26]

[24]We note that in a setting where $e_a(X_i)$ is known, the IPW estimator of the reward in (1) is also applicable. We find in simulations that the AIPW estimator using the known propensity scores outperforms the IPW estimator.

[25]We assigned policies for Groups B.1 and B.2 after removing individuals who either applied without being treated (99 individuals) or moved out of the municipality of Zurich (101 individuals).

[26]Appendix Table A.5 in the supporting information also shows alternative results using logistic regression. The average marginal effects from logistic regression are almost identical to those from OLS.

TABLE 3 The effect of the policy rule compared to randomization, always the same treatment and no treatment.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: Naturalization application						
Panel A. Coefficient estimates							
Policy tree	1.794	5.124***	5.127***	5.004***	5.005***	4.702***	4.758***
	(1.358)	(1.609)	(1.609)	(1.606)	(1.606)	(1.472)	(1.479)
Random		3.225***		3.245***		3.207***	
		(0.821)		(0.822)		(0.752)	
Complexity			2.230**		2.260*		2.199**
			(1.035)		(1.037)		(0.938)
Requirements			3.650***		3.711***		3.613***
			(1.095)		(1.096)		(1.026)
Welcome			3.787***		3.752***		3.675***
			(1.074)		(1.071)		(0.986)
Panel B. Comparison of policy tree with:							
Random	1.794	1.899		1.759		1.495	
	(1.358)	(1.361)		(1.359)		(1.257)	
Complexity			2.897		2.745		2.559
			(1.539)		(1.538)		(1.432)
Requirements			1.477		1.294		1.144
			(1.503)		(1.499)		(1.409)
Welcome			1.340		1.253		1.083
			(1.528)		(1.527)		(1.415)
Sample	Wave 2	Waves 1 and 2	Waves 1 and 2	Waves 1 and 2	Waves 1 and 2	Waves 1 and 2	Waves 1 and 2
Estimator	OLS	OLS	OLS	PDS lasso	PDS lasso	DDML	DDML
Outcome mean	7.69	7.92	7.92	7.92	7.92	7.92	7.92
Observations	1717	6588	6588	6588	6588	6588	6588

Note: The table reports results from estimating versions of (4) using OLS (columns 1–3), PDS lasso (columns 4 and 5), and DDML (columns 6 and 7). Column 1 only uses data from wave 2; the remaining columns use the full data set. The reference group in column 1 is random treatment allocation; no treatment in columns 2–7. Panel A reports the coefficient estimates. Panel B compares the policy rule using policy trees against always assigning the same treatment to everyone and random treatment allocation. Covariates include the region of nationality, age, gender, years lived in Zurich, and years lived in Switzerland. Standard errors are clustered at building address level.Abbreviations: DDML, double-debiased machine learning; OLS, ordinary least squares; PDS, post-double selection lasso.

$^{*}p < 0.05$, $^{**}p < 0.01$, and $^{***}p < 0.001$.

4 | CONCLUSION

This paper employs policy trees for assigning eligible immigrants to the information and encouragement treatment that is most likely to address hurdles on their path to citizenship and boost their propensity to naturalize. We evaluate the benefits of this policy rule using a tailored two-phase field experiment. During the exploration phase, we randomly assign eligible immigrants to one of three treatment arms or the control group, based on which we estimate average treatment effects and train the policy tree. We find that despite its simplicity, the optimal policy tree of depth 3 captures more than 85% of the treatment effect heterogeneity (relative to a model-free plug-in rule). Next, we move on to the exploitation phase, in which we assign the subjects that belonged to the control group in the previous phase to either the policy tree or randomly to one of the three treatments. We find that the policy tree outperforms the best-performing individual treatment slightly. While these differences are not statistically significant, it is worth noting that these benefits persist in a context with at most moderate levels of treatment effect heterogeneity and come at little additional costs.

Policy trees possess several advantages that make them particularly suited for policymakers and researchers interested in tailoring treatment assignment to the specific needs of increasingly diverse populations. Policy trees are transparent in terms of which variables guide treatment assignment, they are simple to visualize, and intuitive to communicate even to users of the research who lack statistical training. While using machine learning to personalize treatment assignments raises a host of important ethical and policy questions, we should keep in mind that a one-size-fits-all approach can often exacerbate existing inequalities. For instance, an earlier information letter sent out by the City of Zurich had by far the strongest effects among newly eligible immigrants, which often score higher on multiple integration dimensions compared with more marginalized immigrants who have been residing in the host country for decades without naturalizing

(Ward et al., 2019). For all these reasons, we believe that policy trees are a well-suited approach to leverage the potential of tailored treatment assignment in a world where rich background characteristics are increasingly available.

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OPEN RESEARCH BADGES

This article has been awarded Open Data Badge for making publicly available the digitally-shareable data necessary to reproduce the reported results. Data is available at https://doi.org/10.15456/jae.2024212.1213209091.

DATA AVAILABILITY STATEMENT

The authors provide replication code through the Journal of Applied Econometrics Data Archive. The data are owned by the City of Zurich and the Canton of Zurich. It is not publicly available for confidentiality reasons. Data access requests should be directed to the City of Zurich's population office and the Canton of Zurich's municipal office.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of the article.

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