How Improvements to Public Service Delivery Shape Populist Right Support

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Abstract

Does improving public service delivery shape populist right appeal? While a body of research links the decline of public services to rising support for right-wing populism, we know considerably less about how improvements in service delivery shape populist right support. Leveraging spatial variation in the provision of high-speed internet across rural German municipalities between 2017 and 2021, I compare changes in electoral support for right-wing populists between neighboring municipalities that did and did not receive internet improvements. I document that populist right support increases in non-recipient municipalities, but only in local contexts where improvement inequality is high. Evidence from a subsample shows that exposure to unequal improvements accounts for this. These patterns are consistent with a story in which improvements and relative deprivation co-occur. Taken together, the findings reveal complications when improvements are selectively distributed, demonstrating how they can contribute to new forms of distributive conflict.

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1 Introduction

Spatial inequality has become a defining political fault line in many Western democracies (Rodríguez-Pose 2018). Long-term developments such as depopulation, austerity, and globalization left their marks on rural regions. While they disproportionally advantaged urbanized areas (Glaeser 2012), they provided the grounds for right-wing populist success in peripheral and economically struggling areas (Baccini and Sattler 2024; Bolet 2021; Fetzer 2019; Colantone and Stanig 2018). Existing research increasingly emphasizes the role of public goods provision in this development. This body of work has focused on two phenomena: first, how urban-rural inequalities in status quo service delivery increase populist right support (Patana 2022), decrease trust in government (Stroppe 2023), and relate to constituent demand (Wessel Tromborg and Schwindt-Bayer 2019); and second, how service decline, such as the closure of hospitals and schools or the discontinuation of administrative services in rural areas lead to increasing populist right support (Cremaschi et al. 2024; Nyholt 2024) and decreased turnout (Cox et al. 2025). Poor public service delivery can furthermore moderate the effects of economic shocks on populist right support (Cremaschi et al. 2025; Dickson et al. 2024).

Is the solution to improve the delivery of public services in rural areas then? Despite a fairly robust understanding of how *negative shocks* to public service provision shape populist right support, we lack a theoretical argument and an empirical test of the political effects of *positive* interventions in particularly rural areas which political scientists typically study in light of decline. Advancing the literature, I present a theoretical argument explaining why we should not understand positive shocks to public service delivery as the inverse of negative shocks and why the spatial distribution of

improvements is crucial to understand their influence on populist right support. I investigate this argument by focusing on the improved provision of high-speed internet in German municipalities. At first, the results appear to match the initial intuition: populist right support differs among neighboring recipient and non-recipient localities. However, I document and point to important complications that follow when improvements are geographically close but distributed highly unequal: in areas where a majority of localities receive improvements, support for right-wing populist follows patterns consistent with relative deprivation resulting from *lacking* improvement.

Starting from research on local context effects and local economic voting, I suggest that voters respond not only to absolute declines, but also to relative disadvantage when gains gather elsewhere, similar to observations made with regards to increased home values (Ansell et al. 2022) and perceived economic gains (Green et al. 2022). Departing and advancing this literature, however, I focus on the relevance of local place-based inequality rather than urban-rural and center-periphery divides. I argue that voters are especially attuned to local-level inequality, given that they identify with their local context (Borwein and Lucas 2023b) and rely on it to form perceptions of the national economy (Bisgaard et al. 2016) and their own status (Books and Prysby 1991, 74–75), but also to inform their vote choice (Green et al. 2025; Healy and Lenz 2017).

Therefore, I propose that the spatial distribution of improvements is crucial when we seek to understand their political effects. Improvements, like service declines, rarely affect all localities within a state and even within regions equally. When improvements occur in close-by localities they impose a relative disadvantage on improvement lacking localities. I argue that when improvements to public services are selectively distributed, they generate feelings of exclusion not because services have

deteriorated, but because improvements are provided elsewhere. In such cases, improvements create new forms of local-level inequality where citizens experience not an absolute decline, but a relative one. I further argue that improvements do not shape populist right support consistently across localities. Perceptions of relative deprivation are not an inevitable reaction to spatial inequalities but critically depend on exposure. When improvements to public service delivery are unequally distributed across localities, levels of exposure vary too. Absent a visible contrast, relative disadvantage may have no electoral consequence.

Taken together, this argument advances the literature on the political effects of public service delivery by recognizing improvements and relative deprivation coexist when improvements are unequally distributed. Understanding this coexistence helps to explain why the political effects of improvements are not the inverse of declines and when and how improvements can shape populist right support among different electorates.

To explore my theoretical argument, I examine the provision of high-speed internet in Germany. In 2017, only 63.5% of households in Germany had access to high-speed internet (>100 Mbit/s) (Eurostat 2025). High-speed internet provision was a contested policy issue, with one federal minister infamously dismissing the need for high-speed internet in rural areas, claiming that not every "milk can"[1] required this infrastructure (dpa 2019). Nonetheless, by 2021, high-speed coverage had reached 89.6% of households, a nearly 40% increase compared to 2017. During this time frame, neigh-

[1] "Milk can" metaphorically refers in this context to small, rural towns and villages. This phrase implies a lack of importance, suggesting that these localities do not need high-speed internet.

boring municipalities differ in whether they received internet speed improvements in many instances, offering a novel and politically relevant setting to study the political consequences of public service delivery improvements. I exploit this variation in a difference-in-differences design comparing support for Germany's populist right party, the Alternative für Deutschland (AfD), between neighboring recipient and non-recipient municipalities. Methodologically, this approach accounts for heterogeneity across localities while also providing leverage to identify the mechanism through which public service delivery improvements shape voter responses.

The analysis proceeds in three steps. First, I demonstrate that AfD support differs among non-recipient municipalities relative to recipients. This result could suggest that improved delivery undermines populist right support. However, standing on its own, the analysis cannot disentangle among which electorate support shifts. To address this, I proceed in a second step to examine the relevance of local improvement inequality by conditioning the baseline relationship on the ratio of recipient municipalities relative to non-recipients in a given local context. This tests whether the political effects of high-speed internet delivery depend on overall extent of local inequality and whether relative deprivation among non-recipients drives the results. Supporting my theoretical argument, I find that AfD support increases in non-recipient municipalities only in areas where the majority of municipalities gained high-speed internet improvements. Thus, populist right support shifts among non-recipients as a consequence of being relatively deprived. Finally, I further probe this mechanism and compare non-recipient municipalities to other municipalities that both did not receive improvements and are not geographically close to recipients. As these municipalities only differ in their exposure to improvements, this approach provides additional

leverage on the effect of exposure to improvement inequality. The results follow the initial analysis: AfD support increases in exposed non-recipient municipalities relative to non-exposed municipalities, reinforcing the relevance of exposure to nearby improvements.

Overall, the empirical results contribute to broader debates on the political effects of place-based inequality and local context conditions. The findings contribute to a growing literature on public service delivery, affirming its relevance as a local condition for populist right support. While existing work focused on how deteriorating provision increases populist right support, the findings in this article document that improvements can carry their own complications. Notably, not only do absolute declines shape support for right-wing populists, but also when improvements are distributed highly unequal across localities. When gains are concentrated, they can generate spillovers by imposing relative disadvantages that structure voting patterns consistent with relative deprivation among non-recipients. Relatedly, recent work on housing policy promotes the idea that the effects of place-based policies extend beyond immediate recipients (Hankinson et al. 2025). This has implications for how we understand and study public service delivery. Acknowledging not only *whether* a public good is provided, but also contextualizing its delivery in place will add to a more nuanced understanding and anticipation of different electorate reactions.

2 Theoretical framework

2.1 Why public services shape political behavior

One possibility for citizens to evaluate the performance of their government is to consider their local environment (Ansolabehere et al. 2014; Books and Prysby 1991). Local economic conditions, for example, not only offer insights into the broader economic situation but also serve as a perception of how well the government is performing (Bisgaard et al. 2016; Larsen et al. 2019). Yet, the local environment provides more than just economic signals. It is also the point where citizens and the state interact most directly, particularly through the provision and consumption of public services.

Public services have long been understood as a key indicator of government performance, responsiveness, and priority. The provision of public goods is not simply a technical task of administration. It represents a core element of an implicit contract between citizens and the state (Besley 2020). This contract rests on a basic exchange: citizens accept the obligation to pay taxes in return for essential services delivered by the state. As such, public service provision offers citizens a concrete means of evaluating how well their tax contribution are being used and provides a basis for holding policymakers accountable. Research on government performance and retrospective voting supports this idea, showing that public service quality influences support for incumbents (Burnett and Kogan 2017; Boyne et al. 2009), reflecting an ongoing process of policy delivery and feedback (Wlezien 1995).

Beyond their role in signaling government performance, public services also influence the attractiveness of localities, carrying both symbolic and economic importance in addition to their immediate functional use (Fischel 2005; Tiebout 1956). In this way,

public services delivery is tied to a locality's economic vitality. Given the geographically uneven impact of economic shocks (Baccini and Weymouth 2021; Colantone and Stanig 2018), public service decline is also geographically uneven (Feler and Senses 2017). The resulting political consequences of the link between deteriorating public services and political behavior have become object of scholarly interest. Populist support in both the US and Europe has been found to be strongest in areas that have experienced sustained economic and social decline over time (Patana 2022; Broz et al. 2021). In these contexts, inadequate service delivery can put strain on the citizen-state relationship. When citizens perceive the state as failing to uphold its obligations, dissatisfaction and resentment may emerge. Citizens then turn away from the thought of the state as caretaker and, as a result, demands for less state intervention are phrased (Cramer 2016). Indeed, citizens living in areas with poor public service provision tend to show lower levels of trust in political institutions (Stroppe 2023). Consequently, what scholars observe is a reshaped electoral landscape alongside the lines of geographic inequalities in public service delivery (Dickson et al. 2024; Cremaschi et al. 2024; Nyholt 2024).

This raises a critical question: can the new provision of public services strengthen the citizen-state relationship? If the citizen-state contract is met, we should expect a positive link between satisfaction with public services and citizens' trust. Existing studies point into this direction: satisfaction with service delivery is connected to citizens' well-being (Whiteley et al. 2010) and fosters trust in political institutions (Christensen 2005). On the electoral side, however, evidence from *positive* service interventions remains mixed. In areas where new public services, such as health care facilities or highways, are introduced, voters may reward the incumbents (Adiguzel et al. 2023;

Akbulut-Yuksel et al. 2023; Harding 2015), although this effect is not consistently observed (Goyal 2024; Imai et al. 2020). Notably, evidence from these studies stems from middle-income countries, where democratic accountability may be weaker or operate differently. Other research finds that increased public spending can strengthen electoral support for left-leaning parties (Hager and Hilbig 2024). However, in general, voters appear to respond less strong for improvements of personal welfare than for its losses (Larsen 2021).

2.2 Why the distribution of improvements to service delivery matters

Do improvements to public service delivery reduce citizen grievances and undermine support for right-wing populist parties, parties that mobilize around narratives of state failure, neglect, and marginalization? To answer this question we need to consider the role of local context, given the highly localized nature of public service delivery and consumption. As with the decline of public service delivery, improvements to it are rarely introduced everywhere at the same time. Schools, doctor's or postal offices shut down not state-wide at once, but one after the other, first in one area, then in another. We can imagine the same situation with improvements to service delivery: the introduction of new train and bus stations, the construction of public pools, or road maintenances will not happen overnight statewide. These processes involve complex planning which take time to turn into action. Changes to service delivery and their variation across time and space, thus, make the role of local context consequential and call for a geotropic lens when scholars wish to study the improvement of public service delivery (Reeves and Gimpel 2012).

Where personal welfare is unequal, feelings of relative disadvantage can emerge. Relative deprivation theory provides a compelling framework for understanding this phenomenon. At its core, the theory proposes that individuals assess their well-being not in isolation, but through mental comparisons with others (Smith et al. 2012; Crosby 1976). These comparisons can generate feelings of injustice and frustration when individuals believe they are worse off than comparable others. Over time, these perceptions of unfair treatment may fuel dissatisfaction and shape both political attitudes and behavior (Kim and Hall 2024). I argue that the uneven distribution of public service improvements may generate feelings of exclusion, not because services have deteriorated, but because improvements are provided elsewhere. In such cases, citizens experience not an *absolute* decline, but a *relative* one. And while the political effects of *absolute* decline are well documented, we know far less about how positive interventions in some areas, alongside stagnation in others, shape populist right support.

Citizens adjust their policy preferences and political demands based on information about the supply of state services (Wlezien 1995). This demand can arise from a constituents' relative deprivation of resources (Wessel Tromborg and Schwindt-Bayer 2019). In voter districts with lower levels of public and private resources, feelings of relative deprivation arise because citizens compare their locality to others with more resources and conclude they receive less than they deserve. Existing research documents similar patterns across different dimensions of inequality. Feelings of relative deprivation may stem from one's position in the income distribution (Burgoon et al. 2019) or broader social standing (Gest et al. 2018). Residents in disadvantaged localities feel overlooked by policymakers, and this perceived relative deprivation is associated with increased support for right-wing populist parties (Green et al. 2022). Crucially,

their feelings of relative deprivation are attributed to the perceived failures of mainstream political actors to represent their interests adequately (Elchardus and Spruyt 2012).

Building on this, I argue that improvements to public service delivery shape support for right-wing populists when they are distributed unequally across space. In localities that do not receive improvements, the absence of action may not be perceived as neutral, but as a disadvantage that can cause its residents to feel relatively deprived. The absence of improvement may produce grievances taken as evidence of political neglect and strengthen support for populist right parties among those who feel disadvantaged. This expectation is summarized by:

> H_1: Relative disadvantage in public service delivery increases support for the populist right.

This hypothesis rests on two elements: a question and an assumption. The question is relative to whom voters perceive themselves to be disadvantaged. The assumption is that voters must be aware of their disadvantage for it to influence their vote choice. While this assumption is often implicitly made in existing work on public service decline, it is far from trivial and deserves further theorization.

I argue that this awareness depends on exposure. Without exposure to relevant information, even voters in objectively disadvantaged localities may not interpret their condition as deprived. Thus, the exposure mechanism I put forward is simple but critical: for citizens to perceive themselves as relatively deprived, they must be exposed to others who are better off. This exposure occurs primarily through citizens' daily interactions with their local context. Local contexts are the primary settings of daily life where individuals encounter cues about public service provision, whether through

direct personal experiences, interpersonal exchange, or local media (Ansolabehere et al. 2014; Reeves and Gimpel 2012). Exposure to specific kinds of information are thus inherently shaped by a citizen's local context (Larsen et al. 2019; Bisgaard et al. 2016; Books and Prysby 1991, 77). Because exposure opportunities are geographically embedded, the answer to "relative to whom" is, in practice, a function of where one lives.

What, then, determines the extent of exposure to unequal improvements of public service delivery? Most fundamentally: the spatial scope and concentration of improvement inequality. Because improvements to public service delivery are unequally distributed across localities, the exposure opportunities vary too. Consider locality A where service delivery is not improved. What matters is the extent to which surrounding localities gain better service delivery. If locality A is mostly surrounded by localities with similar service delivery there is little contrast and feelings of disadvantage are less likely to emerge. That changes with the growing proportion of improved to disadvantaged localities. If locality A is embedded in an area where many localities have received service delivery improvements, the absence of provision becomes harder to ignore.

Local contexts, thus, structure to whom citizens compare to and therefore which information they encounter. In this way, they shape how citizens interpret local conditions and state action. Absent a visible contrast, relative disadvantage with regards to service delivery may have no electoral consequence. These differences in exposure are central to my argument: where relative disadvantage is both present and visible support for the populist right increases. This expectation is summarized by:

H_2: Relative disadvantage in public service delivery increases support for

the populist right *only* in contexts where provision inequality is high.

Alternatively, political reactions to delivery improvements may reflect a delivery effect. Under a simple delivery-based explanation, voters respond primarily to changes in their own locality. This is because voters respond to improvements in their own locality, not to whether other localities (do not) receive improvements. In this case, I would expect the opposite relationship of the one laid out in the first hypothesis. In addition to that, improvements to delivery may shape populist right support through a relative prioritization effect. When improvements are rare overall, recipients may interpret their selection as evidence that policymakers prioritize their locality. Under this logic, I would expect the reverse of the second hypothesis: improvements decrease support for the populist right primarily in context where provision inequality is low.

3 Research design and empricial setting

I examine the provision of high-speed internet in German municipalities between 2018 and 2021 to analyze how improvements in public service delivery influence support for the populist right. During this period, the share of households with high-speed internet access rose from 63.5% to 89.6%, an increase of nearly 40 percent compared to 2017. The timing of the marked expansion aligns closely with the 2017–2021 national legislative term, offering a well-aligned and politically relevant time frame for the analysis.

Improvements to internet infrastructure qualify particularly well for this purpose as they are highly localized. In practice, neighboring municipalities differ in whether they received internet improvements during the studied period. This variation creates

the possibility of a systematic comparison of both recipient and non-recipient municipalities to explore the political effects of locally targeted public service improvements. I leverage these disparities, using a difference-in-differences design that focuses explicitly on neighboring municipalities. The identification strategy relies on parallel trends in AfD vote shares prior to high-speed internet improvements.

Beyond being an empirically relevant case, the improvement of internet infrastructure additionally speaks well to my theoretical argument. A key advantage of this case lies in the near-binary logic of the intervention: municipalities either gained 100 mbit/s coverage for a majority of households or they did not. This stands in contrast to other types of public service changes, such as hospital closures, where the treatment rather approximates a continuous nature as impact may vary depending on how close a voter lives to the affected facility. Focusing on the spatial distribution of improvements also supports the idea that populist right support can be shaped not only by absolute service decline, but also by relative disadvantage when the unequal distribution of improvements clearly benefits only some.

Given this context my theoretical argument predicts increases in AfD support in non-recipient municipalities, particularly when their relative disadvantage is noticeably pronounced. By observing the same municipalities before and after the delivery improvement, and comparing them to neighboring counterparts, the difference-in-differences design offers a strong empirical basis for the causal study of whether delivery improvement create new dividing lines between recipients and non-recipient that shape support for the populist right. The following section lays out the research design and introduces the case in greater detail.

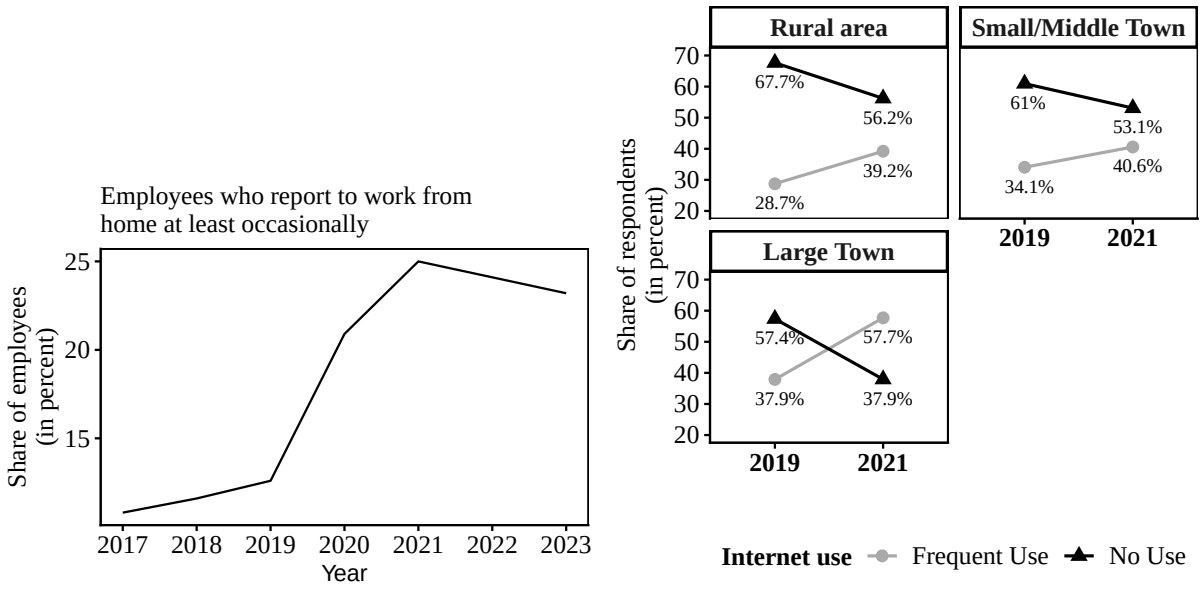
3.1 Why high-speed internet is a salient public good in Germany

The provision of high-speed internet in German municipalities between 2018 and 2021 provides a valuable case study, characterized by its unique combination of being a positive intervention to public service delivery within a relevant policy context.

First, it demonstrates a *positive* policy intervention, serving as a rare instance of infrastructure improvement in less urbanized areas, providing a valuable empirical setting to assess whether improvements in service provision can mitigate the electoral realignments observed in many advanced democracies. Moreover, the observed delivery of internet infrastructure overlaps with the COVID-19 pandemic, a period that highlighted the importance of digital infrastructure. As high-speed internet became a critical resource for participation in education and work life, particularly as homeschooling and remote work became widespread during the repeated lockdowns (Feldmann et al. 2021), high-speed internet access became not only a convenience but a critical component of full societal participation. I illustrate this point further based on data on the prevalence of working from home provided by Eurostat (2025).

Figure 1 Panel (a) shows a sharp increase in the share of employed individuals in Germany working from home during the pandemic, which reflects an almost twofold rise in remote work and underscores the abrupt change in work practices. And although remote work remains concentrated in urban labor markets, given the sectoral and occupational structure of cities, Figure 1 Panel (b) suggests a geographically widespread demand for internet infrastructure (data come from the Eurobarometer waves 91.1 (European Commission And European Parliament 2025) and 95.2 (European Commission 2022)). Respondents across different types of municipalities increasingly report to frequently use the internet at their workplace, including rural areas and small- to

medium-sized towns.



(a) (b)

Figure 1: Panel (a) is a visulaization of the percent share of employed people who reportedly work from home. Panel (b) shows self-reported workplace internet use by region type. High frequent users are respondents who reportedly have a daily to multiple times a week workplace internet use.

Second, the delivery of high-speed internet in urban and rural areas is understood as a public service in Germany that citizens are legally entitled to (Bundesnetzagentur 2021). The federal government regularly releases official reports on the development and support of rural areas in Germany, reflecting the constitutional commitment to equivalent standards of living (*Gleichwertige Lebensverhältnisse*, Article 72(2) of the German Basic Law). In a 2019 interim report, for instance, the development of a comprehensive and high performing internet infrastructure is discussed as central to ensure that all people, regions, and businesses in Germany can benefit equally from digitization (Bundesregierung 2021). These policy goals are not confined to political elites as major media outlets disseminate them to the broader public (cf. Kornmeier 2024). From a voter perspective, these commitments can serve as a normative foundation for expectations, evaluations, and political behavior.

3.2 The provision of high-speed internet

3.2.1 How internet infrastructure provision works in Germany

The provision of the internet infrastructure at the municipal-level depends on various actors, such as state, district, and municipal administration or private internet companies. Municipality characteristics, such fiscal capacity, and the number of potential high-speed internet subscribers (population size), are most likely key determinants of priority for the internet speed to be upgraded. In general, since its introduction to the general public, internet access underwent a period of diffusion, with primarily urban areas gaining internet access at first due to the lower cost of expansion in these higher-density areas (Wernick and Bender 2016). Mid-2018, only half of the rural regions had access to broadband ≥ 50 Mpbs (Rottmann and Hilbing 2019, 6). One hurdle that delayed broadband expansion is the lack of funding available to municipalities (Rottmann and Hilbing 2019). To advance the expansion in rural regions, where private companies are hesitant due to high investment costs against lower return perspectives, the Federal Government launched the *Gigabit Strategy*, which aims to provide all households in Germany with access to broadband (≥ 100 Mpbs) by 2030 (Bundesregierung 2022, 14). In order to achieve this, the strategy provides substantial funding options for areas where "roll-out is not economically efficient and a failure of the market is observed" (Federal Ministry for Digital and Transport 2021).

I derive data on the deployment of internet infrastructure from the Federal Network Agency's Broadband Atlas (Bundesnetzagentur 2023) which tracks the roll-out of high-speed internet in Germany. The data show what percentage of the population has technical access to different internet speed levels (≥ 16, ≥ 30, ≥ 50, ≥ 100, ≥ 200,

and $\geq$ 1000 Mbps). Unfortunately, granular data only dates back to the end of 2018 which decreases the amount of observable internet interventions within the federal electoral period from 2017 to 2021. Given the data structure, I focus on 100 Mbps as the threshold for high-speed internet. This choice aligns with broader policy benchmarks and practical usage standards. For example, the EU 2025 Connectivity objectives aim at EU-wide coverage of 100 Mbps by 2025 (European Commission 2019).

3.2.2 Geographic variation in high-speed internet accessibility

I define high-speed internet improvement based on the internet speed reported for the majority of households in each municipality. I begin by selecting all municipalities in Germany where the majority of households had internet speeds below 100 Mbps in 2018. Among these, I classify a municipality as an improved unit if the majority of households had speeds of at least 100 Mbps in 2021, and as an unimproved unit otherwise.

To operationalize exposure to unequal improvements and to identify the treatment and control groups, I classify an improved unit as a recipient (treated) municipality when it shares a geographic border with at least one unimproved unit. Conversely, unimproved units are classified as non-recipient (untreated) municipalities when they share a geographic border with at least one recipient municipality. Figure 2 illustrates examples of recipient and non-recipient municipalities.

In total, I identify 3,510 municipalities that meet these conditions (1,972 recipient and 1,538 non-recipient municipalities). Table 1 reports summary statistics on population size by treatment status. As shown, the median population size is 1,146 in recipient municipalities and 742 in non-recipient municipalities.

Table 1: Population sizes by treatment condition

Condition	Count	Mean	Median	Min	Max
Recipient	1,972	2,326	1,146	18	25,830
Non-recipient	1,538	1,565	742	22	125,596

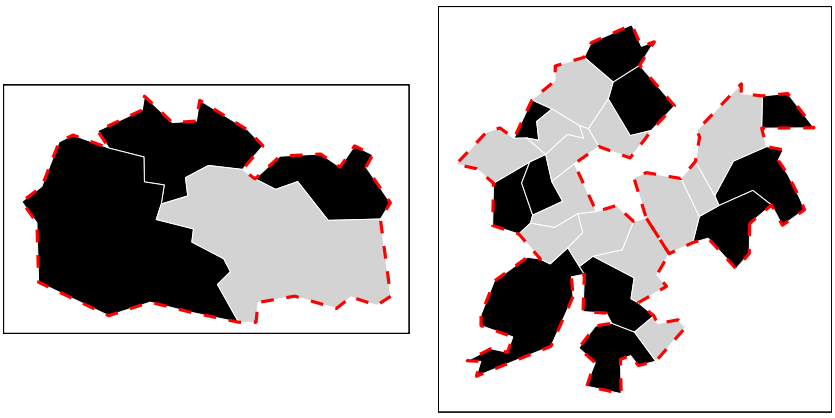
Improvements of internet infrastructure mostly took place in less urban regions during the studied time frame. Notably, these regions are typically studied through the lens of decline, whether with regards to economic deterioration or the retrenchment of public services. The observed pattern thus offers a different perspective, allowing me to investigate instances of positive interventions in these regions.

3.2.3 Measuring exposure and relative deprivation

I argue that feelings of deprivation emerge when voters are exposed to improvement inequality. This exposure occurs as "by-product" of everyday life (Ansolabehere et al. 2014, 384), possibly through contact with proximate municipalities. To operationalize exposure and the extent of local improvement inequality, I develop a clustering algorithm that groups spatially neighboring municipalities based on their treatment status. The algorithm works as follows: it begins with recipient municipalities and connects them to a geographically adjacent non-recipient municipalities, creating a cluster. This cluster continues to expand by adding any non-recipient municipalities that directly border at least one recipient municipality already in the cluster. The cluster stops grouping municipalities when no new non-recipient municipalities share a boundary with recipient municipalities in that cluster. This process creates distinct geographic clusters, each containing a mix of recipient and non-recipient municipalities where non-recipients directly observe improvements in neighboring communi-

ties. In other words, municipalities belong to the same cluster as long as there are direct recipient-non-recipient geographic neighbors.

Figure 2 illustrates how the clustering works. Panel (a) shows a single cluster: every recipient municipality (black) shares a border with one non-recipient municipality (grey). Panel (b) demonstrates when clusters separate: here, two non-recipient municipalities border each other without a connecting recipient municipality between them, resulting in two different clusters. Figure A.1 in Appendix A shows the full map of all identified recipient and non-recipient municipalities.



(a) (b)

Figure 2: Both panels show recipient municipalities (black) and non-recipient municipalities (grey) within municipality cluster boundaries (red dashed lines). Panel (a) displays a cluster where 3 in 4 municipalities are recipients of high-speed internet improvements. Panel (b) shows two separate clusters: clusters end where two non-recipient municipalities border each other without a connecting recipient municipality.

Within each cluster, I calculate the share of recipient municipalities relative to non-recipient municipalities. In this way, I estimate the moderating role of exposure to improvement inequality. Theoretically, I expect that feelings of relative deprivation will be stronger for residents of non-recipient municipalities in contexts where a higher share of municipalities receive high-speed internet. The higher the share of recipient municipalities, the more exposure to improvement inequality there is potentially.

Turning to Figure 2 again, I illustrate this thought. The cluster in panel (a) has a recipient share of 75%: three out of four municipalities received improvements. The left cluster in panel (b) has a recipient share of 44%, while the right cluster has a recipient share of 50%. Thus, a non-recipient municipality in panel (a) are exposed to a higher level of improvement inequality than non-recipients in panel (b) clusters.

Distinguishing between recipient and non-recipient municipalities offers both methodological and substantive advantages. Methodologically, it reduces heterogeneity between groups, improving their comparability. Substantively, it reveals how voters respond to positive interventions in public service delivery. Importantly, the recipient share opens up the possibility to test the exposure mechanism more directly. Therefore, this design provides new leverage on whether public service delivery shapes political behavior through perceptions of relative disadvantage or relative prioritization, more in line with a delivery-based effect.

3.3 Outcome: Municipal-level AfD support

I use GERDA data (Heddesheimer et al. 2025) to measure my outcome variable populist right voting. I use the municipal-level share of valid votes of the by far biggest right-wing populist party in Germany, the Alternative für Deutschland (AfD), in the federal elections in 2013, 2017, and 2021. A challenge is that German municipalities can share joint mail-in voting districts, making it impossible in some cases to assign mail votes unambiguously to specific municipalities. Since my identification strategy depends on measuring exposure to improvement inequality at the municipal level, precise assignment of votes is crucial. For this reason, I exclude votes from joint mail-in voting districts to support consistency with my measurement of local context effects.

3.4 Municipality characteristics

I collect variables on municipalities' socioeconomic conditions to conduct balance checks to assess pre-treatment differences between recipient and non-recipient municipalities (see Table B.1 in Appendix B for descriptives on the covariates) and Table C.1 in Appendix C for the pre-treatment balance checks). I draw on multiple data sources for these municipal-level characteristics. The INKAR database provides data on population size, density, and trends, as well as unemployment rates, disposable income, and fiscal capacity (Bundesinstitut für Bau-, Stadt- und Raumforschung 2023). The German Statistical Office supplies data on the foreign-born population share. House price index data come from the Leibniz Institute for Economic Research (2022).

3.5 Estimation strategy

I rely on a difference-in-differences design to estimate the differential change in populist right voting over time between high-speed internet recipient and non-recipient municipalities. The key identifying assumption is that, in the absence of high-speed internet improvement, recipient and non-recipient municipalities would have followed parallel trends in support for the AfD. To assess the plausibility of this assumption, I rely on the standard approach of examining pre-treatment trends in the outcome variable[2] (see Appendix D Table D.1 and Figure D.1 for reported

[2]Note, however, data on the AfD only dates back to 2013, the founding year of the party, limiting the number of possible pre-treatment periods. I therefore use a combined radical right vote share in the years before to cover more periods following Heddesheimer et al. (2024). The number of pre-treatment observations in this

parallel trends results). The null hypothesis is that pre treatment trends in recipient and nonrecipient municipalities do not differ and is rejected when the p value is smaller than 0.05. This threshold is consistent with contemporary political science research studying the political effects of public service decline (Cremaschi et al. 2024; Nyholt 2024; Dickson et al. 2024) and aligns with general recommendations for difference-in-difference designs (Angrist and Pischke 2009). I find no evidence of differential pre trends as the p values exceed 0.05 across all pre treatment periods.

To test the first hypothesis, I analyze the differential change in AfD support from 2017 to 2021 in non-recipient municipalities relative to recipient municipalities. Formally, I estimate the following model:

$$\text{AfD}_{mt} = \alpha_m + \alpha_{te} + \beta_1 D_{mt} + \epsilon_{mt} \tag{1}$$

In Equation (1), AfD_{mt} denotes the AfD vote share in municipality m at election t, α_m are municipality fixed effects and α_{te} are election year $\times$ east-west fixed effects, D_{mt} indicates whether a municipality m has high-speed internet infrastructure in election year t and equals 1 if it did *not*. The coefficient of interest is the β_1, which captures the average differential change in AfD support in non-recipient municipalities relative to recipient municipalities.

Testing the second hypothesis, I analyze whether this effect is conditional on local context, specifically, the prevalence of recipient municipalities. The formal model

study is, moreover, comparable to the number of pre-reform observations reported in Cremaschi et al. (2024).

reads:

$$\text{AfD}_{mt} = \alpha_m + \alpha_{te} + \beta_1 \text{D}_{mt} + \beta_2 \text{D}_{mt} \times \text{RecipientShare}_{C(m)t=2021} + \epsilon_{mt} \quad (2)$$

$\text{RecipientShare}_{C(m)t}$ is the share of municipalities that have high-speed internet infrastructure in election year $t = 2021$ in a local municipality cluster C of municipality m. The key coefficient of interest is β_2 and reflects the conditional effect of D as a function of local inequality in internet infrastructure provision. Thus, β_1 and β_2 together show how the effect of being in a non-recipient municipality varies depending on exposure to surrounding recipient municipalities.

Both models are estimated using ordinary least squares. Following Abadie et al. (2022), I cluster standard errors at the municipal-level for the model in equation 1 and at the municipality cluster level for the model in equation 2.

4 Results

4.1 Unequal delivery of high-speed internet improvements

How do unequal improvements of internet infrastructure shape AfD support? I begin the analysis by simply testing whether unequal high-speed internet improvements among neighboring municipalities lead to differences in AfD support. Figure 3 shows the estimated difference-in-differences coefficients (see Appendix E Table E.1 for full model results). The estimated coefficients show the difference in the AfD vote share between recipient and non-recipient municipalities from 2017 to 2021. Model (1) in-

cludes municipalities from Whole Germany, to which I add an interaction fixed effect of an East-West dummy and election year to account for time-varying effects between East and West Germany, following Ang (2019, 13). Models (2) and (3) show the estimated coefficients separately for Western and Eastern Germany, where the East-West-year fixed effect is replaced with a year fixed effect. All three models include municipality fixed effects, which control for unobserved unit-varying effects on AfD vote share at the municipal-level.

According to Model (1), the estimated effect is 0.524 [CI: 0.257; 0.791, p = 0.000], implying that in non-recipient municipalities which did not receive high-speed internet infrastructure, the AfD vote share increased by 0.45 percentage points between 2017 and 2021 relative to neighboring recipient municipalities. The separate models for (2) Western and (3) Eastern Germany reveal a similar pattern. The effect sizes are similar to Model (1), with a lower effect in Western Germany [0.460, CI: 0.200; 0.721, p = 0.001] than in Eastern Germany [0.697, CI: 0.003; 1.389, p = 0.049].

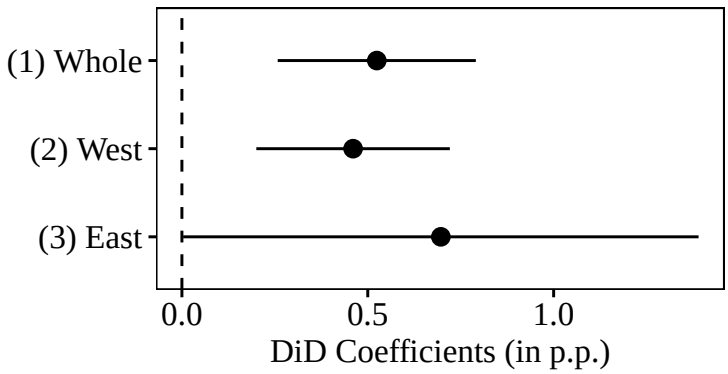


Figure 3: The figure shows the estimated difference-in-differences coefficients for (1) Whole Germany, (2) Western Germany, and Eastern Germany (3). Estimates are shown in percentage points. The dependent variable is municipal-level AfD vote share. The baseline group are recipient municipalities. Whiskers represent 95% confidence intervals.

This contrasts with early work on basic internet provision, which found no effect on party support (Falck et al. 2014). This difference likely reflects that high-speed internet

has become a more salient public good, and that the research designs capture different mechanisms: earlier work focused on the effect of internet on political information and media consumption rather than studying the unequal delivery of high-speed internet and relative deprivation (Schaub and Morisi 2020).

How do the results compare to other instances of changes to public service delivery? The magnitude of the estimated effect may appear modest in absolute terms. Compared to recent studies examining the deterioration of public service delivery, the effect is indeed somewhat smaller. For example, Cremaschi et al. (2024) find that municipal reforms which reduced access to public services were associated with an increase of 1.5 percentage points in far-right support in the following national elections. Nyholt (2024) observes a 1.1 to 1.4 percentage points increase in support for the populist right in the second election following a local school or hospital closure. These disruptions, of course, represent far-reaching forms of public service deterioration but even against this backdrop, the observed effect in this study remains notable. Despite the absence of absolute deterioration, voters appear responsive to relative conditions in their local context suggesting that perceptions of unequal improvement can be politically meaningful in their own right.

4.2 Exposure to improvement inequality is key

Thus far, I have examined how, on average, the unequal improvement of internet infrastructure in neighboring municipalities lead to differences in AfD support. Being geographically close to recipient municipalities appears to increase AfD support, following patterns that could match with relative deprivation. However, the preceding analysis is unable to disentangle whether this effect works through exposure to im-

provements that make voters aware of their relative disadvantage. It remains an option that the observed pattern results from a delivery-based explanation. Both explanations rely on exposure, because only when voters observe differences are they likely to perceive themselves as disadvantaged or advantaged.

Moving on to the main analysis, I probe the relative deprivation mechanism and test the role of exposure, measured as the proportion of non-recipient municipalities relative to recipient-municipalities in a local context. The logic is straightforward: if delivery improvements shape populist right support through relative deprivation, then relative disadvantage should be most salient when a non-recipient municipality is embedded in a local environment where improvements are widespread.

Table 2 reports the results of this formal test. The quantity of interest is the interaction term *Non-recipient* $\times$ *Recipient share*, which captures how the non-delivery effect varies across local environments. *Recipient share* is measured on a continuous scale. In Model (1), the estimated effect is 2.815 [CI: 0.343; 5.286, p = 0.026], indicating that non-recipient municipalities embedded in contexts with a higher share of recipient municipalities show stronger increases in AfD support. Models (2) and (3) again show similar patterns, with a slightly stronger effect in Western Germany [2.958, CI: 0.622; 5.293, p = 0.013] and weaker effect in Eastern Germany [2.233, CI: -5.445; 9.912, p = 0.562]. However, the estimated effect in Model (3) is not statistically significant at the conventional levels.

Figure 4 presents the marginal effects based on Model (1), estimated with the *Interflex* package in R (Hainmueller et al. 2019). The figure shows changes in AfD support in non-recipient municipalities across varying levels of recipient municipalities within a municipality cluster. As a reminder, a municipality cluster refers to a geo-

Table 2: Exposure to internet improvements and AfD vote share

Region:	Whole	West	East
Model:	(1)	(2)	(3)
Variables			
Non-recipient	-1.133	-1.234*	-0.715
	(0.668)	(0.579)	(2.292)
Non-recipient × Recipient share	2.815*	2.958*	2.234
	(1.255)	(1.184)	(3.830)
Fixed-effects			
Municipality	Yes	Yes	Yes
East-Year	Yes		
Year		Yes	Yes
Fit statistics			
Observations	10,362	7,509	2,853
R^2	0.897	0.772	0.897
Within R^2	0.004	0.005	0.003

Clustered (Municipality Cluster) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05*

graphic area composed of all neighboring recipient and non-recipient municipalities. This clustering approach is used to measure the degree of exposure that non-recipient municipalities have to nearby improvements in internet infrastructure. In brief, a recipient share value of 0.5 indicates that the cluster is evenly composed of recipient and non-recipient municipalities.

The figure reveals that when non-recipient municipalities are located in contexts with a low share of recipients, there is no observable difference in AfD support. However, once recipient municipalities become locally dominant, non-recipient municipalities increasingly support the AfD. In line with relative deprivation arguments, political dissatisfaction can emerge from a perceived inequality in public service delivery. This perception appears to evolve with rising exposure. For example, the marginal effects indicate that if three in four municipalities in a cluster have received internet

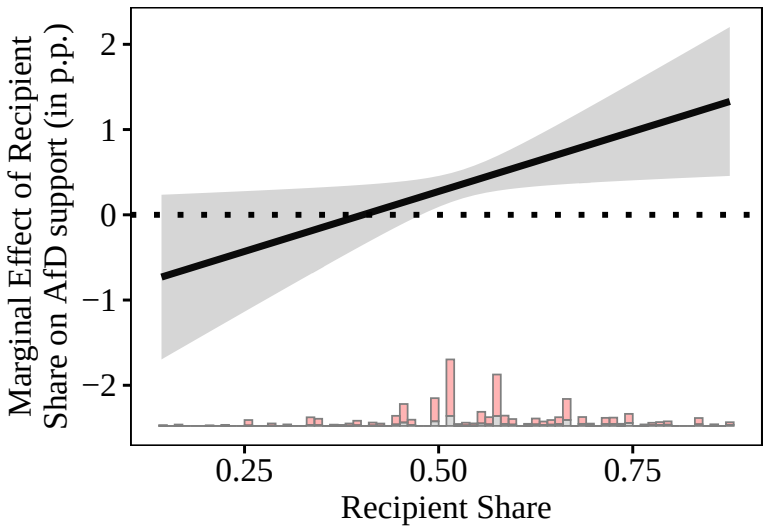


Figure 4: Marginal Effects of recipient share on AfD support in non-recipient municipalities. Shading represents 95% confidence intervals.

improvements, AfD support in the non-recipient municipalities increases by approximately 0.98 percentage points. Given that high-speed internet provision is a narrow and specific measure, while support for right-wing populists reflects a broad political outcome, even modest effect sizes point to the substantive relevance of public services for voting behavior.

What do these findings tell us about the role of exposure with regards to the distribution of public service improvements? How do they square with a relative deprivation and a delivery-based explanation? If exposure and *relative* conditions were irrelevant, the local share of recipient municipalities should have no effect on AfD support. Instead, the findings support a a relative deprivation explanation. The interaction effect reveals a nuanced dynamic: improvements in internet infrastructure benefit the populist right under certain conditions. Specifically, when voters are highly exposed to surrounding improvements leading them to notice their relative disadvantage. By contrast, if the results were driven by a delivery effect, I would anticipate changes in AfD support irrespective of the recipient share in a local context.

A delivery-based mechanism depends less on exposure to improvement inequality because the critical factor would be the improvement itself. Alternatively, the exposure logic could operate through a relative prioritization effect. Under this logic, improvements should shape populist right support in recipient municipalities in local environments with many non-recipient municipalities as improvements should be most salient then. However, the data do not support either of these explanations.

Next, I further probe the exposure mechanism to differentiate between a relative deprivation and a delivery effect. To do so, I define a new control group. I define non-recipient municipalities as being *exposed* to high-speed internet improvements by geographically neighboring a recipient municipality. By contrast, the new control municipalities, which I refer to as *not exposed non-recipient*, did not receive internet improvements and do not geographically neighbor any recipient municipalities. I focus on exposed non-recipient municipalities that neighbor at least one not exposed non-recipient municipality. A not exposed non-recipient municipality can but does not have to neighbor an exposed non-recipient municipality. This yields a balanced sample of 335 exposed non-recipient and 335 not exposed non-recipient municipalities (see Figure F.1 in Appendix F for a full map).

Figure 5 illustrates this. Key to this operationalization is that direct geographic boundaries are avoided between the not exposed non-recipients (grey) and recipient municipalities (striped).

Because not exposed non-recipients neither receive internet improvements nor geographically neighbor municipalities that do, I argue that voters are not aware of their relative disadvantage compared to exposed non-recipients. Perceptions of relative deprivation are therefore less likely to emerge in not exposed non-recipient municipal-

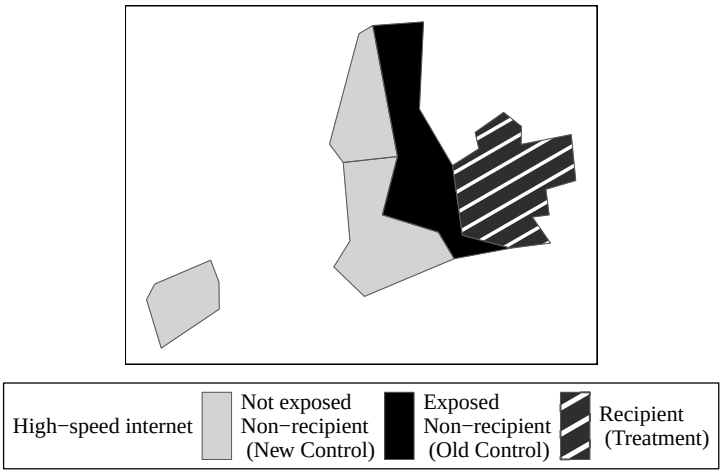


Figure 5: The figure visualizes the spatial relationship between not exposed non-recipient (grey), exposed non-recipient (black), and recipient (striped) municipalities.

ities. Comparing not exposed non-recipient to exposed non-recipient municipalities creates additional leverage to testing the mechanism: following a relative deprivation explanation I expect an increase in AfD support in exposed non-recipient municipalities relative to not-exposed non-recipients. For delivery effect explanation I anticipate no difference in AfD support between exposed and not exposed non-recipients as both do not experience improved internet speed.

Figure 6 shows the estimated difference-in-differences coefficients (see Appendix F, Table F.1 for parallel trends test and Table F.2 for full model results).

The estimated coefficient in Model (1) is 1.011 [CI: 0.361; 1.660, p = 0.002], suggesting that AfD support increases in exposed non-recipient municipalities relative to not exposed non-recipients. The results are similar for Western Germany [1.022, CI: 0.351; 1.692, p = 0.003], whereas the estimates for Eastern Germany [1.022, CI: -1.411; 3.263, p = 0.432] do not meet the standard level of statistical confidence. Taken together, these findings are consistent with a relative deprivation mechanism.

Crucially, this pattern is inconsistent with a delivery effect. Under a delivery logic,

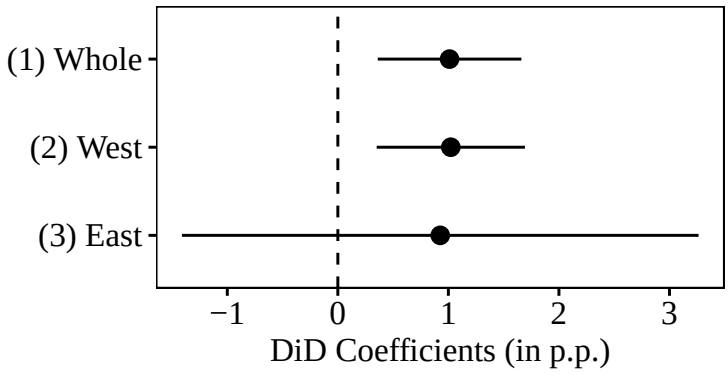


Figure 6: The figure shows the estimated difference-in-differences coefficients for (1) Whole Germany, (2) Western Germany, and (3) Eastern Germany. Estimates are shown in percentage points. The dependent variable is municipal-level AfD vote share. The baseline group are not exposed non-recipient municipalities. Whiskers represent 95% confidence intervals.

both exposed and not exposed non-recipient municipalities experience no internet improvements and should therefore exhibit similar changes in AfD support. Political effects would be expected only in recipient municipalities where high-speed internet is improved. The observation that exposure affects AfD support among non-recipients indicates that voters respond to close-by improvements while at the same being disadvantaged by their unequal spatial distribution[3].

[3]This finding also speaks to recent work emphasizing social media exposure as a driver of radical right support (Gattermann and Tulin 2025; Albertazzi and Bonansinga 2024; Heyna 2025). The observed pattern is difficult to reconcile with a social media mechanism, as AfD support differs systematically among municipalities that all lack high-speed internet improvements. If social media diffusion were the primary channel, political effects should not vary across non-recipient municipalities. While it remains possible that AfD support increased in recipient municipalities, this would imply that the estimates presented here are conservative.

4.3 Robustness analyses and alternative explanations

In this section, I present additional tests to address potential challenges to causal inference, beginning with analyses relevant to both the baseline model (Table E.1) and interaction model (Table 2), followed by model-specific robustness checks.

One potential alternative explanation for the observed changes in AfD support could be simultaneous shocks to other public services. This is a common concern for studies investigating the political effects of policy interventions. While it is difficult to account for all possible unobserved confounders, I focus on public service types that prior research has identified as relevant for populist right support. Specifically, the closure of doctors' offices, hospitals, and schools, as well as the discontinuation of administrative services, affects populist right voting (Dickson et al. 2024; Cremaschi et al. 2024; Nyholt 2024). To address potential confounding from these previously studied shocks, I examine changes in the number of patients per general practitioner and the number of schools per municipality. Appendix G provides details on the estimated models, and Tables G.1 and G.2 report the results. For both public service types, there are no observable differential changes in provision between recipient and non-recipient municipalities during the study period.

Another alternative explanation for the observed shift in AfD support is a difference in political responses to the COVID-19 pandemic. Since the treatment period overlaps with the pandemic, it could be that the effect reflects dissatisfaction with the government's pandemic policies rather than reactions to infrastructure delivery. To explore this possibility, I conduct an additional analysis comparing vote shares for the party *DieBasis* (TheBase) in 2021. Founded in 2020, DieBasis emerged from Germany's anti-Covid protests and received approximately 1.5% of the votes in the 2021 federal elec-

tion. If the observed effect were primarily driven by pandemic-related discontent, I expect differences in support for DieBasis between recipient and non-recipient municipalities. However, as shown in Appendix H, Table H.1, there is no significant difference in DieBasis vote share across the two groups.

I now turn to discussing the supplementary analyses specific to the interaction model. First, I address concerns that the moderating effect of improvement inequality may not capture exposure due to large cluster sizes or may depend on how improvement inequality is measured, i.e., through the way the clusters are built. While Appendix I provides full details, I summarize the key results here. First, I examine whether results are driven by overly large clusters where exposure is less plausible. Table I.1 in Appendix I shows variation in cluster sizes: while the median cluster contains 28 municipalities, the mean is 158, with some clusters reaching 573 municipalities. In large clusters, it is less clear whether residents observe the full extent of improvement inequality. Restricting the sample to below-median cluster sizes (Table I.2), the interaction effects remain statistically significant and substantively consistent with the main findings. Second, I operationalize improvement inequality using a distance-based radius around a given municipality. I calculate the ratio of recipient to non-recipient municipalities within a radius of 10 kilometers and 20 kilometers. The median municipality has 18 municipalities within 10 kilometers and 39 within 20 kilometers, also providing useful contextualization of the cluster-based approach. Table I.4 in Appendix I shows that the estimated effects remain consistent with the main specification, suggesting that the moderating role of improvement inequality does not depend critically on measurement approach.

Moreover, interaction effects often raise concerns related to incorrect linearity

assumptions, model dependence, and limited common support (Hainmueller et al. 2019). To address these concerns, I follow the diagnostic checklist proposed by the Hainmueller et al. (2019). Figure J.1 in Appendix J presents the linear interaction diagnostic plots, Figure J.2 shows the kernel smoothed estimates, and Figure J.3 the shows conditional marginal effects. While the results suggest that the interaction effect is not perfectly linear, theoretically, it is not necessary for the effect to increase linearly across the full range of the moderator. What matters is that medium and high levels of *recipient share* are statistically different from zero, while low levels are not, to be consistent with the argument that feelings of relative disadvantage develop when a meaningful share of surrounding municipalities receive internet infrastructure.

Lastly, the findings are robust to the inclusion of a binary coded version of the recipient-share variable, capturing below and above median exposure to improvement inequality (see Appendix K Table K.1).

5 Conclusion

Can improving public service delivery undermine populist right appeal? In this article, I exploit the spatially unequal provision of high-speed internet in German municipalities to understand how and when public service improvements shape populist right support. The findings demonstrate that improvements to public service delivery can have important complications in areas where improvements are distributed highly unequal across localities. Using a difference-in-differences design, I show that AfD support increases in municipalities that did not receive high-speed internet, primarily in contexts with high improvement inequality. Further suggestive evidence points

out that increases in AfD support among non-recipient municipalities are driven by exposure to close-by improvement inequality. I argue that the observed relationship reflects a relative deprivation dynamic that depends on the spatial distribution of improvements which structures the level of improvement inequality and exposure to it in a given context. In this way, service improvement and relative deprivation co-occur which can lead to political grievances that benefit the populist right among the non-recipients.

The empirical setting combines dynamics that are advantageous for exploring my theoretical argument. First, the expansion divided otherwise comparable municipalities into recipients and non-recipients of public service improvements, producing a quasi-random treatment allocation that supports a causal analysis. Second, high-speed internet is a highly excludable public good due to its place-based availability. Residents in non-recipient municipalities remain limited to low-speed internet, despite being so close to an improved infrastructure. This creates a treatment allocation that is near-dichotomous. The clear distinction between recipients and non-recipients supports the test of a relative deprivation explanation as the observed *positive* policy shock for recipients simultaneously imposes a relative disadvantage on non-recipients. This setting facilitates a test of whether delivery improvements impose relative deprivation on non-recipients, a condition that is more difficult to establish with less excludable public goods.

The conditional pattern in the empirical results leads to several conclusions that contribute to our knowledge on how public services delivery can drive support for the populist right (Cremaschi et al. 2024; Nyholt 2024; Stroppe 2023; Patana 2022). First, the conditional pattern reveals that the observed shifts in AfD support happens

among non-recipient localities. This speaks to an asymmetry in citizen responses to perceived state action: while declines in personal welfare may be met with dissatisfaction, improvement does not necessarily shape political behavior (Larsen 2021). Second, because populist support increases among non-recipients primarily in contexts with high improvement inequality, the findings emphasize that place-based inequalities matter to voters. In line with research on local economic voting, I provide further aggregate-level documentation that voters monitor and evaluate their local context, implied by the changes in voting outcomes. More work is needed, however, to causally analyze how positive interventions affect these perceptions at the individual-level. The implementation of these interventions should be paired with panel survey data to substantiate our knowledge on opinion changes among recipients and non-recipients of public service delivery improvements. Last, the findings also add to recent work on citizens' responsiveness to place-based inequality by demonstrating that relative disadvantages do not automatically translate into political grievances (Green et al. 2025). Instead, the political effects of being disadvantaged vary based on local context conditions, reflecting the importance of exposure and political salience.

Crucially, the results do not imply that public service delivery should not be improved. Rather, they reveal how specific types of improvements can increase support for the populist right under certain conditions. The treatment logic of internet speed improvements is crucial to this finding. The intervention approximates an almost dichotomous logic: its advantages are highly constrained by place, offering no advantage to those living near but not directly in areas with high-speed internet access. However, most public goods are not as exclusionary. Improvements to public transportation, schools, or healthcare facilities may generate positive spillover effects that extend be-

yond immediate recipients. In this way, they may mitigate relative deprivation effects. Future research should examine the political consequences of improvements to other categories of public services to deepen our theoretical understanding and test whether these findings generalize beyond internet infrastructure provision.

More broadly, the results speak to the role of local environments as points of comparison when citizens assess state performance. While much of the literature on place-based inequality has emphasized urban–rural divides, this study draws attention to the political salience of more disaggregated and horizontal divides (Borwein and Lucas 2023a). The findings in this article emphasize the relevance of local level inequalities. Citizens appear to assess public service delivery not only against past baselines or metropolitans, but rely also on information of close-by localities. Still, further work is necessary to understand which dimensions of distributional inequality matter at which scale. The literature on reference points may be a promising point of connection. Neighboring jurisdictions appear to be relevant for assessing tax policies (Besley and Case 1992), and citizens generally appear to turn to country neighbors to evaluate their personal welfare (Hansen et al. 2015). This appears as a fruitful avenue of future research to investigate which comparisons citizens draw on in their judgement of public service delivery and state performance.

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Appendix (intended for online publication)

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A Map of recipient and non-recipient municipalities and clusters

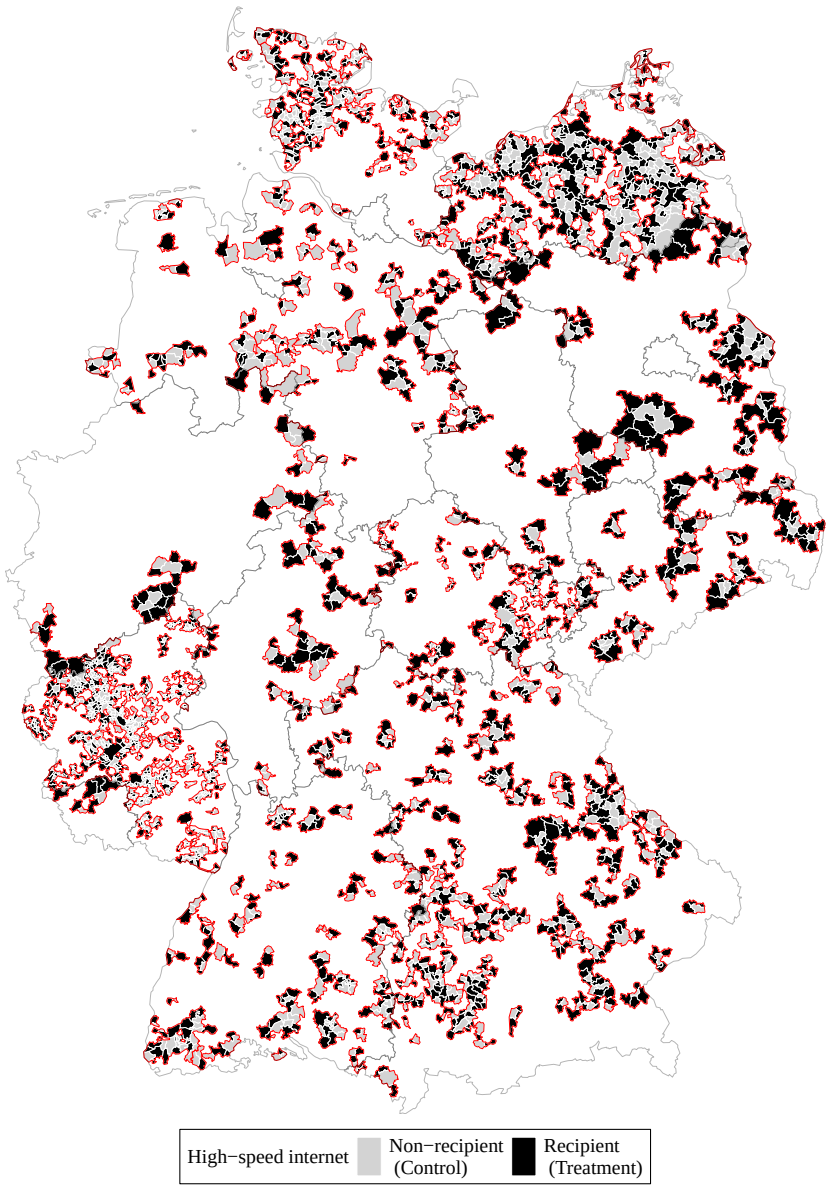


Figure A.1: Location of broadband recipient municipalities (treated) and non-recipient municipalities (control) in Germany. Municipality cluster boaders are marked in red. Overall, there are 1,972 recipient municipalities and 1,538 non-recipient municipalities.

B Summary statistics of pre-treatment covariates

Table B.1 presents summary statistics for the pre-treatment covariates at the municipal-level.

Table B.1: Summary statistics of covariates at the municipal-level

Statistic	N	Mean	St. Dev.	Min	Max
East	3,510	0.273	0.446	0	1
Recipient (treatment)	3,510	0.562	0.496	0	1
Pop. size (2017)	3,510	1,992.547	3,520.843	18	125,596
Pop. density (2017)	3,510	88.853	86.771	4.760	1,058.270
Pop. 10 years trend (2017)	3,510	−4.112	9.065	−44.440	140.610
Pop. 5 years trend (2017)	3,510	−0.531	6.337	−50.300	210.280
Share foreigners (2018)	3,449	0.046	0.045	0.000	0.739
Unemployment (2017)	3,510	3.300	2.103	0.000	18.121
Disposable income (2017)	3,510	22,320.050	2,549.142	11,549.180	45,358.140
Fiscal capacity (2017)	3,510	709.522	1,178.922	−1,443.330	46,764.330
House price index (2017)	3,314	14.733	24.123	−68.684	315.081

C Pre-treatment balance

Table C.1 reports the pre-treatment balance in 2017 across recipient and non-recipient municipalities.

Table C.1: Pre-treatment balance

Outcome	Coefficient	se	p-value	CI	n
Pop. density, log	0.146	0.026	0.000	[0.096; 0.197]	3510
Pop. size, log	0.413	0.041	0.000	[0.334; 0.493]	3510
Pop. 10 years trend, log	0.028	0.078	0.723	[-0.125; 0.180]	1028
Pop. 5 years trend, log	-0.073	0.053	0.163	[-0.176; 0.030]	1536
Share foreigners, log	0.138	0.028	0.000	[0.084; 0.193]	3327
Unemployment	-0.002	0.061	0.972	[-0.121; 0.117]	3510
Disposable income	64.287	79.535	0.419	[-91.654; 220.227]	3510
Tax capacity, log	0.023	0.016	0.135	[-0.007; 0.054]	3455
House price index, log	-0.005	0.041	0.896	[-0.085; 0.075]	2604

Note: The table shows the estimated regression coefficients, heteroskedasticity-robust standard-errors (se), p-values, 95% confidence intervals (CI), and number of observations of the pre-treatment covariates.

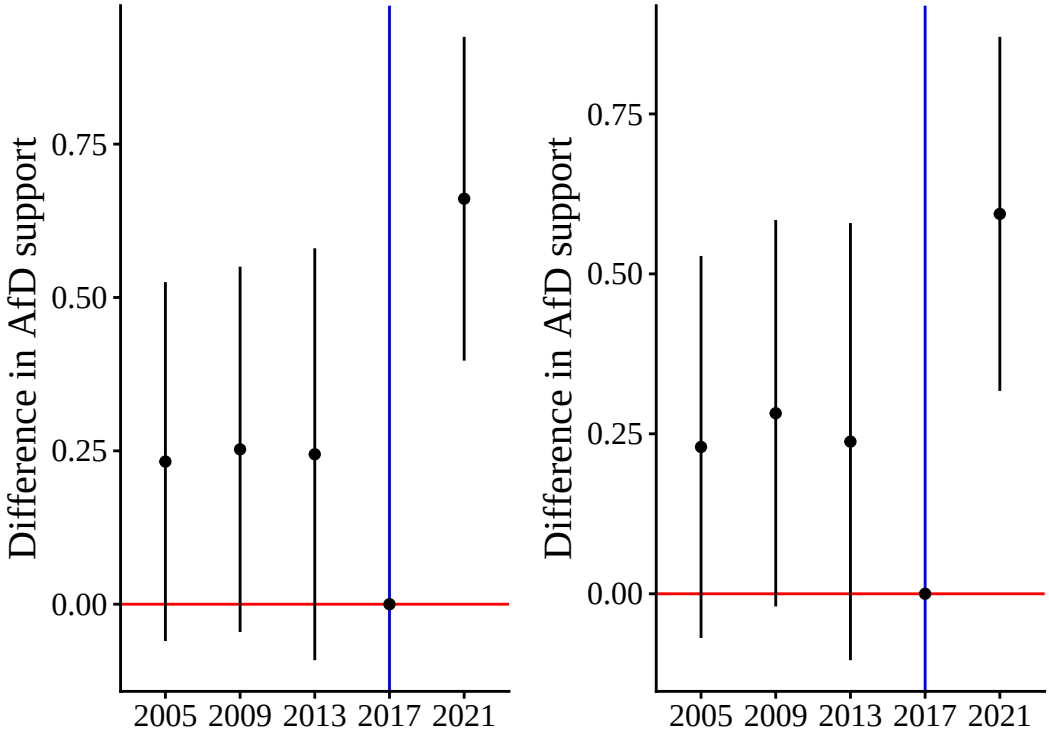
D Testing parallel trends

To test the parallel trends assumption, I estimate a two-way fixed effects event study model. This approach allows me to examine whether recipient and non-recipient municipalities differ in AfD vote share prior to the internet improvements after 2018. I use data from the GERDA database that provide combined election results for far-right parties (see Heddesheimer et al. (2025) for a description of the variable). I code a new far-right variable that matches AfD vote share in 2021, 2017, and 2013 and uses the GERDA far-right categorization as a proxy for the AfD prior to 2013. Using 2017 as the baseline year, I test whether the pre-treatment coefficients are statistically indistinguishable from zero at the conventional 0.05 p-value level. The results are presented in Table D.1, with a visual representation in Figure D.1 (black lines represent 95% confidence intervals). The results show that the parallel trends assumption holds at the conventional .95 level.

Table D.1: AfD support relative to 2017

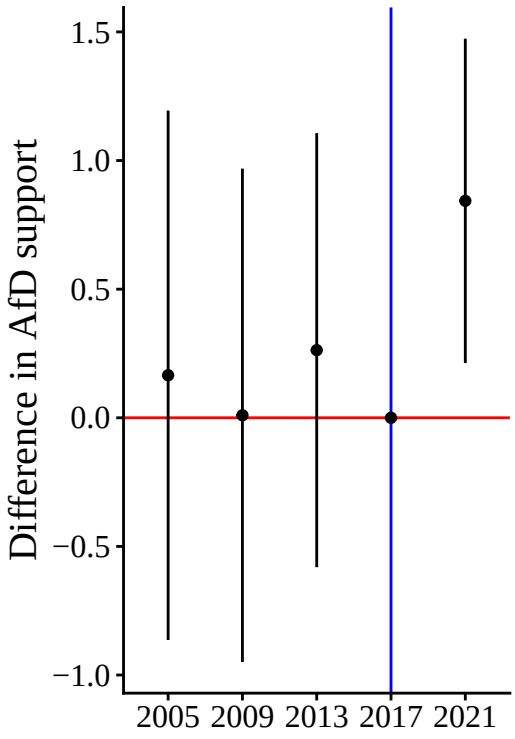
Region:	Whole	West	East
Model:	(1)	(2)	(3)
Variables			
2005 × Non-recipient	0.233	0.229	0.165
	(0.149)	(0.152)	(0.525)
2009 × Non-recipient	0.252	0.282	0.010
	(0.152)	(0.154)	(0.489)
2013 × Non-recipient	0.244	0.238	0.263
	(0.171)	(0.174)	(0.430)
2021 × Non-recipient	0.661***	0.594***	0.843**
	(0.135)	(0.141)	(0.322)
Fixed-effects			
Municipality	Yes	Yes	Yes
East-Year	Yes		
Year		Yes	Yes
Fit statistics			
Observations	16,277	12,592	3,685
R^2	0.901	0.808	0.905
Within R^2	0.002	0.002	0.002

Clustered (Municipality) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05*



(a) Whole

(b) West



(c) East

Figure D.1: Parallel trends plots. Bold lines represent 90% confidence intervals and thin lines 95% confidence intervals.

E Full model results for H1 test

Table E.1: Internet improvement and AfD vote share

Region:	Whole	West	East
Model:	(1)	(2)	(3)
Variables			
Non-recipient	0.524***	0.460***	0.697*
	(0.136)	(0.133)	(0.353)
Fixed-effects			
Municipality	Yes	Yes	Yes
East-Year	Yes		
Year		Yes	Yes
Fit statistics			
Observations	10,362	7,509	2,853
R^2	0.897	0.771	0.897
Within R^2	0.002	0.002	0.002

Clustered (Municipality) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05*

F Exposure vs. no exposure to improvement inequality

Figure F.1 shows the non-recipient and control municipalities included in the alternative test of the exposure mechanism. Table F.1 presents the results of the parallel trends test.

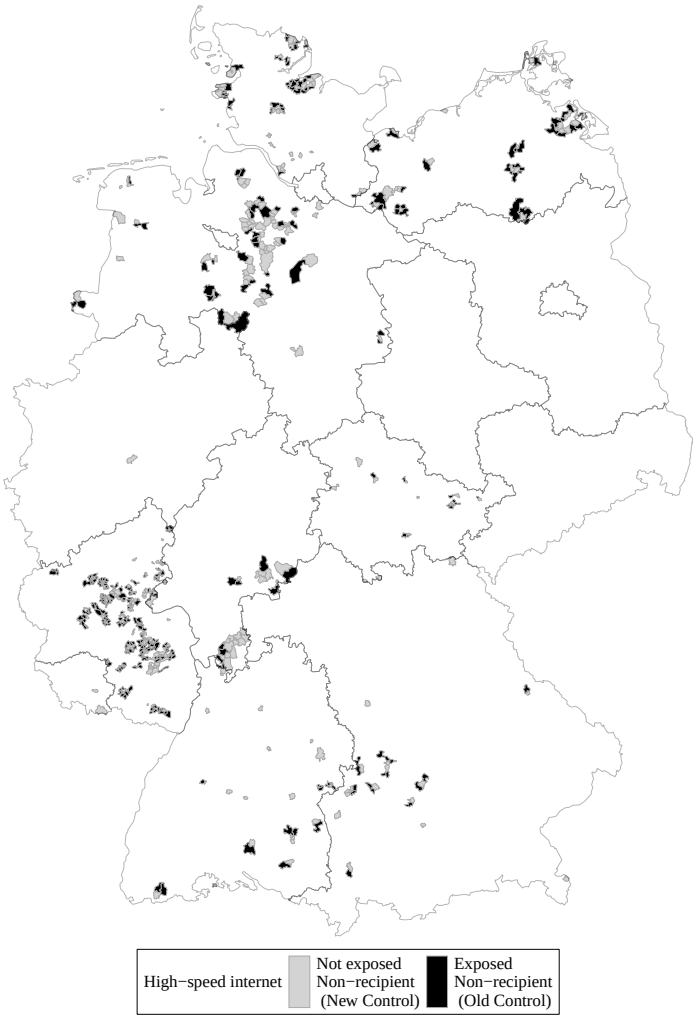


Figure F.1: Location of non-recipient municipalities (n = 335) and control municipalities (n = 335) in Germany.

Table F.1: Parallel trends far right voting

Region:	Whole	West	East
Model:	(1)	(2)	(3)
Variables			
Non-recipient × 2005	-0.301	-0.322	-0.156
	(0.321)	(0.329)	(1.621)
Non-recipient × 2009	-0.259	-0.275	-0.235
	(0.317)	(0.323)	(1.782)
Non-recipient × 2013	-0.364	-0.423	0.078
	(0.368)	(0.371)	(1.453)
Non-recipient × 2021	0.787*	0.783*	0.822
	(0.325)	(0.343)	(1.024)
Fixed-effects			
Municipality	Yes	Yes	Yes
East-Year	Yes		
Year		Yes	Yes
Fit statistics			
Observations	3,200	2,924	276
R^2	0.849	0.777	0.907
Within R^2	0.007	0.008	0.003

Clustered (Municipality) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

Table F.2: Exposed vs not exposed non-recipient municipalities and AfD vote share

Region:	Whole	West	East
Model:	(1)	(2)	(3)
Variables			
Exposed non-recipient	1.011**	1.022**	0.926
	(0.331)	(0.341)	(1.174)
Fixed-effects			
Municipality	Yes	Yes	Yes
East-Year	Yes		
Year		Yes	Yes
Fit statistics			
Observations	1,982	1,750	232
R^2	0.837	0.739	0.893
Within R^2	0.007	0.008	0.004

Clustered (Municipality) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

G Potential other shocks to public service delivery

Could the effects on AfD support be driven by other shocks to public service delivery that occurred simultaneously in recipient and non-recipient municipalities? This is a common concern that studies investigating the political effects of policy interventions should address. While it is nearly impossible to rule out all potential unobserved confounders, I focus on several public service types that prior research has identified as relevant for populist right support. Specifically, the literature has shown that hospital and school closures, as well as the withdrawal of administrative services affect populist right voting (Cremaschi et al. 2024; Nyholt 2024; Dickson et al. 2024). To approximate whether these negative shocks could confound the reported results in this study, I examine changes in the number of schools per municipality and the number of patients per general practitioner (GP). I estimate the following difference-in-differences models to test whether recipient and non-recipient municipalities experienced differential changes in service provision during the studied time frame:

$$\text{PS}_{mt} = \alpha_m + \beta_1 D_m + \beta_2 \text{D}_m \times \text{Post-treatment}_t + \epsilon_{mt}. \tag{3}$$

The dependent variable PS_{mt} denotes the provision of a given public service type in municipality m at election t. The term α_m represents municipality fixed effects, D_{mt} indicates whether a municipality m has high-speed internet infrastructure in election year t and equals 1 if it did *not*. The coefficient of interest is β_2, which reflects the interaction effect of D and the post-treatment election year. A statistically significant β_2 would indicate that recipient and non-recipient municipalities experienced differential changes in the provision of a given public service type, suggesting the possibility of confounding policy shocks beyond high-speed internet improvements.

The results are reported in Table G.1 and G.2. For both public service types, the estimated coefficients suggest that there are no observable differential changes in the provision between recipient and non-recipient municipalities during the observed time frame.

Table G.1: Change in number of patients by GP

Region:	Whole	West	East
Model:	(1)	(2)	(3)
Variables			
Post-treatment (2021)	-0.008	-0.004	-0.018
	(0.006)	(0.006)	(0.013)
Post-treatment (2021) × Non-recipient	0.004	0.009	-0.012
	(0.008)	(0.009)	(0.018)
Fixed-effects			
Municipality	Yes	Yes	Yes
East	Yes		
Fit statistics			
Observations	2,625	1,897	728
R^2	0.987	0.988	0.984
Within R^2	0.002	0.001	0.015

Clustered (Municipality) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

Table G.2: Change in number of schools

Region:	Whole	West	East
Model:	(1)	(2)	(3)
Variables			
Post-treatment (2021)	0.025***	0.025**	0.024*
	(0.007)	(0.009)	(0.012)
Post-treatment (2021) × Non-recipient	-0.012	-0.009	-0.021
	(0.011)	(0.014)	(0.014)
Fixed-effects			
Municipality	Yes	Yes	Yes
East	Yes		
Fit statistics			
Observations	6,950	5,053	1,897
R^2	0.992	0.992	0.994
Within R^2	0.004	0.004	0.006

Clustered (Municipality) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

H Post-treatment dieBasis vote share

Table H.1 reports the results for checking if recipient and non-recipient municipalities differ in the 2021 dieBasis vote share.

Table H.1: dieBasis vote share in 2021

Region:	Whole	West	East
Model:	(1)	(2)	(3)
Variables			
Constant	1.724***	1.740***	1.747***
	(0.038)	(0.042)	(0.065)
Recipient	-0.017	-0.046	0.061
	(0.045)	(0.053)	(0.084)
East	0.071		
	(0.048)		
Fit statistics			
Observations	3,346	2,409	937
R^2	0.001	0.000	0.001
Adjusted R^2	0.000	0.000	-0.001

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

I Alternative ways to measure improvement inequality

Is the conditional effect of high-speed internet improvements robust to the exclusion of large municipality clusters and alternative specifications of exposure to improvement inequality? In this section, I address this question through additional regression models and an alternative measure of improvement inequality exposure. My theoretical argument emphasizes that a) voters should be exposed to improvement inequality for relative deprivation to occur and b) that exposure is most likely to happen in highly unequal local contexts. To operationalize exposure in the main analysis, I construct municipality clusters consisting of all spatially contiguous recipient and non-recipient municipalities in a given area. This approach produces substantial variation in cluster sizes, as some regions contain many neighboring treated and untreated municipalities while others contain few. As shown in Table I.1, municipality cluster sizes have a wide range, with some being as small as 2 and some over 500. This variation raises concerns about the exposure assumption: in very large clusters, it becomes uncertain whether residents are aware of the full extent of the measured improvement inequality. If clusters contain hundreds of municipalities spread across large geographic areas, the assumption that non-recipients observe and react to local improvements becomes less plausible and threatens the interpretaion of the results.

Table I.1: Descriptives on municipality clusters

Mean	Median	Max	Min	p10	p25
158	28	573	2	5	11

To address this concern, I first restrict the sample to below-median cluster sizes. I choose below-median clusters as mean sized clusters still count a number of above 150 municipalities, reflecting a distortion as a result of very large clusters. While exposure to improvement inequality may still be uncertain in clusters exceeding 150 municipalities, clusters of fewer than 28 municipalities better satisfy the assumption that residents can observe disparities in their local context. Table I.2 reports the results. Reassuringly, the estimated interaction effects remain statistically significant and substantively consistent with the main findings: even in smaller, more localized clusters, improvement inequality moderates the effect of lacking high-speed internet infrastructure in non-recipient municipalities.

Second, I employ an alternative approach to operationalize exposure to improvement inequality, using a distance-based radius around a given focal municipality. Within a specified radius, I count the number of recipient and non-recipient municipalities and calculate the ratio of recipients to non-recipients, producing again the share of recipients in a given local context that I use to operationalize improvement inequality and exposure in a given municipality m. This alternative specification

Table I.2: Exposure to internet improvements and AfD vote share, below median size clusters

Region:	Whole	West	East
Model:	(1)	(2)	(3)
Variables			
Non-recipient	-1.148	-0.759	-1.963
	(0.659)	(0.503)	(2.411)
Non-recipient × Recipient share	3.062*	2.130*	5.142
	(1.239)	(0.958)	(3.987)
Fixed-effects			
Municipality	Yes	Yes	Yes
East-Year	Yes		
Year		Yes	Yes
Fit statistics			
Observations	4,938	3,804	1,134
R^2	0.911	0.807	0.899
Within R^2	0.008	0.006	0.013

Clustered (Municipality Cluster) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05*

provides another test of whether the main results are sensitive to the clustering approach.

I calculate improvement inequality within a radius of 10-kilometer and 20-kilometer for each municipality. Table I.3 reports descriptive statistics on the number of municipalities within each distance-based radius. The median municipality has 18 neighboring municipalities within 10 kilometers and 39 within 20 kilometers. these reflect values that fall between the smaller and larger cluster sizes from the main specification, providing useful context for comparing approaches. This approach additionally contextualizes the municipality clustering as the median size reported in Table I.1 falls in between the distance-based radius.

Table I.3: Descriptives on distance-based radius size

Radius	Mean	Median	Max	Min	p10	p25
10km	23	18	92	2	7	10
20km	53	39	205	3	12	19

Again, I estimate the regression model using recipient share as the measure of improvement inequality, interacting it with the binary non-recipient indicator. Table I.4 reports the results. I compute Conley standard errors to account for spatial correla-

tion between the errors (Conley 1999). The estimated effects remain consistent with the main model specification, reinforcing that the moderating role of improvement inequality is not overly dependent on how I measure improvement inequality and exposure. Figures I.1 and I.2 display the Interflex binning approach. Because Interflex does not support Conley standard errors, I cluster the standard errors at the County level (a geographically larger administrative unit). The figures reveal a clear linear relationship that supports the credibility of the linear interaction specification.

Table I.4: Exposure to internet improvements and AfD vote share, distance measure

Region:	Whole	West	East	Whole	West	East
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Non-recipient	-1.002*	-1.332**	0.278	-1.904**	-2.027**	-1.440
	(0.457)	(0.455)	(1.345)	(0.632)	(0.644)	(1.951)
Non-recipient ×						
10km Recipient Share	2.558**	3.077***	0.660			
	(0.852)	(0.908)	(2.173)			
Non-recipient ×						
20km Recipient Share				4.111**	4.337**	3.356
				(1.254)	(1.388)	(3.220)
Fixed-effects						
Municipality	Yes	Yes	Yes	Yes	Yes	Yes
East-Year	Yes			Yes		
Year		Yes	Yes		Yes	Yes
Fit statistics						
Observations	10,362	7,509	2,853	10,362	7,509	2,853
R^2	0.898	0.773	0.897	0.898	0.773	0.897
Within R^2	0.004	0.007	0.002	0.006	0.009	0.003

Conley standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05*

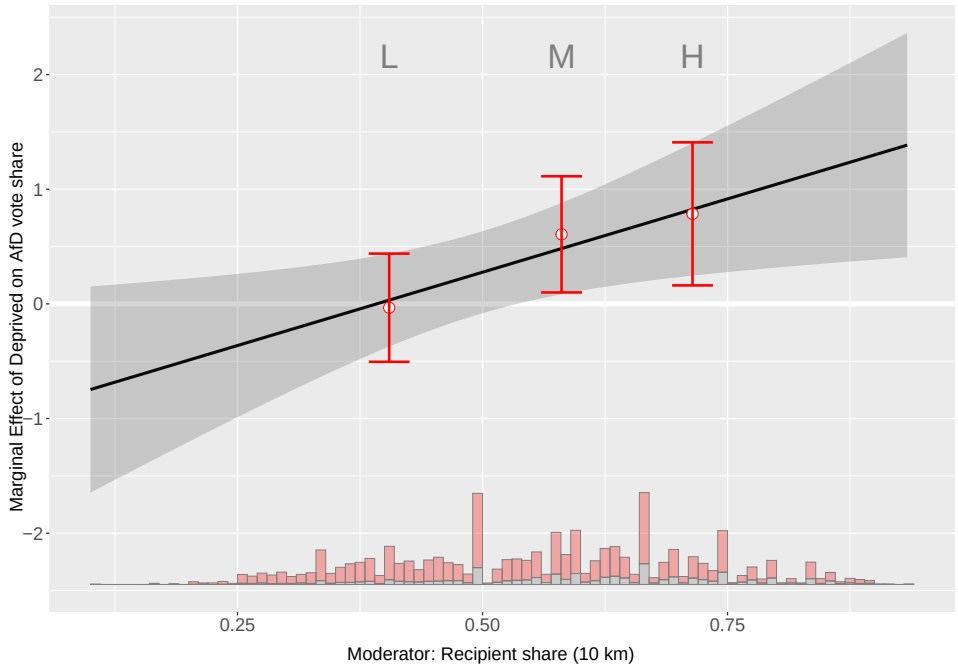


Figure I.1: Binning, 10 km radius

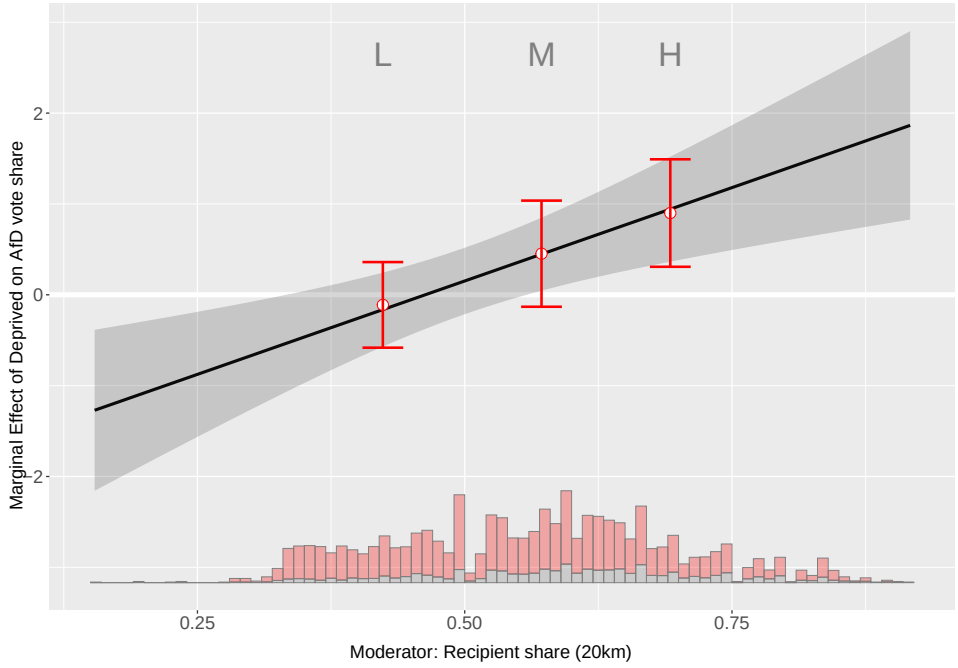


Figure I.2: Binning, 20 km radius

J H2: Interflex robustness checks

I follow the diagnostic checks recommended by Hainmueller et al. (2019) to assess the validity of the assumptions underlying linear interaction models. Figure J.1 shows the Linear Interaction Diagnostic plots, which are simple scatter plots of the underlying data. While the relationships are not perfectly linear, slightly less linear in the non-recipient group, they broadly follow a linear trend (the kernel estimation shown in Figure J.2 suggests a linear association). The slope for the non-recipient group appears slightly steeper than that of the recipient group, indicating a positive interaction effect. Lastly, common support appears across a slightly broader range of values within the recipient group.

Figure J.3 presents the results of the binning estimator using the recommended three bins. Again, this visualization indicates that the interaction effect does not strictly follow a linear trend, this does not undermine the interpretation of the results. The exposure mechanism only requires that a meaningful level of exposure to internet improvements in non-recipient municipalities is necessary to shape AfD vote share and does not assume a perfectly linear relationship.

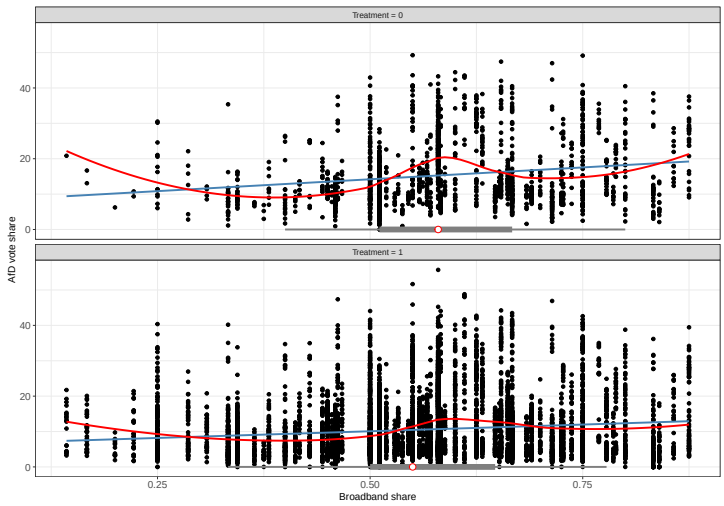


Figure J.1: Linearity check

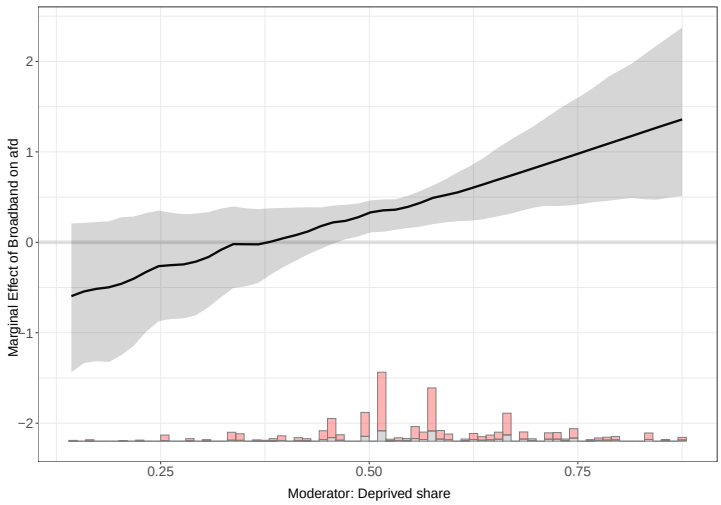


Figure J.2: Kernel estimation, shaded areas represent 95% confidence intervals.

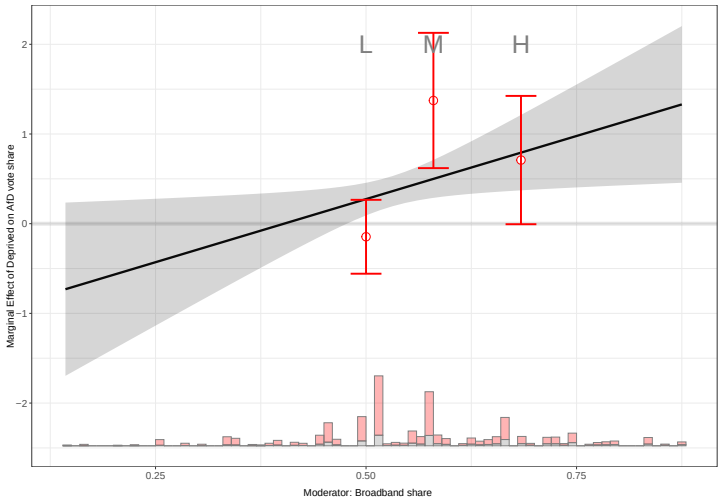


Figure J.3: Binning approach

K H2: Median split of recipient share

Table K.1 shows the interaction effect model re-estimated with a median-split version of the original recipient share variable.

Table K.1: Spatial DiD, AfD vote share

Region:	Whole	West	East
Model:	(1)	(2)	(3)
Variables			
Non-recipient	0.061	0.033	0.512
	(0.151)	(0.111)	(1.020)
Non-recipient × Above median recipient share	0.772*	0.859*	0.228
	(0.359)	(0.425)	(1.088)
Fixed-effects			
Municipality	Yes	Yes	Yes
East-Year	Yes		
Year		Yes	Yes
Fit statistics			
Observations	10,126	7,391	2,735
R^2	0.896	0.771	0.897
Within R^2	0.004	0.006	0.002

Clustered (Municipality cluster) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

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