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Bridging the Digital Divide Narrows the Participation Gap: Evidence from a Quasi-Natural Experiment

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Abstract: Socio-economic inequality in access to the internet has decreased in affluent societies. We investigate how gaining access to the internet affected the civic and political participation of relatively disadvantaged late adopters by studying a quasi-natural experiment related to the American National Election Studies. In 2012, when about 80% of the U.S. population was already connected to the internet, the ANES face-to-face study was for the first time supplemented with a sample of online respondents. Our design exploits the fact that the firm (KnowledgePanel) that conducted the web survey and provided the prerecruited respondents had equipped offline sample households with free laptop computers and internet access. The findings show that gaining internet access promotes late adopters' civic participation and turnout, whereas there is no evidence for effects on the likelihood of political activism. These findings indicate that the closing of the digital divide alleviated participatory inequality.

Keywords: political participation; civic participation; digital divide; internet; political inequality

Replication Package: A replication package including all analysis code is available on the Open Science Framework (https://osf.io/vq34k/).

THE advent of the internet sparked hopes of a revitalized civic and democratic society. But scientists and pundits also warned that the technology might exacerbate the socio-economic stratification of civic and political participation if the well-off adopt the internet as yet another tool while the disadvantaged cannot afford a computer with internet subscription (e.g., DiMaggio et al. 2001; DiMaggio and Garip 2011; Norris 2001). Indeed, this was a valid concern. Initially, people at the lower end of the socio-economic ladder adapted internet technology at much lower rates and often mentioned financial constraints as the reason (e.g., National Telecommunications and Information Administration 2002, chapter 8). However, this "digital divide"[1] has narrowed in affluent societies (van Deursen and van Dijk 2019; Sanders and Scanlon 2021; Van Dijk 2020, chapter 4). For example, the percentage of internet users increased from 60% in 2009 to 86% in 2021 among U.S. adults with an annual income below $30,000; whereas, in 2009, it stood already at 95% among those with an income above $75,000 (Pew Research 2021). Did this closing of the digital divide ameliorate participatory inequality?

In this article, we investigate whether gaining access to the internet benefited the civic and political participation of underprivileged late adopters. To do so, however, we must go beyond observational data to account for selection effects that complicate the causal assessment. In fact, also among late adopters, it may be that engaged citizens disproportionately adopt internet technology, supposedly to still their hunger for news and information (Jennings and Zeitner 2003). Or it may

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be that people who shy away from real-world social involvement are particularly inclined to use the internet (Bauernschuster, Falck, and Woessmann 2014:84). We deal with endogeneity issues by studying a unique quasi-natural experiment. In 2012, about 80% of the U.S. population had internet access (Zickuhr and Smith 2012) and the American National Election Studies (ANES) face-to-face interviewing was for the first time complemented with a sample of respondents who answered the questionnaire online. Offline households presented an obstacle to the firm (KnowledgePanel) that conducted the web survey and provided the pre-recruited respondents. When recruiting respondents into their panel, KnowledgePanel therefore offered a laptop computer and internet subscription for free to offline sample households. We investigate how receiving a computer with internet access affected civic and political participation among the people in this "treatment group." The ANES two-mode design allows us to compare their participation levels to offline respondents in the face-to-face study and, at the same time, account for baseline differences across the web and face-to-face study.

Connecting to the internet could boost participation through various pathways, and this mobilizing effect might even be especially strong for underprivileged late adopters. Brady, Verba, and Schlozman (1995) identified three reasons that help explain why some people do *not* participate: they can't; they don't want to; and nobody asked. The internet allows conveying information in simple, interactive ways and this could make it possible for those with less education and political resources to form opinions and join the discussion (Mahéo 2017). The non-hierarchical and participatory nature of the internet could also encourage people at the lower end of the socio-economic hierarchy to assume a more active role in society and, thus, increase their willingness to participate (Anduiza, Cantijoch, and Gallego 2009, section 3.3). In addition, even if disengaged late adopters do not go online to seek information in the first place, they will often be exposed to political content, for example content that acquaintances shared on social media, and this could stimulate their interest (Valeriani and Vaccari 2016). Finally, over time, civic and political organizations shifted their recruitment and mobilization efforts more and more online. Connecting to the internet could thus boost late adopters' participation levels by ending their seclusion from online recruitment channels.

Other arguments question whether the closing of the digital divide decreased participatory inequality or even suggest an opposite effect. Greater availability and easier access to information may increase knowledge and engagement among the resource rich but have little impact on those who have more difficulty absorbing the information (Tichenor, Donohue, and Olien 1970). In fact, the abundance of information online might reduce the capacity to participate among the latter group by leading to information overload (Richey and Zhu 2015). Underprivileged late adopters may also not benefit from the fact that internet access makes certain forms of participation easier if they still lack the skills to write an email to a government official or the money to make an online donation. Digital media use might also undermine rather than stimulate late adopters' desire to participate: Pre-existing preferences might be reinforced and a voluntary segmentation might occur because the immense choice of content allows those who lack interest to stop consuming news altogether and tune out even further (Heiss and Matthes 2019; Prior 2005).

Finally, online recruitment will be skewed towards people who are likely to participate and have resources for effective participation, maybe even more so than offline recruitment (Schlozman, Verba, and Brady 2010). Online mobilization may also reinforce participatory gaps because the recipient typically has to make the first step (e.g., by visiting a website or subscribing to a channel or mailing list; Krueger 2006).

The empirical literature is inconclusive: Cross-sectional studies show that internet users are more engaged citizens than non-users (for meta analyses see Boulianne 2020; Chae, Lee, and Kim 2019; Lorenz-Spreen et al. 2023), but studies that attempt to identify a causal effect yield an ambiguous picture (see Table 1 for a summary of selected studies; see also the review by Zhuravskaya, Petrova, and Enikolopov, 2020, section 1.1). For instance, Hampton and Wellman's (2003) analysis of a natural experiment that occurred in a newly built Canadian suburb confirms a positive effect of internet use on involvement in the local community. By contrast, Richey and Zhu's (2015) analysis of ANES panel data reveals that internet access through Web-TVs that KnowledgePanel handed out to offline households did not affect political knowledge, efficacy, and interest. This null result could reflect that Web-TVs arguably failed to provide user-friendly internet access (KnowledgePanel also started equipping offline households with standard laptop computers in 2009, slightly before the Web-TV technology was discontinued). But null results and even negative effects have also been obtained by some studies that exploit technological and historical peculiarities that led to quasi-random variation in the supply of fast internet across geographic space. Gavazza et al. (2019), for example, exploited the fact that high rainfall increases the costs of supplying broadband services and used an instrumental variable estimation to show that in election districts with less rainfall and, hence, higher broadband penetration, turnout rates were lower.[2]

The inconsistency of empirical findings might be due to real variation across domains of participation, time, countries, and stages in the diffusion process (Bimber et al. 2015; Campante, Durante, and Sobbrio 2018; Geraci et al. 2022). Internet skills, usage patterns, and effects also differ between societal groups (Hargittai 2021; Mahéo 2017; Schlozman et al. 2010) and research should thus focus explicitly on late adopters to investigate how the closing of the digital divide affected participatory inequality. Although there is a scarcity of such research, some studies that have compared effects across subgroups of society suggest that digital media use enhances engagement among people with less education, income, or political interest, maybe even more than among people further up the socio-economic hierarchy (Bimber et al. 2015; Morris and Morris 2013; Valeriani and Vaccari 2016). However, these findings might again reflect endogeneity of technology adaption, and they are called into question by results of studies with more rigorous designs (Gavazza, Nardotto, and Valletti 2019; Heiss and Matthes 2019; Hur and Kwon 2014; Oser and Boulianne 2020; Richey and Zhu 2015). Thus, although major policy programs promoting universal access to the internet were in part motivated by concerns about participatory inequality and, although calls for continued efforts to connect "the last few" persist, we still know little about how the closing of the digital divide actually affected late adopters' participation levels.

Table 1: Summary of the design and findings of selected studies.

Study (Country, year*, internet penetration#)	Key aspects of study design	Key findings
Hampton & Wellman (2003) Canada, 1998, 25%	Prospective residents of a new suburb were promised high-speed internet. Due to unforeseen issues only a random subset of homes was connected ('treatment group') while a 'control group' remained without internet connection.	Positive internet effect on community involvement (recognizing community members and talking to them).
Richey & Zhu (2015) USA, 2008, 74%	KnowledgePanel equipped offline households with WebTVs in staggered waves. This created exogenous variation in internet access among participants of a longitudinal ANES study, the effects of which are examined in a control-waitlist design.	No internet effect on political interest, efficacy, and knowledge.
Miner (2015) Malaysia, 2004-8, 42-56%	Costs of supplying internet services increase in distance to 'network backbone.' With backbone placement reflecting geographical constraints, this provides exogenous variation in internet penetration across districts that is exploited in an instrumental variable estimation.	No internet effect on turnout at district level.
Poy & Schüller (2020) Italy, 2013, 58%	Exploit spatial and temporal variation of advanced broadband rollout; difference-in-differences approach controlling for year and municipality fixed effects.	Positive internet effect on turnout rates in national elections.
Bauernschuster et al. (2014) Germany, 2001-8, 32-78%	Panel analysis to account for time-invariant individual traits. Plus, instrumental variable estimation exploiting regional variation in broadband internet supply induced by differences in broadband compatibility of pre-existing infrastructure.	Positive internet effect on political interest, null effects on civic and political participation.
Campante et al. (2018) Italy, 2006-13, 38-58%	Costs of supplying internet services vary with distance to 'high-order exchange' in the telecommunication network. This created variation in internet access across municipalities that is exploited in an instrumental variable estimation.	Negative internet effect on turnout in 2006 and 2008, null effect in 2013. Positive effect on grass-root political participation.
Falck et al. (2014) Germany, 2004-8, 65-78%	Instrumental variable estimation exploiting regional variation in broadband supply related to (1) distance from 'main distribution frame' in telecommunication network and (2) compatibility of infrastructure with broadband technology.	Negative internet effect on voter turnout rates in West German municipalities, no effect in East Germany.
Gavazza et al. (2019) UK, 2006-10, 72-85%	Rainfall increases costs of providing fast internet. This leads to variation in internet penetration across election districts that is exploited in an instrumental variable estimation.	Negative internet effect on turnout rates (stronger in districts with higher share of less educated people).
Geraci et al. (2022) UK, 1997-2017, 7-90%	Broadband internet speed decays with distance to 'network node,' which leads to exogenous variation in access to fast internet that is investigated within the instrumental variable framework.	Negative internet effect on chances of membership in civic and political organizations.

∗ Years of outcome assessment, may be compared to data from 'pre-internet' years.
% of population using the internet; data from https://data.worldbank.org.

Our primary goal is to assess the causal effects of connecting potential late adopters to the internet. We examine causal effects on participation in three domains: civic participation, turnout, and political activism. Participation in these domains is related (c.f. Ekman and Amnå 2012; Jenkins et al. 2003) but it may be that gaining internet access has different effects on participation in local community life than on participation in national politics (Wellman and Gulia 1999) or on participation in behaviors that are common, such as voting, than on engagement in more demanding forms of political participation that only a small number of activists engage in (Bakker and De Vreese 2011). We describe in the next section how the ANES 2012 data allow us to identify causal effects, and we present the results in the section "Does connecting to the internet affect participation?".

Secondarily, in the section "Exploration of mechanisms", we explore possible pathways through which gaining internet access affects late adopters' participation levels. Although a comprehensive testing of possible mechanisms is not possible with the data at our disposal, we use available information to trace four possible pathways.

Research Design and Methods

The two modes of the 2012 ANES Time Series study—face-to-face interviewing and self-complete, online surveys—were conducted with independent samples. GfK KnowledgePanel (formerly 'Knowledge Networks') organized the web study and invited a sample of their panel of regular survey respondents to participate. For the initial recruitment of panelists, KnowledgePanel used standard random digit-dialing and address-based sampling methods. If the recruitment interview revealed that a household did not have a home computer and access to the internet, the household members were offered a standard Windows-based laptop computer and a monthly internet subscription as part of the compensation for periodically participating in surveys (American National Election Studies 2014, p. 22).

This created a quasi-natural experiment. A "treatment group" of people who were randomly sampled from the offline population was furnished with internet access by KnowledgePanel. Sometime later, their civic and political participation was measured in the ANES web study. At the same time, a "control group" of randomly sampled offline individuals answered the ANES questionnaire in the face-to-face study. For the purpose of our study, we restrict the treatment group to those respondents who were recruited initially in the years 2009, 2010, or 2011 (N = 271). This choice reflects that, before 2009, KnowledgePanel equipped offline households with WebTVs rather than standard laptop computers and it warrants that the treatment had some time to unfold its effect.[3] The control group comprises the 446 respondents of the face-to-face study who indicated that they do not have internet access at home.

Table 2(A) provides descriptive statistics. We refer to individuals of the treatment and control group as '*laggards*,' borrowing terminology from the literature on the diffusion of innovations (Rogers 1962). Table 2(A) also provides information on '*adopters*' (i.e., ANES 2012 respondents who had themselves adopted internet technology). Table 2(A) shows that laggards are more often non-White and on

average older, less educated, and poorer than adopters. Retrospective reports also reveal an expected turnout gap in the presidential election *before* the treatment: Turnout in 2008 was lower among laggards than among adopters, within both survey modes. Thus, Table 2(A) shows the expected associations between socio-economic position and internet access as well as participation (DiMaggio and Garip 2011).

How did receiving a laptop with internet access affect participation? We investigate this question by focusing on 13 participation behaviors that fall into three domains: (a) acts of civic participation such as the attendance of community meetings, (b) turnout in the presidential election and primaries, and (c) acts of political activism as measured by a 5-item module on activist engagement in the election campaign (see e.g., Collitt and Highton, 2021, or Mason, 2015, for studies using these items to identify political activists). Table 2(B) shows the percentage of participators among adopters and laggards, in both survey modes, for each of the 13 behavioral outcomes. We examine the effect of gaining internet access on each of the 13 participation behaviors individually as well as on the indices "civic participation," "turnout," and "political activism," which are mean scales of the binary items.[4]

Simple treatment-control comparisons of participation rates are not adequate. The treatment group completed the survey online, whereas the control group was part of the face-to-face study. Treatment-control differences could thus include substantial survey mode effects on (over-)reporting engagement (cf. DeBell et al. 2020; Liu 2017). Furthermore, online respondents participated regularly in surveys, and this could have affected their actual engagement, similar as answering the ANES pre-election questionnaire increases turnout (Jackman and Spahn 2019). Finally, minor differences in the sampling procedures and non-response bias could have led to compositional differences across the modes.

Due to these complications, we employ a difference-in-difference approach to "correct" for survey mode. We also use the data of adopters (respondents who had themselves adopted internet technology) and compare treatment-control differences to "baseline mode differences" observed among adopters. Specifically, we regress indicators of engagement on the binary variables *web mode* and *laggard* as well as their interaction. The coefficient of the interaction term *web mode* $\times$ *laggard* captures the treatment effect. It measures the direction and degree to which laggards who participated in the web study and thus received a laptop with internet access differ in their participation levels more from laggards in the face-to-face study than adopters in the web study differ from adopters in the face-to-face study. We use linear regression when considering the indices civic participation, turnout, and political activism as dependent variables. We also use linear regression (linear probability models) for the 13 individual, binary indicators of participation because logistic or probit regression yield biased estimates for group comparisons based on coefficients of multiplicative terms (Mood 2010).

Models are estimated on a sample of 3,643 respondents, distributed across the four groups as shown in the bottom row of Table 2.[5] Throughout, we use full information maximum likelihood (FIML) estimation, rather than OLS estimation, to warrant consistency with robustness analyses that include covariates and where this estimation procedure allows us to retain respondents with missing values on

individual control variables. Despite using FIML, we exclude respondents with a missing value on the respective dependent variable and thus estimate the individual models on samples of somewhat fewer than 3,643 respondents (see the regression tables in online supplement A).[6]

We note that our approach presumes that "mode effects" do not differ systematically between adopters and laggards. We see no clear reasons why survey mode effects—taking the survey in person vs. online—on response styles and desirability bias should differ between laggards and adopters. However, it is conceivable that more engaged citizens are particularly interested in gaining access to the internet (Jennings and Zeitner 2003), and that engaged offline citizens were therefore especially likely to accept the invitation to join KnowledgePanel's respondent pool. This could have led to compositional differences across the treatment and control group that do not match compositional differences across the modes among adopters. A placebo test indicates that such imbalance does not exist: Individuals who received access to the internet (treatment group) did display levels of past political engagement that were similar to those who did not receive access (control group).

For this placebo test, which also serves to illustrate our approach, we consider turnout in the 2008 presidential election as the outcome variable. This election took place *before* the treatment and we should thus not observe a "treatment effect." Table 2(A) shows that turnout in 2008 was 14.0 %-points higher in the treatment group than in the control group. However, this treatment-control difference differs little from the baseline mode difference: Turnout was also 11.7 %-points higher among adopters in the web study than among adopters in the face-to-face study (Table 2(A)). The regression of turnout on *web mode*, *laggard*, and their interaction does *not* (falsely) suggest a treatment effect: The interaction effect is not significant (b = 0.023; $p = 0.536$; see online supplement A, Table A2, for the full regression results). This confirms that when we account for baseline differences across the modes, the people of the treatment group were not a-priori more or less engaged citizens than those of the control group. This result also indicates that if we observe effects on post-treatment participation, these effects reflect how connecting to the internet changes actual behavior, rather than how connecting to the internet affects over- or under-reporting of participation.

A replication package including all analysis code is available on the Open Science Framework (https://osf.io/vq34k/).

Does connecting to the internet affect participation?

We first look at simple descriptive comparisons across groups. Table 2(B) describes the levels of political participation of respondents of the treatment group (laggards, web), the control group (laggards, face-to-face), as well as respondents who had themselves adopted internet technology (adopters, web and face-to-face). The share of participators is on all 13 behaviors higher among adopters, in both survey modes, than in the control group of individuals without internet access at home. This may reflect the socio-economic stratification of internet access and participation rather than a causal effect of internet use on participation. However, also the treatment group of laggards who received a computer and internet subscription outperforms

Table 2: Socio-demographic characteristics and pre-treatment political participation **(A)** and post-treatment participation **(B)**, separate for adopters and laggards in both survey modes. All variables are binary (0 = no, 1 = yes) and numbers indicate percentages, except for age and household income.

	Adopters		Laggards	
	Face-to-face	Web	Face-to-face (Control)	Web (Treatment)
(A) Background characteristics				
Age	43.2	48.9	52.9	55.3
Female	57.6	49.3	54.3	57.9
Race/ethnicity				
White	47.6	67.9	34.7	46.1
Black	22.6	11.9	33.8	26.4
Hispanic	22.9	14.2	24.3	23.4
Other	7.0	6.1	7.2	4.1
College degree	28.2	37.7	8.6	12.7
HH income (yearly; in $1000)	53.9	65.7	23.7	28.2
Turnout 2008 Presidential election	69.9	81.6	58.2	72.2
(B) Post-treatment participation				
Civic participation (past 12 months)				
Contact government official	15.1	25.4	7.8	22.4
Community meetings	25.5	26.6	13.9	29.5
Community work	31.8	33.3	20.4	30.3
Volunteer work	47.0	45.6	28.4	37.8
Organization membership	46.1	53.0	25.6	43.2
Donation to church or charity	71.8	70.7	60.4	71.0
Turnout (2012)				
Presidential election	75.9	81.4	61.9	75.8
Presidential primaries	24.9	39.1	19.8	46.7
Political activism				
Persuade others to vote	40.0	40.9	31.1	39.0
Attend speeches, rallies	5.7	5.5	3.6	5.4
Display campaign sign	14.4	17.3	14.3	20.3
Other work for a campaign	3.8	3.3	2.9	4.6
Donation to a campaign	11.9	15.1	6.6	9.5
N	1606	1318	446	271

the control group on all 13 indicators of participation. For almost all indicators, the treatment-control difference also exceeds the baseline difference across the modes among adopters. Thus, Table 2(B) suggests that gaining access to the internet enhances participation among late adopters.

The difference-in-difference estimation confirms positive and statistically significant effects on civic participation and turnout (Figure 1; online supplement A contains the regression tables). The model estimates suggest that gaining internet access increases late adopters' chances of engaging in community meetings, volunteer work, and charitable donations by more than 10 %-points. The estimates also reveal

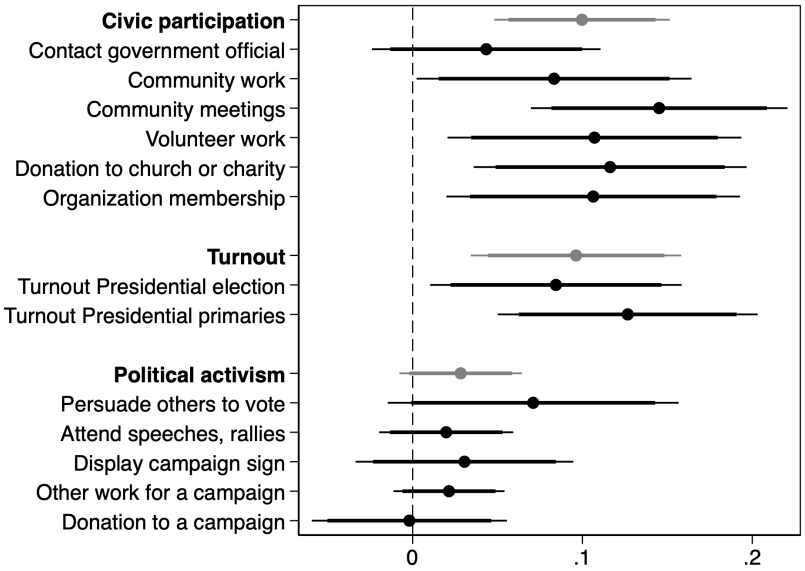

Figure 1: Estimates of the effect of gaining access to the internet on the likelihood that potential late adopters engage in thirteen participation behaviors (black markers). Gray markers show estimates of effects on indices constructed as mean scales of the binary items under the respective heading. Point estimates and confidence intervals (90 and 95%) from linear models. Online supplement A contains the associated regression tables.

substantial and statistically significant improvements in the chances of organization membership and engagement in community work. For the likelihood of contacting government officials, the coefficient estimate points to an improvement, too, but it is not statistically significant. Concerning turnout, the model estimates suggest that connecting potential late adopters to the internet increases their likelihood of voting in the presidential election and the presidential primaries by 8 and 13 %-points, respectively.

On the other hand, the difference-in-difference estimation does not indicate that gaining access to the internet affects political activism (Figure 1). We do not find that connecting late adopters to the internet affects the likelihood that they (a) talk to others trying to influence their vote; (b) attend political meetings, like rallies or speeches, in support of a candidate; (c) publicly display their support for a candidate, for instance, by wearing a campaign button or placing a sign in the window or front yard; (d) do any (other) work to support a party or candidate; or (e) donate money to support a candidate. In sum, the results show that gaining access to the internet increases late adopters' civic participation and turnout but does not lead to more demanding forms of political activism.

Does the impact of gaining access to the internet vary across subgroups of late adopters?

The effects of connecting to the internet vary little between socio-demographic groups of late adopters. Figure 2 illustrates results of regressions with three-way

interactions testing whether the treatment effect, measured by the interaction *web mode* × *laggard*, depends significantly on a third (binary) variable such as gender (see Online Supplement B for the full regression results). Dependent variables are the indices civic participation, turnout, and political activism (i.e., mean scales of the items listed under the respective headings in Figure 1). Figure 2 shows that there is hardly any significant variation in treatment effects by gender (male vs. female), race (White vs. non-White), education (college degree or not), yearly household income, and age (income and age are dichotomized on their median among the sample of laggards). Only whether a respondent is older than 55 significantly moderates the effects of gaining internet access on civic participation and political activism ($p = 0.039$ and 0.001; p-values *not* adjusted for the multiple comparisons). Given the limited size of the treatment and control groups, these results should not be interpreted as evidence for an absence of heterogeneous effects. But they do suggest that the observed participation enhancing effects occur across socio-demographic subgroups of late adopters.

On the other hand, the data suggest that connecting to the internet may promote participation especially among those who were already engaged. The only information available about *pre*-treatment engagement pertains to turnout in the 2008 presidential elections. The right-most panel in Figure 2 shows a significant interaction ($p < 0.001$) such that gaining internet access increases turnout among those who have already been politically involved, but not among those who did not vote in 2008. This aligns with the conjecture that the internet as a high-choice medium increases voluntary segmentation and mobilizes primarily those who were already engaged (Prior 2005) and with findings that digital media use reinforces participation but does not mobilize new people to participate (Oser and Boulianne 2020). However, effects on civic participation and political activism do not vary significantly by whether someone went to the polls in 2008, which may reflect that pre-treatment turnout is more distant to these participation outcomes.

Robustness of results

Highly similar results are obtained from alternative analysis approaches that differ in the degree to which they rely on the quasi-random assignment of internet access. Comparing participation rates across the treatment and control group using chi-square tests, which neglects possible baseline differences across the modes, suggests positive and highly significant effects on all indicators of civic participation and turnout ($p \leq 0.01$) and non-significant or borderline-significant effects on the indicators of political activism ($p \geq 0.04$; online supplement C.1).

The results presented in Figures 1 and 2 are also robust to the inclusion of control variables. Our analysis approach assumes that compositional differences across the modes are congruent among adopters and laggards, but we find that this assumption is violated on some socio-demographic characteristics (Online Supplement C.2, Table C2). Therefore, we included in additional analysis variables to control for past turnout, age, gender, race/ethnicity, education, household income and composition, religiosity, and geographic region. This renders the estimated effect on the likelihood of engagement in community work insignificant ($p = 0.051$)

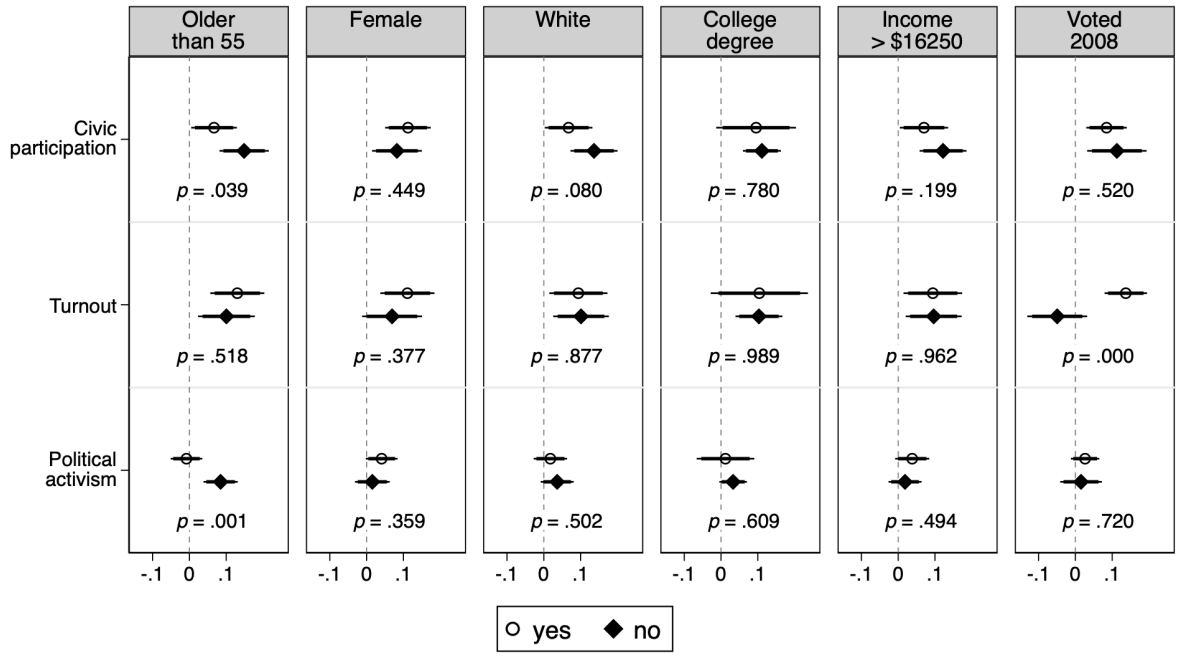

Figure 2: Estimates of the effect of internet access on civic participation, turnout, and political activism, separate for different groups of potential late adopters. Point estimates, confidence intervals (90 and 95%), and p-values for the difference between the groups from linear models. Online supplement B contains the associated regression tables.

but does not change any of the other results at the 5% significance cutoff (Online Supplement C.2).

Exploration of mechanisms

Through which mechanisms does connecting to the internet increase late adopters' civic participation and turnout? Guided by our review of mechanisms in the introduction and by the availability of data[7], we explore four pathways: interest in politics, political efficacy, political knowledge, and having been contacted online by a party or candidate during the campaign. Unlike our previous analysis, this mediation analysis is descriptive: It includes steps for which we cannot leverage the quasi-random allocation of internet access to measure causal relationships. Therefore, we included to all regressions reported in this section the same set of control variables as in the robustness analysis reported in the previous section.[8]

How does gaining internet access affect late adopters' political interest, efficacy, and knowledge, and their chances of being contacted online? To examine this, we regressed, for instance, *political interest* on *web mode* and *laggard* as well as their interaction, and our interest is again with the coefficient estimate of the interaction term. Figure 3 summarizes the results and Online Supplement D provides the

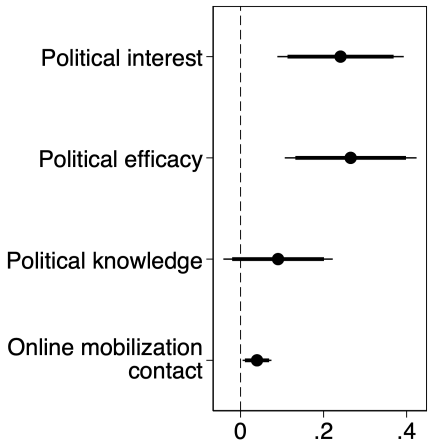

Figure 3: Estimates of the effect of gaining access to the internet on late adopters' political interest, efficacy and knowledge (standard scores), and the likelihood that they are contacted online in a mobilization effort (dichotomous outcome). Point estimates and confidence intervals (90 and 95%) from linear regressions. Online supplement D contains the associated regression tables.

full estimation results and details on the operationalizations. Figure 3 shows that gaining access to the internet leads to an increase by 0.24 standard deviations in late adopters' interest in politics ($p < 0.01$; standardized scale of two questions about paying attention to politics and interest in political campaigns). This could result from incidental exposure to political content and easier access to information. Connecting to the internet also increases late adopters' political efficacy (standardized scale of four questions about internal and external political efficacy), which could be due to the non-hierarchical, participatory nature of the internet. Finally, receiving a laptop with an internet connection leads to a 4%-point increase in the likelihood that a respondent reports having been contacted by a party or candidate by email, through a social network site, or in another web-based manner during the campaign. On the other hand, despite easy access to a wealth of information, connecting to the internet does not significantly increase late adopters' political knowledge (measured by the standard score of the proportion of correct answers to 17 questions testing political knowledge, including office recognition questions).

To what extent do these pathways account for the participation enhancing effects of gaining internet access? We focus our analysis on the effects on civic participation and turnout (Online Supplement D also contains analyses for effects on political activism). Political interest, efficacy, and online mobilization contact are all positively associated with civic participation and turnout ($p < 0.001$). To establish this, we added these mediators as covariates to the regressions testing effects of gaining internet access on civic participation and turnout (see Tables D2 and D3). These mediators also explain part of the participation enhancing effect of gaining internet access: Adding them individually to the models leads to a decrease in the size of the estimated effect of gaining internet access on civic participation of 15% (political efficacy) to 17% (political interest) and a decrease in the size of the estimated effect of gaining internet access on turnout of 6% (mobilization contact)

to 19% (political interest). However, these changes in the size of "direct" effects are not statistically significant ($p \geq 0.62$; Tables D2 and D3). In sum, we find descriptive evidence that gaining access to the internet promotes late adopters' civic participation and turnout by stimulating their interest in politics, increasing their trust that they can understand and influence political affairs, and increasing their chances of being contacted online in mobilization efforts. However, only a relatively small part of the participation enhancing effect of gaining internet access seems to run through these pathways.

Conclusion

We find that gaining access to the internet increases late adopters' civic and political participation. This suggests that the diffusion of internet technology and deliberate efforts at promoting universal access contributed to narrowing participation gaps: Gaining access to the internet allowed late adopters of lower socio-economic status to catch up with people who are better off and connected earlier to the internet.

The more specific pattern of results carries a positive message, too. We find that connecting to the internet promotes late adopters' participation in national politics *and* local community life. This stands at odds with fears that the internet would undermine the local community and lead to a fragmented mass society in which people are at best involved in national politics and transient, virtual communities of shared interest (Nie, Hillygus, and Erbring 2002; Putnam 2000). Instead, our findings align with arguments suggesting that the internet can protect community life and facilitate mobilization for local collective action (Antoci, Sabatini, and Sodini 2015; Hampton 2010; Hampton and Wellman 2003). In addition, the absence of clear effects on the likelihood of activist forms of political participation can be taken to suggest that gaining internet access stimulates late adopters to be involved, without becoming fierce partisans, contrary to suggestions about opinion polarization online. This pattern of results also indicates that the effects of connecting late adopters to the internet vary along a dimension that ranges from "common acts of good citizenship" to "less common acts of activism," rather than along a distinction between civic participation and political participation (see Ekman and Amnå, 2012 and Jenkins et al., 2003 for discussions of the close relationship between civic and political participation).

Our study design allows for a clear causal assessment of the effects of connecting to the internet on political and civic participation, ruling out the possibility that differences are driven by endogeneity of technology adoption. However, the estimated effect sizes should be viewed with caution. Engaged citizens are especially likely to accept an invitation to partake in a survey (DeBell et al., 2020), and our results provide some (albeit quite limited) indication that gaining internet access boosts especially the participation levels of engaged citizens. Thus, our estimates could be inflated. On the other hand, we focused exclusively on traditional forms of participation, and the neglect of new forms of participation that take place exclusively online might lead to an underestimation of the effects.

Caution is also warranted in the interpretation of the results of our exploration of mechanisms. These results indicate that internet use partly promotes participa-

tion because it stimulates interest, increases feelings of efficacy, and leads to the inclusion to online recruitment channels. However, the strength of the observed associations may not accurately reflect the strength of causal relationships. For example, the observed association between gaining internet access and feelings of political efficacy may exceed the strength of the direct causal effect if connecting to the internet also increases participation for some other reason and if participation increases feelings of efficacy. More research is needed to firmly establish these relationships.

We also want to warn against possible misinterpretations. Our findings show that connecting disadvantaged late adopters to the internet enhances their participation *compared to* a situation in which they remain in a small offline minority. This does not imply that the advent of the internet generally led to an increase in the participation levels of those at the lower end of the socio-economic ladder. It is, for example, possible that their participation levels fell when they were still offline while information, communication, and recruitment channels moved online. When they eventually connected to the internet and escaped the seclusion from the online sphere, their participation levels bounced back but may not have reached pre-internet levels.

Similarly, our findings indicate that the closing of the digital divide contributed to a reduction of participatory inequality *compared to* a situation with persisting disparities in internet access along the socio-economic ladder. This does not mean that the advent of the internet reduced participation gaps. Maybe more affluent people benefited more from connecting to the internet than underprivileged late adopters or, as just explained, maybe the advent of the internet had a negative "net" effect on late adopters' participation levels.

Digital divides in access to fast and reliable internet still exist today, to some extent even in wealthy countries (van Deursen and van Dijk 2019; Mangla et al. 2022; Sanders and Scanlon 2021; Watts 2020), and our results suggest that efforts to connect the "last few" will help in lessening participatory inequality and strengthening community life and democratic politics. However, it is clear that effects of bridging the digital divide might differ across countries and historical time (Bimber et al. 2015; Campante et al. 2018). Although our study indicates that the reduction of digital inequality decreased participatory inequality in the United States at the beginning of the last decade, future studies should investigate whether similar effects exist nowadays in countries that still have sizable populations without access to the internet.

Notes

1 With *digital divide* we refer to disparities in physical access to the internet. The term is also used in relation to disparities in internet skills and usage patterns (second-level digital divide) and disparities in returns to internet use (third-level digital divide).

2 The studies by Campante et al. (2018), Falck et al. (2014), Gavazza et al. (2019), and Geraci et al. (2022), summarized in Table 1, investigate causal effects of *fast* (broadband) internet access. Some potentially participation enhancing affordances of the internet do not require a fast connection, as is the case with the possibility for making an online

donation or sending an email to a government official. In addition, fast internet may increase the chance of information overload and the chance that entertainment replaces information. Hence, negative internet effects reported by some of these studies may represent negative effects of fast vs. slow internet access, rather than effects of access vs. no access.

3 Another reason to exclude from the treatment group respondents who were furnished with free internet access before 2009 is that the composition of the offline population changed over time (Zickuhr and Smith 2012). The group of those who had no internet access, for example, in 2006 is not identical to the group of those who still had no internet access in 2012 and who were eligible for the control group. If more engaged citizens are likely to adopt internet technology earlier (Jennings and Zeitner 2003), including in the treatment group individuals who were furnished with free internet access a longer time ago (before 2009) could bias our results because the treatment group would likely contain more engaged people than the control group.

4 The 13 items together form a scale with reasonable internal consistency (*alpha* = 0.76) and the three indices are correlated (r[civic participation, turnout] = 0.27, r[civic participation, political activism] = 0.38, r[turnout, political activism] = 0.30; $p < 0.001$; the correlations are highly similar among adopters and laggards). This confirms that participation in these domains is related. Not surprisingly, however, a factor analysis also provides some support for grouping the 13 items into three domains as we do in our analysis and as they are grouped in the ANES questionnaires (data available upon request). The participation rates reported in Table 2(B) also show that most forms of turnout and civic participation are rather common, whereas only relatively few people engage in behaviors that can be labeled political activism.

5 Adopters in the web study are included to the sample only if they entered KnowledgePanel's respondent pool in 2009, 2010 or 2011 because the treatment group is restricted to laggards in the web study who were recruited in these years. Additional analysis also including adopters in the web study who were recruited earlier or later lead to the same substantive results as reported here.

6 *Not* removing respondents with a missing value on the respective dependent variable does not alter any of the reported results at the 5% significance level (analysis available upon request).

7 Some questions of interest—for example, questions about online news consumption—have not been presented to respondents who had indicated that they do not use the internet, and we cannot therefore test questions related to these aspects.

8 Online Supplement D provides analyses showing that none of the results reported here change at the 5% significance cutoff if the control variables are omitted.

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