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# Felony Status, Political Identity, and Civic Engagement

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**ABSTRACT**
There is a popular, widespread assumption that adults with felony records are overwhelmingly aligned with the Democratic Party. To evaluate this popular belief, we evaluate the partisan orientation and civic participation of nonincarcerated American adults with felony records. We compare three national YouGov surveys fielded after 2022 midterm election: adults with felony records, adults demographically matched to the felony population but not screened for felony status, and the general population. Compared to the general population, we find that people with felony records report significantly lower turnout and a slight overall lean toward the Democratic Party, but these differences are largely explained by demographics and local partisan context. These findings clarify both the limited partisan stakes and the broader democratic stakes of restoring voting rights to people with felony records.

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A prevalent assumption among policymakers and the public is that people with felony records overwhelmingly support Democrats and liberal causes (see, e.g., Adams, 2016; Fung, 2023; Boodhoo 2023), despite some recent reports that suggest considerable support for Donald Trump among people in prisons and jails (Lewis et al., 2024). Researchers have used various informative but indirect methods to investigate this question, finding some support for this claim (Burch, 2011 Meredith & Morse, 2015; Uggen & Manza 2002). Nevertheless, few if any previous studies have directly elicited partisan self-identification information from people with felony records at the national level.

The question of partisanship is consequential for elections as well as individuals, especially as more Americans with felony records regain the right to vote. Efforts to roll back felony disenfranchisement policies in the United States have met considerable recent success (Uggen et al., 2024), and the last decade has featured the largest expansion of voting rights to people with felony records since Reconstruction (Behrens et al., 2003; Uggen et al., 2024). According to estimates by The Sentencing Project, the number of people prohibited from voting because of a felony conviction has dropped by 31% between 2016 and 2024 (Uggen et al., 2024). Nevertheless, we still know little about how or whether increased electoral access amongst this population might affect rates of political participation and the partisan balance of the general electorate.

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This research empirically assesses the voting behaviors and political affiliations of people with felony records by conducting an original survey of people with self-reported felony records and comparing them against a general adult sample and a demographically matched sample. Through these comparisons, we find corroboratory support for the claim that people with felony records are less likely to vote compared to the general population. Second, although on average this group leans more Democratic, we find varied and nuanced political views and orientations among people with felony records. Moving beyond assumptions and grounding the debate in empirical evidence, this research contributes to a more informed and balanced understanding of the political implications of re-enfranchisement. In doing so, it helps situate the political identity and behavior of people with felony convictions within the great complexity and diversity of justice-impacted populations.

## Background

Scholars estimate that at least nineteen million American adults have at least one felony conviction (Shannon et al., 2017). Although such figures are generally difficult to obtain, estimates by racial group are especially fraught as different states use different methodologies and definitions of racial identification, and racial self-identification is not static (Liebler et al., 2017). Nonetheless, Shannon et al. (2017) estimate more than one-third of the total population of people with felony convictions (which we refer to as the "felony population" for brevity) is Black. According to their estimates, approximately 8% of U.S. adults had a felony record in 2010 and 23% of Black U.S. adults had a felony record at that time. Given the disproportionate representation of Black adults among the felony population, it is important to consider whether the political impacts of a felony conviction are moderated by race. Put another way, the relationship between felon status and political behavior may not be uniform across racial groups.

Moreover, more than ninety percent of the ever-convicted felony population is not incarcerated but instead lives in the community. This includes those currently serving a community supervision sentence (such as probation or parole) and those who have completed their felony sentences and are no longer under felony-level correctional control. Whether members of this population can vote depends on their state of residence, their correctional status, and (in some states) their specific offense (Uggen et al., 2024). During the 2024 election, two states and the District of Columbia did not disenfranchize anyone based on a felony conviction; 23 states prohibited only people in prison from voting; 15 prohibited people who were in prison or on a community supervision (parole or probation) sentence; and 10 disenfranchized some or all people with felony convictions during and after completion of their sentences. However, this constellation of state policies has been in flux: a dozen states have rolled back elements of their disenfranchisement restrictions since 2016 (NCSL, 2024; Uggen et al., 2024). As a result, national levels of disenfranchisement have decreased significantly. According to estimates by The Sentencing Project, approximately 4 million people were prohibited from voting because of a felony conviction during the 2024 election, which represented an 8% decline since the 2022 midterm election and a

31% decline from disenfranchisement's high point of 5.9 million during the 2016 presidential election (Uggen et al., 2024).

Although this expansion of voting eligibility represents considerable success for advocates, there remains steadfast resistance. Opponents of rights restoration have argued that losing one's rights is part of the punishment (e.g., Clegg & Von Spakovsky, 2018) and that even if rights were restored, people with felony records are not likely to vote anyway (Haselswerdt, 2009; Miles, 2004). Less explicitly, some opponents have claimed rights restoration efforts are partisan "vote grabs" or surreptitious efforts to expand the base of the Democratic Party (e.g., Jacobson, 2015). In this study, we test whether re-enfranchisement of people with felony records would overwhelmingly benefit Democrats by examining their expected turnout and party support.

### *Determinants of Political Participation and Partisanship*

The determinants of participation and partisanship are multifaceted, encompassing a range of characteristics. For example, higher socioeconomic status and educational attainment have been consistently linked with higher political participation and a more substantial likelihood of identifying with a political party (Verba et al., 1995; Willeck & Mendelberg, 2022). Race and ethnicity significantly influence both participation and partisanship, with different groups exhibiting distinct political preferences and varying levels of engagement, partly due to systemic barriers and mobilization efforts (Dawson, 1994; Tate, 1994). Gender differences in political participation have narrowed or "vanished" in recent years (Burns et al., 2018), but disparities in partisanship persist, influenced by socialization, issue preferences, and candidate appeal (Krupnikov et al., 2021; Norris, 2000). Local context plays a critical role, with individuals' political leanings and participation rates being shaped by local political cultures, community norms, and the competitiveness of elections (Gimpel et al., 2003; MacKuen & Brown, 1987). The partisan context of neighborhoods can have a particularly strong influence on party choice net of individual level characteristics (Burbank, 1997; Cantoni & Pons, 2022; Gimpel et al., 2004).

Beyond these demographic predictors, partisanship has become a central component of individual identity, increasingly intertwined with other social identities, religiosity, and personal values (Green et al., 2002; Perry, 2022). This phenomenon has led to stronger in-group affiliations and out-group animosity, making partisan identity a powerful predictor of political behavior (Iyengar et al., 2012; Stewart et al., 2018; West & Iyengar, 2022). The rise of identity-based politics, in which political parties represent not just policy preferences but also (and perhaps more acutely) demographic, religious, and cultural identities, has further solidified the role of partisanship as a key facet of social identity (Mason, 2018).

Thus, as a form of social identity, partisanship can drive people to vote to support their in-group and influence political outcomes in favor of their party (Green et al., 2002). Partisanship fosters issue salience and can also spur participation, especially in elections that prominently feature such issues (Carmines & Stimson, 1980). More generally, familial, community, and social network pressures all motivate people to vote (Mcclurg, 2003).

### *Felon Status and Political Identity*

The question remains as to how felon status may affect engagement and partisan identity, though there have been several efforts to address these questions. Uggen and Manza conducted an early counterfactual analysis estimating the impact of felony disenfranchisement on U.S. Senate and presidential elections (Uggen & Manza, 2002; Manza & Uggen, 2008). To construct their counterfactual estimates, they modeled turnout and vote choice among the disenfranchized population based on the observed voting behavior of eligible citizens who matched them in terms of gender, race, age, income, labor force status, marital status, and education. For example, in the 2000 presidential election in Florida, they estimated that approximately one-third of disenfranchized people would have voted if eligible. Further, they would have supported the Democratic candidate by a two-to-one margin, thus swinging the election to the Democrat. Their approach rested on the assumption that political preferences and engagement are tightly linked with and can be accurately predicted by demographics, and therefore that the political preferences of disenfranchized people with felony convictions would align with the preferences of those who are demographically similar.

Burch (2011, 2012) expanded these efforts by examining how felony convictions affect actual voting behavior through administrative data linking. By matching correctional data to voting records, Burch estimated how disenfranchized people would vote based on the turnout and registration patterns of formerly disenfranchized people in five states. Her analyses suggested that Uggen and Manza overestimated turnout among people with felony convictions (averaging 22% across the five states). Later work by Meredith and Morse (2015), Gerber et al. (2017), and others who used similar administrative data linking approaches found similarly low turnout relative to Uggen and Manza's estimates. Based on party registration data, Burch also found that partisan alignment varied by race; therefore, it would have been unlikely that disenfranchisement would have affected the outcome of the 2000 election. Analyses of linked administrative data such as these provide several advantages, but they are not without their limitations. These analyses heavily depend on the quality of their linking procedures, as overly stringent matching techniques often lead to underpowered studies that fail to detect real treatment effects (Tahamont et al., 2021), and administrative data are generally time-limited, thus left censoring older segments of the felon population who are more likely to vote than younger segments (Bhatti et al., 2012).

Nevertheless, prior research provides reasons to expect that felon status would affect political identity. Disenfranchisement laws in many states prevent people with felony records from voting, directly removing them from the political process (Manza & Uggen, 2006). As a result, people with felony records may experience a diminished sense of agency and a heightened distrust of government and institutions, thereby inhibiting their engagement and altering their political identities due to a sense of exclusion (Travis, 2005; Weaver & Lerman, 2010). Experiencing such discrimination clearly disrupts employment trajectories and residential stability (e.g., Western, 2006). Research on "system avoidance" (Brayne, 2014) also suggests diminished political participation, as does research on legal cynicism (Sampson & Bartusch, 1998), legal estrangement (Bell, 2017). Further, incarceration and felon status can lead to changes in social networks, distancing individuals from their civic and political roots. Criminal legal system experiences might destabilize or displace preexisting personal or familial partisan allegiances or loosen foundational values or earlier identity markers. This isolation can reduce exposure to political discussions and activities, impacting partisan self-identification (Burch, 2011; Weaver & Lerman, 2010). Experience with the criminal legal system may also affect how people perceive the two major political parties. Policy advocacy related to law enforcement, sentencing, and rehabilitation may similarly influence their political identities, possibly aligning them with parties advocating criminal justice reform (Meredith & Morse, 2014, 2015).

Although such arguments are plausible, it may also be the case that felon status does not have a particularly salient influence on political identity. People's political identities and engagement are also shaped by long-standing social, economic, religious, and cultural factors that precede their criminal legal experiences, such that felon status may not easily or drastically alter preexisting political beliefs (Green et al., 2002; Perry, 2022; Woodall & Shannon, 2022). On average, people convicted of felonies have comparatively low levels of educational attainment and economic status, both of which are associated with lower engagement. They may also have developed an antipathy or sense of disconnection from the state and its institutions before criminal legal involvement (Miller & Stuart, 2017). Conversely, for some, specific issues rather than party allegiance may drive political engagement. Thus, people with felony records may remain politically active and maintain staunch political identities, regardless of their ability to participate in elections and past disenfranchisement (Knight, 2024; Owens & Walker, 2018; Walker, 2020).

Considering this political and scholarly context, our primary research questions are as follows:

1. Are people with felony records (who are eligible) less likely to vote?
2. Are people with felony records more likely to self-identify as Democrats, and is that self-identification consistent across racial groups?
3. Are people with felony records more likely to have supported Joe Biden rather than Donald Trump in the 2020 presidential election, and is that support consistent across racial groups?

These questions highlight the possibility of race-based variation in outcomes. We do not assume that felon status will have identical effects for different racial groups. Thus, our analysis will test for any such moderating influences.

## Data and Methods

To answer these questions, we collected new national survey data so that we could compare the preferences of people with felony records with those of other eligible voters. This study thus draws on a comparative analysis of three national surveys fielded by YouGov after the 2022 midterm election to address these questions. YouGov is a popular opt-in survey platform with a panel of more than 1.5 million US respondents. It uses a multistage-matched sampling process to generate a sampling frame. In the first stage, YouGov creates a synthetic sampling frame that simulates large, rigorous probability-based samples (such as the American Community Survey) from which they select a random sample or target sample. In the second stage, they use proximity-matching techniques to select one or more members of their opt-in pool that matches each randomly selected member of the target sample on a range of variables (Ansolabehere & Rivers, 2013). This approach, in theory, should reduce sample selection bias.

Recent comparisons of YouGov with probability-based social surveys (e.g., GSS) have provided support for this methodological approach, finding that it produces results similar in direction and magnitude to those derived from more expensive and intensive probability-based sampling strategies (Ansolabehere & Schaffner, 2014; Graham et al., 2021; Revilla et al., 2015; Simmons & Bobo, 2015; Thompson & Pickett, 2020). YouGov is the primary survey firm used by the Cooperative Election Study (CES; formerly the Cooperative Congressional Election Study), a national stratified sample survey fielded in election years with pre- and post-election waves. In their comparative analysis of the CES with the ANES, which uses an area-probability sampling method, Ansolabehere and Rivers concluded that the CES produced "nearly identical summary statistics and regression estimates for models of turnout and vote choice" (2013, p. 326).

### *Felony, Matched, and CES Samples*

We conduct comparative analyses of three national samples, which we refer to as the Felony Sample, the Matched Sample, and the CES Sample. Working with YouGov, we surveyed a sample of individuals ($n = 1{,}000$) who self-disclosed a felony record (e.g., "Felony Sample") and a demographically matched comparison sample ($n = 1{,}000$) from the general population ("Matched Sample"). The CES Sample draws on data collected through the post-election wave of the CES ($n = 50{,}981$). Data were collected for the Felony Sample from December 9, 2022, through January 10, 2023, and for the Matched Sample from January 18, 2023, through January 30, 2023. The CES sample was collected in two waves, from September 29, 2022, through November 7, 2022, and then from November 10, 2022, to December 15, 2022, respectively. By comparing these three samples, we aim to disentangle the extent to which our outcomes of interest are explained by demographic factors and/or felony status.

Establishing a sampling frame that resembles the population of those with felony records is a difficult task. Benchmark data describing the national population of people with felony records in terms of observable, socio-demographic characteristics that are reliable and obtainable are not realistically available. It would potentially be possible to generate benchmark data at the state level for some states, but the breadth, scope, and quality of data available on this population varies considerably from state to state (Sandoval & Lageson, 2022).

### *Census Click Methodology*

To identify the Felony sample, YouGov used a "census click" methodology to construct a representative sample of individuals with felony records. They first created a synthetic sampling frame of a known population (US residents) of which the target population (people with felony records) is a subset. The frame was constructed by stratified sampling from the full 2019 American Community Survey (ACS) 1-year sample with selection within strata by weighted sampling with replacements. They invited successive random samples of respondents from the sampling frame to complete four screening questions, which included the key screening question ("Social scientists estimate that nearly 20 million Americans have been convicted of a felony at some point in their lives. Have you ever been convicted of a felony?') and three dummy questions on unrelated but sensitive topics (e.g., history of unemployment, gun possession, prior marijuana use) to mask the intended selection criteria.

During a census clicks data collection, YouGov counts every person who opens the link in the related quota frame to ensure that the total sample represents the target population as much as possible. The assumption is that if YouGov can adequately sample an encompassing population (in this case, the general US population) using meaningful demographics, the natural fallout of the target sample should resemble that of a random sample of the target population (i.e., those who have been convicted of a felony). For this project, 35,022 respondents started the screening survey and were counted in YouGov's quotas during fielding. After data collection, YouGov applies several internal criteria to ensure high data quality, such as filtering people who skipped multiple questions, who sped through the survey, used duplicate IP addresses, or provided internally inconsistent item responses. YouGov also removes panelists who have been cleaned out of other YouGov surveys and who have repeatedly failed internal attention checks.

Following cleaning and data preparation, YouGov matched and weighted the data to the original target frame to ensure that the sample was representative using propensity scores. The sample cases and the frame were combined, and a logistic regression was estimated for inclusion in the frame. The propensity score function included age, gender, race/ethnicity, years of education, and region. Propensity scores were grouped into deciles of the estimated propensity score in the frame and post-stratified according to these deciles. Weights were then post-stratified on a four-way stratification of gender, age (4-categories), race (4-categories), and education (4-categories). Following the weighting procedure, those screened out were removed (that is, those *without* felony convictions), leaving 1,000 completed cases.

A similar approach was taken to create the Matched Sample. For the matched sample, 2,400 people started the survey and were counted in the quotas. This was to ensure that they represented a random sample of the closest known population, which was the population demographics that represented the US felon population—that is, the synthetic felon sampling frame rather than the ACS 2019. The Matched Sample was matched and weighted similarly to the felon sample to produce the 1,000 completed cases.

### Cooperative Election Study

The Cooperative Election Study (CES) is fielded annually to investigate the voting patterns, electoral experiences, attitudes, and beliefs of the American electorate. The CES has a large sample size, which allows for detailed estimation of electoral dynamics across different constituencies. During election years, CES data collection consists of two waves: one before the election and one shortly after. In addition to the Felony and Matched Samples, we include a CES Sample, drawn from the 2022 Cooperative Election Study (Schaffner et al., 2024). Only those respondents who completed both waves were included in the analytic sample. To adjust for any remaining imbalance that exists among the CES Sample, we use the "commonpostweight" weighting variable as we draw on items from both waves of the survey.

### *A Note about Selection Bias*

Our felony-status respondents come from an opt-in, online panel and had to self-disclose a conviction on a screening questionnaire, therefore this sample could be systematically different from the full population of people with felony records. The sample likely overrepresents people who are both willing to complete an online survey and comfortable revealing their felony status. However, we believe this potential selection bias effect is mitigated, at least in part, for two reasons.

First, respondents in opt-in surveys that use multi-stage matching methodologies– including YouGov–are more civically engaged than the general population and that survey participation may itself be an expressive form of political participation (Schaffner & Luks, 2018; Graham & Huber, 2021; Hopkins & Gorton, 2024). In a recent benchmarking study comparing opt-in panelists to the general population, Hopkins and Gorton (2024) found that panelists in their large opt-in survey voted in primary and midterm elections at rates far greater than state averages. As this study compares relative self-reported voting behavior and ideology between three samples drawn from the same panel, we expect that selection bias related to civic engagement would be similarly distributed across the samples.

Second, our felony sample relies on a screening question to identify eligible respondents. Although administrative or other external criminal history data may be preferable, topic-specific applications to sensitive populations consistently yield prevalence estimates that comport with prior research despite acknowledged selection concerns. Recent evidence on using opt-in panels to learn about gang membership suggest that online opt-in panels can be effective in capturing hard-to-reach, justice-involved adults at non-trivial rates, while reproducing known correlates of criminal justice contact (Pyrooz et al. 2025b). Pyrooz et al. (2025b) identified eligible respondents with a single yes/no "ever a gang member" item and still reproduced the demographic and legal patterns found in probability samples. Screening questions have been used in other sensitive contexts with opt-in samples, including research on experiences with intimate-partner violence (Adhia et al., 2021), exposure to mass shootings (Pyrooz et al. 2025a), and sex-trafficking victimization (Kulig, 2022). Therefore, we have confidence that our targeted screening approach can be effective in identifying the population of interest.

### *Analytical Approach*

Our goal is to determine whether the differences in political behavior and identity, as described in previous research, can be mainly explained by demographic differences between the felony population and the general population, or whether they are more directly associated with having felony status. To achieve this, we conduct comparative analyses of the three samples. The Felony and Matched samples share similar demographic profiles, including characteristics that influence political participation and partisanship such as race, gender, educational attainment, and income. The distinction between these two samples is that Felony Sample respondents were screened for felony record status while Matched respondents were not. In contrast, the demographic profile of the CES sample is reflective of the general population.

Our comparisons are guided by two key expectations. First, by comparing the Felony sample to the Matched sample, we isolate the effects of felony status on outcomes independent of demographic factors. Second, by comparing the Matched sample to the CES sample, we highlight how differences in outcomes are influenced by demographic variation. Together, these dual comparisons offer a lens through which we attempt to disentangle the distinct influences of felony status and demographic factors.

As an example, if an outcome such as voter turnout differs between the Felony sample and the Matched sample, the difference is likely explained by felony status. If, however, similar turnout is observed among the Felony and Matched samples, but they differ from the CES sample, these differences in demographic composition provide a likely explanation. Finally, if all three samples differ, then the difference is likely explained by both felony status and demographics (see Table 1 in which $T_F$, $T_M$, and $T_C$ represent outcomes for the Felony, Matched, and CES samples, respectively). Moreover, by comparing the Felony Sample with the CES Sample, we can understand how individuals with felony records, as a specific demographic group, differ from or align with the general population regarding political behaviors and beliefs. The general population represented by the CES sample serves as a baseline, allowing us to identify unique patterns for those with felony records.

In short, this approach directly addresses a key limitation of much earlier work, specifically the lament that researchers "do not have any survey data that asks disenfranchised felons how they would have voted" (Uggen & Manza, 2002, p. 784). These insights will provide an empirical basis for better understanding voter behavior, addressing political representation and inclusivity, and informing public policy, particularly policy around the needs and priorities of a historically disenfranchized population. The Matched Sample, which is demographically matched to the Felony Sample but not screened for felony status, is a similarly situated comparison group. Ideally, this comparison will allow us to isolate the effect of having a felony record from demographic factors. By comparing the Felony Sample with the Matched Sample, we can better recognize whether any observed differences in political engagement or affiliations are attributable to the felony record itself or are merely expressions of the underlying demographic composition of the group.

For each of our analyses, we estimate separate models for our Felony sample (people with felony records), CES sample (general adult population), and Matched sample (demographically matched to felon population but not screened for felony status). We chose to run separate models rather than pooled models for several

**Table 1.** Sample comparison expectations.

| $T_F \approx T_M$ | $T_F \approx T_C$ | $T_M \approx T_C$ | Likely Explanation |
|---|---|---|---|
| Yes | Yes | Yes | Not applicable |
| Yes | No | No | Demographic |
| No | No | Yes | Felony status |
| No | No | No | Mixed |

reasons. First, each sample targets a distinct population—two of which (Felony and Matched) are subpopulations of the third (CES). As each sample may have distinct characteristics and slightly different data collection methodologies, separate models ensure that any design-specific characteristics are treated and confined within each sample's model. Moreover, separate models allow for between-sample comparison without the potential confounding effects of pooling different characteristics, ensuring that any design-specific characteristics are treated and confined within each sample's model. This provides for a cleaner, more attuned analysis and interpretation. Although pooled models would provide a more direct statistical comparison than separate models, pooled models would also make interpretation more complex.

We present findings from three primary analyses. In the first analysis, using logit models, we estimate and compare voter turnout in the 2022 midterm election between the three samples. The outcome is a self-reported dichotomous measure (0 = did not vote, 1 = voted), though we exclude from this analysis respondents in any sample who self-reported they were ineligible to vote. Respondents may be ineligible because of felony conviction, lack of US citizenship, or some other reason. To be sure, midterm elections differ from presidential elections in that they feature lower average turnout and are influenced by a somewhat different set of factors, including mid-term presidential assessments, reactions to the prior presidential election, and—in many states—statewide elections for state and local offices and (e.g., Tufte, 1975; Jackson, 2000).

In addition to political behavior, we also measure partisanship and prior candidate choice. Thus, in our second analysis, we use OLS regression models to estimate self-identified political party on a seven-point scale (1 = Strong Republican and 7= Strong Democrat). In the last analysis, we use a logit model to estimate support for Joe Biden in a two-candidate race (Trump = 0, Biden = 1) in the 2020 presidential election. This measure includes those who reported voting for either Trump or Biden for president in 2020. We exclude from this third analysis those who did not vote (by choice or because they were ineligible), those who did not report supporting either Biden or Trump in 2020, and those who supported a third party or write-in candidate.

### *Additional Covariates*

Several robust predictors of partisanship and political participation have been consistently identified in the literature on political behavior. These predictors influence an individual's likelihood of identifying with a political party and their propensity to engage in the political process through voting and other forms of participation. Therefore, in the full models for our analyses, we include several demographic and context characteristics, including race/ethnicity, gender (using an inclusive measure that includes transgender people who indicated a male or female gender identity), age, education, family income, region, the state-level voting-eligible population (VEP) turnout rate, and a dichotomous measure of whether the respondent identified as "Born Again."

We also incorporate measures at the congressional district level. District density for each respondent is an ordinal variable based on the Congressional District Dashboard (Dashboard Team, 2024), which includes "Pure Rural," "Rural-Suburban Mix," "Sparse Suburban," "Dense Suburban," "Urban-Suburban Mix," and "Pure Urban." As a measure of local district partisanship, we use the Cook Partisan Voting Index (PVI), a calculated measure of a district's partisan lean in presidential elections (Wasserman, 2022). For our purposes, the PVI is centered at zero, and a positive number represents a Democratic lean and a negative number represents a Republican lean (compared to the national average).

To allow for complete case analysis, we limit our samples to those respondents who do not have missing data on the key outcome variables, independent variables, and relevant covariates, producing three analytic samples: Felony ($n = 905$), Matched ($n = 954$), and CES ($n = 46{,}019$). Weighted descriptive statistics for our dependent variables and covariates for each sample are provided in Table 2. We also include two columns that split Felony sample respondents into "Enfranchised" and "Disenfranchised" subgroups (eligibility inferred from self-reported ineligibility)[1] to allow for side-by-side comparisons across eligibility status. Given the small disenfranchized subgroup, we suggest that differences should be interpreted cautiously. By design, our Felony sample and matched sample are generally comparable across covariates. However, as would be expected based on the demographic distribution of people convicted of felonies (see, e.g., Shannon et al. 2017), both the Felony and Matched samples are more racially diverse, more male, and less educated than the CES sample. Moreover, the average Cook PVI lean, a district-level measure of partisanship, is close to zero for the CES sample, whereas the Felony and Matched samples lean $D + 2.3$ and $D + 3$, respectively.

## Turnout, Partisanship, and 2020 Support

We first focus on whether people with felony records are less likely to vote than similarly situated people and the general adult sample. Before moving into the results, we must first address the issue of over-reporting. Over-reporting of voter turnout has been documented in many surveys of voting behavior, particularly among highly educated respondents (Ansolabehere & Hersh, 2012). As a result, we would expect larger over-reporting by the CES sample as it features higher average educational attainment (36% with bachelor's degrees or higher) than the Felony or Matched samples (14% and 16%, respectively). With that caveat in mind, our focus is not on the overall levels of turnout but rather on any gaps between samples.

In Table 3, we present weighted 95% confidence intervals for voter turnout across the Felony, Matched, and CES samples. These intervals represent the degree of uncertainty around the estimated turnout rates, showing the degree of overlap or distinctiveness between these groups without implying a false sense of precision. Respondents who indicated they were ineligible to vote, either because of their citizenship status,

[1]We did not directly ask respondents whether they were disenfranchised for two reasons. First, disenfranchisement policies can be confusing, and–based on our experience–there is a considerable amount of misinformation regarding eligibility among not only people subject to these policies but also criminal legal actors and election officials. Thus, any responses to inquiries about disenfranchisement status may be misleading. Second, after weighing the potential value added by such a question against its potential stigmatizing impact (and how that could affect how people responded throughout the survey), we decided that the limited benefit would not outweigh the potential costs.

**Table 2.** Sample descriptives (weighted).

| | Felony Sample | | | Matched | |
|---|---|---|---|---|---|
| | *Total* | *Eligible*[*] | *Ineligible*[*] | Sample | CES sample |
| | ($n = 905$) | ($n = 820$) | ($n = 85$) | ($n = 954$) | ($n = 46{,}019$) |
| *Race* | | | | | |
| White | 0.569 | 0.562 | 0.625 | 0.577 | 0.698 |
| Black | 0.172 | 0.182 | 0.093 | 0.161 | 0.128 |
| Hispanic | 0.197 | 0.190 | 0.254 | 0.195 | 0.089 |
| Other/Unk | 0.062 | 0.066 | 0.027 | 0.067 | 0.084 |
| *Gender Identity* | | | | | |
| Male | 0.667 | 0.664 | 0.692 | 0.649 | 0.483 |
| Female | 0.333 | 0.336 | 0.308 | 0.351 | 0.517 |
| *Age* | | | | | |
| 18–29 | 0.106 | 0.095 | 0.194 | 0.118 | 0.166 |
| 30–44 | 0.344 | 0.340 | 0.378 | 0.330 | 0.251 |
| 45–64 | 0.405 | 0.415 | 0.324 | 0.410 | 0.335 |
| 65+ | 0.145 | 0.150 | 0.104 | 0.142 | 0.248 |
| *Education* | | | | | |
| HS or below | 0.524 | 0.517 | 0.583 | 0.523 | 0.358 |
| some college | 0.336 | 0.338 | 0.316 | 0.315 | 0.287 |
| Bachelor's or higher | 0.141 | 0.145 | 0.101 | 0.161 | 0.355 |
| *Family Income* | | | | | |
| <30,000 | 0.419 | 0.405 | 0.539 | 0.341 | 0.266 |
| 30,000 ~ 60,000 | 0.261 | 0.261 | 0.261 | 0.290 | 0.277 |
| 60,000 ~ 100,000 | 0.173 | 0.180 | 0.115 | 0.203 | 0.229 |
| >100,000 | 0.148 | 0.155 | 0.085 | 0.166 | 0.228 |
| *Pew Born Again* | 0.410 | 0.417 | 0.350 | 0.312 | 0.331 |
| *Density* | | | | | |
| Pure rural | 0.160 | 0.154 | 0.212 | 0.179 | 0.197 |
| Rural-suburban mix | 0.225 | 0.214 | 0.324 | 0.210 | 0.242 |
| Sparse suburban | 0.202 | 0.207 | 0.154 | 0.166 | 0.222 |
| Dense suburban | 0.169 | 0.171 | 0.145 | 0.208 | 0.158 |
| Urban-suburban mix | 0.123 | 0.122 | 0.135 | 0.129 | 0.118 |
| Pure urban | 0.121 | 0.132 | 0.030 | 0.110 | 0.063 |
| *Region* | | | | | |
| Northeast | 0.164 | 0.180 | 0.031 | 0.168 | 0.178 |
| Midwest | 0.193 | 0.190 | 0.225 | 0.181 | 0.213 |
| South | 0.375 | 0.366 | 0.446 | 0.352 | 0.383 |
| West | 0.268 | 0.264 | 0.298 | 0.299 | 0.226 |
| *Vep Turnout* | 0.469 | 0.468 | 0.472 | 0.472 | 0.474 |
| *Cook PVI* | 2.261 | 2.872 | −3.014 | 3.032 | −0.395 |

[*]The Eligible and Ineligible Felony sample categories reflect inferred eligibility status as we did not directly ask respondents about their status. We provide descriptive statistics for these subcategories to provide a comparison within the Felony Sample, but we caution the reader not to read too much into this inferred identification.

**Table 3.** Self-reported voting behavior, 2022.[*]

| | Felony Sample ($n = 905$) | | Matched Sample ($n = 954$) | | CES sample ($n = 46{,}019$) | |
|---|---|---|---|---|---|---|
| | Pct. | [95% CI] | Pct. | [95% CI] | Pct. | [95% CI] |
| Voted | 59.8 | [55.9, 63.7] | 76.0 | [71.6, 80.3] | 77.7 | [76.9, 78.5] |
| Did Not Vote | 29.8 | [26.2, 33.5] | 20.6 | [16.5, 24.7] | 20.3 | [19.6, 21.0] |
| Ineligible to Vote | 10.4 | [07.9, 12.9] | 03.4 | [01.5, 05.4] | 02.0 | [01.7, 02.3] |

[*]Estimates are weighted; confidence intervals account for the complex survey design.

felon status, age, or something else, are identified as ineligible. Relative to the Matched and CES samples, Felony sample participants were much less likely to report having voted in 2022. As we would expect, those in the Felony sample reported significantly higher levels of voter ineligibility (from 8% to 13%) compared to the Matched and CES samples. Of those who did not self-report they were ineligible (Table 4), Felony

**Table 4.** Self-reported voting behavior of eligible respondents, 2022.*

| | Felony Sample ($n = 820$) | | Matched Sample ($n = 931$) | | CES sample ($n = 45,734$) | |
|---|---|---|---|---|---|---|
| | Pct. | [95% CI] | Pct. | [95% CI] | Pct. | [95% CI] |
| Voted | 66.7 | [62.8, 70.7] | 78.7 | [74.5, 82.9] | 79.3 | [78.5, 80.0] |
| Did Not Vote | 33.3 | [29.3, 37.2] | 21.3 | [17.1, 25.5] | 20.7 | [20.0, 21.5] |

*Estimates are weighted; confidence intervals account for the complex survey design.

sample respondents were significantly less likely to report having voted in the 2022 midterm election (ranging from 63 to 71%) than those who are demographically similar and the general adult population (ranging from 75 to 83% and 79 to 80%, respectively).

Table 5 shows the logistic regression coefficients and standard errors for models estimating the probability of self-reported voting in the 2022 midterm election among those who did not identify as ineligible. For each sample, model 1 only includes race/ethnicity, and model 2 includes additional demographics and geographic contextual variables. In both the Felony and matched samples, race and ethnicity are not statistically significant predictors of turnout even after including additional covariates across the models. In contrast, among CES respondents, those self-identifying as Black, Hispanic, or some other race were less likely to vote relative to Whites.

When we include additional demographic and contextual variables in model 2, we generally find that age and education in all three samples and income in the Felony and CES samples are positively associated with self-reported turnout. We also find a negative association between female and turnout for the Matched and CES samples but not for the Felony sample. The Felony and Matched samples show less variability in turnout across demographic predictors compared to the CES sample, which may suggest that demographics play a comparable role for these two groups. Although age and education influence turnout for both samples, however, the effects are generally smaller for the Felony sample compared to the Matched sample. Thus, these differences provide some evidence, albeit limited, that felon status may be suppressing political participation, or at least reducing the influence of the known predictors of turnout. A suppressive impact of this kind could stem from factors such as discrimination based on felon status, which disrupts employment and housing stability (e.g., Western, 2006), system avoidance (Brayne, 2014), legal cynicism (Sampson & Bartusch, 1998), or diminished political efficacy tied to conviction status (Weaver & Lerman, 2010).

To compare more directly across samples, Figure 1 depicts the predicted marginal probability of self-reported voting in the 2022 mid-term election, holding all other covariates at their means, for Black, White, and Hispanic respondents within each sample. The comparisons in Figure 1 reflect the model-adjusted predictions for each racial and ethnic category within each sample. Across all three groups, the Matched and CES samples exhibit the highest predicted probability of voting, although for the smaller samples of Black and Hispanic respondents, levels of predicted turnout were not statistically distinguishable between samples. For White respondents, however, the Felony sample level was significantly lower than the Matched and CES samples. We also estimated a pooled logistic model interacting sample and race to test for race-specific differences in turnout across samples. The interaction terms were not statistically significant (adj. Wald $F(6, 47,477)=0.46$, $p = 0.837$), indicating

**Table 5.** Logistic regression models of self-reported voting behavior (of those eligible).

| | Felony Sample ($n = 820$) | | | | Matched Sample ($n = 931$) | | | | CES Sample ($n = 45{,}734$) | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Model 1 | | Model 2 | | Model 1 | | Model 2 | | Model 1 | | Model 2 | |
| | Coef. | (SE) | Coef. | (SE) | Coef. | (SE) | Coef. | (SE) | Coef. | (SE) | Coef. | (SE) |
| *Race: (ref = White)* | | | | | | | | | | | | |
| Black | −0.07 | (0.21) | 0.11 | (0.24) | −0.37 | (0.34) | −0.07 | (0.44) | −0.57*** | (0.07) | −0.20* | (0.09) |
| Hispanic | −0.42 | (0.31) | −0.20 | (0.32) | −0.57 | (0.38) | −0.26 | (0.41) | −0.85*** | (0.08) | −0.44*** | (0.10) |
| Other/Unk | −0.44 | (0.29) | −0.40 | (0.33) | −0.96 | (0.54) | −0.82 | (0.57) | −0.39*** | (0.08) | −0.38*** | (0.10) |
| *Sex: (ref = Male)* | | | | | | | | | | | | |
| Female | | | −0.31 | (0.19) | | | −0.83** | (0.31) | | | −0.17** | (0.05) |
| *Age: (ref = 18–29)* | | | | | | | | | | | | |
| 30–44 | | | 0.37 | (0.37) | | | −0.02 | (0.38) | | | 0.37*** | (0.08) |
| 45–64 | | | 0.76* | (0.38) | | | 0.98* | (0.42) | | | 0.92*** | (0.08) |
| 65+ | | | 1.64*** | (0.43) | | | 2.75*** | (0.82) | | | 1.61*** | (0.09) |
| *Education: (ref = HS or less)* | | | | | | | | | | | | |
| Some college | | | 0.58** | (0.20) | | | 0.98** | (0.30) | | | 0.61*** | (0.06) |
| Bachelor's or Higher | | | 1.51*** | (0.35) | | | 1.85*** | (0.42) | | | 1.14*** | (0.08) |
| *Income: (ref= <\$30K)* | | | | | | | | | | | | |
| 30,000 ~ 60,000 | | | 0.78** | (0.24) | | | −0.01 | (0.32) | | | 0.66*** | (0.06) |
| 60,000 ~ 100,000 | | | 1.21*** | (0.31) | | | 0.66 | (0.43) | | | 1.12*** | (0.08) |
| >100,000 | | | 0.95* | (0.37) | | | 1.03 | (0.55) | | | 1.41*** | (0.10) |
| *Pew Born Again* | | | 0.27 | (0.19) | | | 0.92** | (0.32) | | | 0.23*** | (0.06) |
| *Density: (ref = Rural)* | | | | | | | | | | | | |
| Rural-suburban mix | | | 0.35 | (0.28) | | | 0.27 | (0.41) | | | 0.15 | (0.08) |
| Sparse suburban | | | 0.24 | (0.33) | | | 0.59 | (0.45) | | | 0.17* | (0.09) |
| Dense suburban | | | 0.41 | (0.40) | | | 0.52 | (0.53) | | | −0.02 | (0.11) |
| Urban-suburban mix | | | 0.31 | (0.48) | | | −0.01 | (0.58) | | | 0.08 | (0.13) |
| Pure urban | | | −0.07 | (0.64) | | | 1.10 | (0.85) | | | 0.26 | (0.17) |
| *Region: (ref = North~)* | | | | | | | | | | | | |
| Midwest | | | −0.32 | (0.31) | | | 0.79 | (0.51) | | | 0.11 | (0.09) |
| South | | | −0.43 | (0.32) | | | −0.20 | (0.49) | | | 0.16* | (0.08) |
| West | | | 0.09 | (0.34) | | | 0.17 | (0.47) | | | 0.18 | (0.09) |
| *Vep Turnout* | | | 0.01 | (0.02) | | | 0.02 | (0.02) | | | 0.03*** | (0.00) |
| *Cook PVI* | | | 0.01 | (0.01) | | | −0.01 | (0.01) | | | 0.01* | (0.00) |
| *Constant* | 0.82*** | (0.10) | −1.10 | (1.00) | 1.56*** | (0.13) | −1.27 | (1.19) | 1.55*** | (0.03) | −2.16*** | (0.25) |

*$p < 0.05$.
**$p < 0.01$.
***$p < 0.001$.

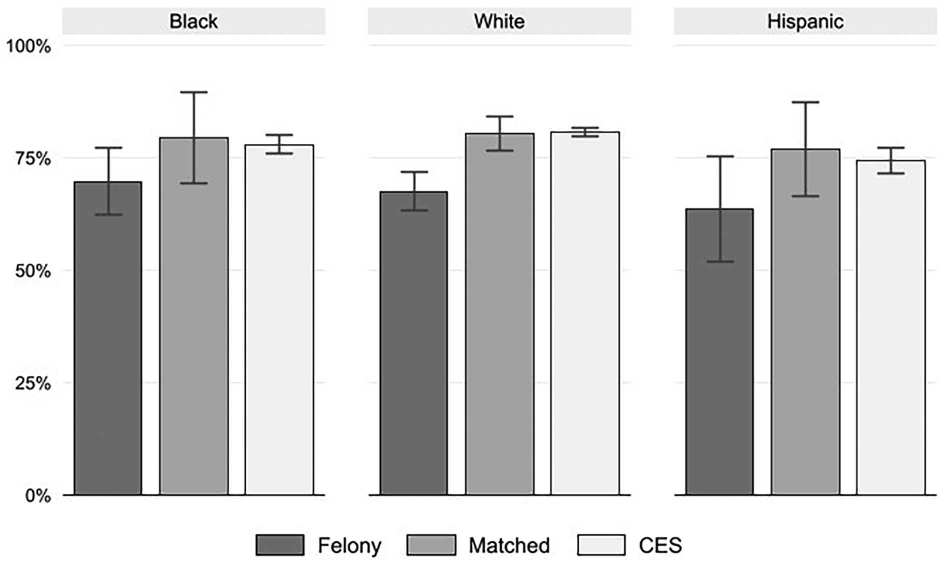


**Figure 1.** Predicted 2022 voter turnout by race and ethnicity across samples (adjusted for demographics).

that the turnout gap between those with and without felony records is similar for Black, White, Hispanic, and other respondents (see Appendix Table A1). Taken together, the results provide mixed support for prior findings that individuals with felony records are less likely to (self-report) turnout compared to the general population. Overall, people with felony records were less likely to report having voted in the 2022 midterm elections. Notably, Figure 1 suggests the overall difference may be driven by lower turnout among White people with felony records, although a similar pattern is observed in the smaller Black and Hispanic samples as well. Further, although we find some evidence that felon status contributes to lower levels of turnout, particularly for White respondents, demographic variables such as age, race, and sex exert significant influence on voting behavior within all three samples

### *Partisan Self-Identification*

Turning to our second research question, we next examine partisan self-identification. Table 6 shows the distribution of partisanship across samples. These descriptive results show similar patterns of partisan self-identification across the three samples, with some notable differences. For example, the Felony and Matched samples appear less likely to identify as "Strong Republicans" than the CES but more likely to identify as "Strong Democrats," providing descriptive support for the claim that partisan differences may be explained in part by differences in demographic makeup. As noted above, the Felony and Matched samples are more racially diverse, less educated, and are less economically advantaged than the general population CES sample. Almost one in five respondents in the Felony sample (19.6%) identified as Independents, relative to about one in six in both the Matched (16.2%) and CES (15.3%) samples.

**Table 6.** Descriptive partisan self-identification.

| | Felony Sample ($n = 905$) | | Matched Sample ($n = 954$) | | CES sample ($n = 46,019$) | |
|---|---|---|---|---|---|---|
| | Pct. | [95% CI] | Pct. | [95% CI] | Pct. | [95% CI] |
| Strong Republican | 14.6 | [12.0, 17.3] | 16.6 | [13.1, 20.0] | 19.3 | [18.7, 19.9] |
| Not very strong Republican | 09.3 | [07.3, 11.2] | 8.4 | [06.2, 10.7] | 11.2 | [10.7, 11.7] |
| Lean Republican | 08.0 | [05.7, 10.3] | 7.4 | [05.2, 09.6] | 9.7 | [09.2, 10.2] |
| Independent | 19.6 | [16.3, 23.0] | 16.2 | [12.9, 19.4] | 15.3 | [14.7, 15.9] |
| Lean Democrat | 08.1 | [06.3, 10.0] | 9.8 | [07.0, 12.6] | 8.7 | [08.3, 09.2] |
| Not very strong Democrat | 12.7 | [10.0, 15.4] | 13.6 | [10.2, 17.0] | 12.1 | [11.5, 12.6] |
| Strong Democrat | 27.6 | [24.2, 31.1] | 28.1 | [23.7, 32.4] | 23.7 | [23.1, 24.3] |

Table 7 presents the results of linear regression models predicting partisanship (1 = "Strong Republican" and 7 = "Strong Democrat"). Consistent with prior research, Black respondents, especially in the CES sample, and those with post-secondary education were consistently more likely to identify as Democrats across all samples. However, these characteristics are less predictive in the Felony sample compared to the Matched and CES samples. Moreover, income and a "born again" religious identity were negatively associated with Democratic self-identification in the Matched and CES samples but less predictive in the Felony sample. This pattern suggests that although demographics maintain some influence, felon status may moderate the influence of traditional demographic factors on partisanship. As would be expected, the local congressional district partisanship measure, Cook Partisan Voting Index (in which negative indicates Republican lean, positive indicates Democratic lean), is positively associated with Democratic self-identification net of other covariates. This indicates a significant association between the local political context and the strength of partisan self-identification. When we compare predicted partisan identification for the reference group (represented by the constant), we find a slight Republican lean (3.57) compared to a stronger Democratic lean in the demographically Matched sample (5.03) and the general population CES sample (4.62). Thus, net of other demographics and contextual variables, felon status may have a moderating effect on partisan identification.

A pooled regression also indicates a significant moderating effect of race on party identification ($F(6, 47,870) = 3.51$, $p = 0.002$) (Table A2). In particular, the Black–White gap in Democratic identification is narrower in the Felony sample than in the general population, consistent with the negative Felony × Black interaction coefficient (Table A2a–A2c). In the Felony sample, White respondents' mean partisanship is approximately 4.42, compared to 3.84 in the CES sample (on the 7-point scale, higher = more Democratic). Black respondents in the Felony sample average 5.28 vs 5.53 in CES. Thus, the Black–White gap is ~0.86 in the Felony group versus ~1.69 in CES. White respondents with felony records tend to identify more Democratic than White respondents in the general population, whereas Black respondents with felonies identify slightly less Democratic than Black respondents in the general population (though the difference was not statistically significant), resulting in a narrowed Black–White partisan gap in the Felony sample. In short, people with felony records show a somewhat smaller racial gap in partisanship.

To further investigate the relationship between race, local political context, and partisan self-identification, Figure 2 compares model-adjusted predictions of partisan self-identification for Black and White respondents for each sample, holding all other

**Table 7.** Linear regression models of self-reported partisanship.

| | Felony Sample ($n = 905$) | | | | Matched Sample ($n = 954$) | | | | CES Sample ($n = 46{,}019$) | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Model 1 | | Model 2 | | Model 1 | | Model 2 | | Model 1 | | Model 2 | |
| | Coef. | (SE) | Coef. | (SE) | Coef. | (SE) | Coef. | (SE) | Coef. | (SE) | Coef. | (SE) |
| *Race: (ref = White)* | | | | | | | | | | | | |
| Black | 1.05*** | (0.20) | 0.81*** | (0.21) | 1.15*** | (0.26) | 0.94*** | (0.28) | 1.71*** | (0.05) | 1.70*** | (0.06) |
| Hispanic | 0.42 | (0.27) | 0.44 | (0.26) | 0.17 | (0.37) | 0.19 | (0.29) | 0.80*** | (0.07) | 0.57*** | (0.07) |
| Other/Unk | 0.19 | (0.27) | 0.17 | (0.27) | 0.32 | (0.45) | 0.13 | (0.41) | 0.76*** | (0.06) | 0.37*** | (0.06) |
| *Sex: (ref = Male)* | | | | | | | | | | | | |
| Female | | | 0.20 | (0.15) | | | | 0.25 | (0.19) | | 0.31*** | (0.03) |
| *Age: (ref = 18–29)* | | | | | | | | | | | | |
| 30–44 | | | −0.06 | (0.36) | | | | −0.06 | (0.33) | | −0.14* | (0.06) |
| 45–64 | | | 0.12 | (0.34) | | | | −0.45 | (0.34) | | −0.32*** | (0.06) |
| 65+ | | | 0.17 | (0.37) | | | | 0.06 | (0.38) | | −0.41*** | (0.06) |
| *Education: (ref = HS or less)* | | | | | | | | | | | | |
| Some college | | | −0.07 | (0.17) | | | | 0.48* | (0.19) | | 0.13** | (0.04) |
| Bachelor's or Higher | | | 0.71** | (0.24) | | | | 0.88*** | (0.22) | | 0.51*** | (0.04) |
| *Income: (ref= <$30K)* | | | | | | | | | | | | |
| 30,000 ~ 60,000 | | | −0.28 | (0.18) | | | | −0.80*** | (0.24) | | −0.20*** | (0.05) |
| 60,000 ~ 100,000 | | | 0.31 | (0.24) | | | | −0.61* | (0.25) | | −0.23*** | (0.05) |
| >100,000 | | | −0.32 | (0.30) | | | | −0.42 | (0.28) | | −0.17** | (0.05) |
| *Pew Born Again* | | | −0.18 | (0.17) | | | | −0.64** | (0.21) | | −1.12*** | (0.04) |
| *Density: (ref = Rural)* | | | | | | | | | | | | |
| Rural-suburban mix | | | −0.29 | (0.24) | | | | 0.08 | (0.28) | | −0.12* | (0.05) |
| Sparse suburban | | | 0.22 | (0.28) | | | | −0.30 | (0.31) | | −0.21*** | (0.05) |
| Dense suburban | | | −0.19 | (0.33) | | | | 0.13 | (0.39) | | −0.14* | (0.07) |
| Urban-suburban mix | | | −0.05 | (0.43) | | | | −0.50 | (0.50) | | −0.29*** | (0.08) |
| Pure urban | | | 0.20 | (0.49) | | | | 0.53 | (0.61) | | −0.23* | (0.11) |
| *Region: (ref = North~)* | | | | | | | | | | | | |
| Midwest | | | 0.45 | (0.25) | | | | −0.40 | (0.29) | | 0.02 | (0.05) |
| South | | | 0.26 | (0.25) | | | | −0.60* | (0.29) | | −0.17*** | (0.05) |
| West | | | 0.18 | (0.26) | | | | −0.87** | (0.29) | | 0.09 | (0.05) |
| *Vep Turnout* | | | 0.01 | (0.01) | | | | 0.00 | (0.02) | | 0.00 | 0.00 |
| *Cook PVI* | | | 0.02** | (0.01) | | | | 0.02* | (0.01) | | 0.03*** | 0.00 |
| *Constant* | 4.19*** | (0.10) | 3.57*** | (0.75) | 4.23*** | (0.10) | 5.03*** | (0.93) | 3.79*** | (0.02) | 4.62*** | (0.15) |

*$p < 0.05$.
**$p < 0.01$.
***$p < 0.001$.

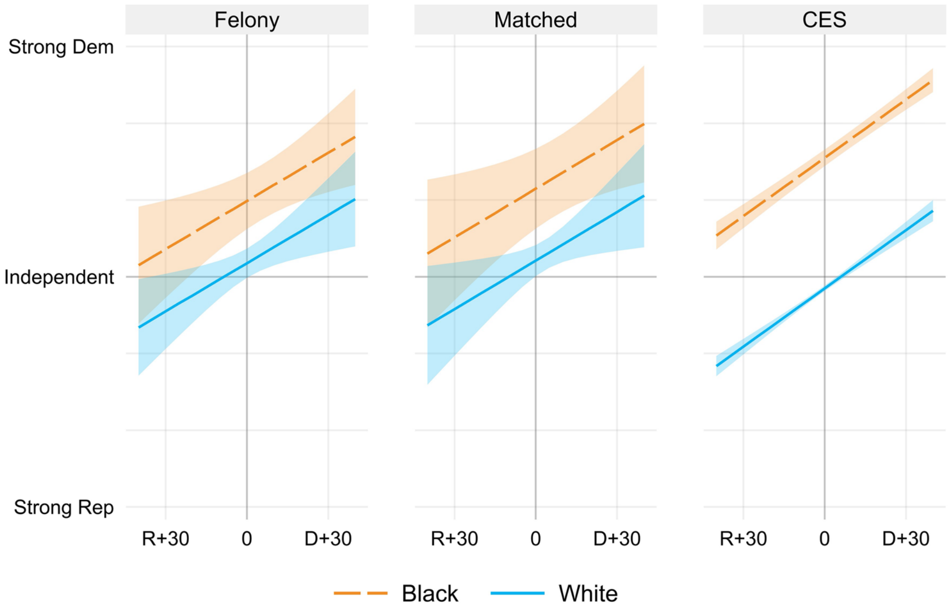


**Figure 2.** Predicted partisan self-identification for black and white within samples (adjusted for demographics).

covariates at their means (we limit to Black and White as three groups are hard to distinguish). The y-axis represents the seven-point partisanship scale, ranging from 1 ("Strong Republican") to 7 ("Strong Democrat"), and the x-axis represents the Cook PVI for the respondents' congressional district (ranging from $R+40$ to $D+40$). In all three samples, the local political context moderates political self-identification. Although Black respondents were consistently more likely to identify as Democrats, the predicted margins in heavily Republican-leaning districts begin close to the ("Independent") middle. White respondents, on the other hand, were more likely to identify as Republicans in Republican-leaning districts and as Democrats in Democratic-leaning districts, though the CES slope had a slight Republican lean at $D+0$, whereas the Felony and Matched White slopes had a slight Democratic lean at $D+0$ (as indicated by the position of the predicted lines relative to the "Independent" marker on the y axis).

The CES sample also features greater Black-White polarization. We find both a larger gap between the Black and White slopes in the CES sample and slightly steeper slopes compared to the Felony and Matched samples. At similar district partisanship levels, Black and White CES respondents are more left-leaning and right-leaning, respectively, than their counterparts in the Felony and Matched sample. This suggests that socioeconomic factors may dampen partisan dynamics seen in the broader general population, but, contrary to popular assumption, felon status does not appear to substantially affect partisan self-identification.

### *Biden 2020 Support*

Prior vote recall is an imperfect measure, especially for irregular voters, but we include analysis of the 2020 election as a concrete example and a complement

**Table 8.** Presidential vote choice of 2020 voters.*

| | Felony Sample ($n = 630$) | | Matched Sample ($n = 809$) | | CES sample ($n = 40{,}178$) | |
|---|---|---|---|---|---|---|
| | Pct. | [95% CI] | Pct. | [95% CI] | Pct. | [95% CI] |
| Joe Biden | 33.8 | [29.5, 38.1] | 36.0 | [31.3, 40.6] | 45.5 | [44.7, 46.2] |
| Donald Trump | 63.3 | [58.9, 67.7] | 61.5 | [56.8, 66.3] | 52.1 | [51.3, 52.8] |
| Third Party | 02.9 | [01.6, 04.3] | 02.5 | [01.4, 03.6] | 02.5 | [02.2, 02.7] |

*Estimates are weighted; confidence intervals account for the complex survey design.

to the participation and partisanship analysis. In Table 8, we compare the 2020 candidate choice among those who indicated they voted in that election as weighted 95% confidence intervals. We find very similar levels of support for Joe Biden for president in 2020 among respondents in the Felony and Matched samples, which each exceed the narrow level of Biden preference reported in the CES sample.

In Table 9, we present the results of logit models predicting 2020 candidate support, limiting our analysis to two candidates: Donald Trump (0) and Joe Biden (1). Across models and samples, we find substantially stronger support for Biden in 2020 among Black respondents compared to White respondents and weaker support among those who identify as "born again," net of other factors. However, the magnitude of these coefficients is largest among the CES sample respondents and smallest among the Felony sample respondents. Further, in model 2, we find bachelor's degree or higher educational attainment, female, and Cook PVI are positively associated with Biden support and greater income levels relative to the reference group are negatively associated in only the Matched and CES samples. In the Felony sample, in contrast, neither sex nor education nor income are significant predictors of 2020 Biden preference, net of the other predictors.

We compare model-adjusted predictions for Biden support in Figure 3 for Black and White respondents across the Cook PVI range, adjusted for covariates. Across the models, the predicted margins for Black respondents increase curvilinearly toward Biden and begin to flatten as districts become more Democratic-leaning. The predicted margins for White take a much more linear and steeper shape, suggesting that local political context plays a stronger role in shaping White voter behavior in the general population. Because Black support for Biden starts and remains high across local context, however, the latter result may simply reflect a ceiling effect. The more gradual slope for White respondents in the Felony and Matched samples may suggest that other factors, such as felon status or socioeconomic disengagement, may dampen the influence of local partisanship—including, perhaps, more constrained housing options than those represented in the broader CES sample. We also see polarization analogous to that of Figure 2.

We tested whether sample differences in 2020 vote choice vary by race using a pooled logit model. The sample × race interaction terms were jointly insignificant ($F(6, 40{,}188) = 1.27$, $p = 0.268$), confirming that the pattern of Biden 2020 support is consistent across racial groups in our samples (see Table A3). Although Black respondents in all samples were far more likely to support Biden than White respondents, the between-sample differences in this Black–White gap were not statistically significant.

**Table 9.** Logit regression models of Biden 2020 support.

| | Felony Sample ($n = 610$) | | | | Matched Sample ($n = 782$) | | | | CES Sample ($n = 38,804$) | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Model 1 | | Model 2 | | Model 1 | | Model 2 | | Model 1 | | Model 2 | |
| | Coef. | (SE) | Coef. | (SE) | Coef. | (SE) | Coef. | (SE) | Coef. | (SE) | Coef. | (SE) |
| *Race: (ref = White)* | | | | | | | | | | | | |
| Black | 1.74*** | (0.34) | 1.63*** | (0.41) | 1.86*** | (0.38) | 2.08*** | (0.46) | 2.15*** | (0.09) | 2.58*** | (0.11) |
| Hispanic | 0.62 | (0.37) | 0.61 | (0.35) | 0.24 | (0.38) | 0.33 | (0.34) | 0.91*** | (0.09) | 0.80*** | (0.09) |
| Other/Unk | 0.03 | (0.36) | −0.06 | (0.39) | 0.70 | (0.55) | 0.76 | (0.51) | 0.71*** | (0.08) | 0.35*** | (0.08) |
| *Sex: (ref = Male)* | | | | | | | | | | | | |
| Female | | | 0.12 | (0.21) | | | 0.62** | (0.23) | | | 0.44*** | (0.04) |
| *Age: (ref = 18–29)* | | | | | | | | | | | | |
| 30–44 | | | −1.08 | (0.59) | | | 0.42 | (0.49) | | | −0.11 | (0.09) |
| 45–64 | | | −0.54 | (0.59) | | | −0.30 | (0.48) | | | −0.52*** | (0.08) |
| 65+ | | | −0.89 | (0.62) | | | 0.11 | (0.51) | | | −0.65*** | (0.08) |
| *Education: (ref = HS or less)* | | | | | | | | | | | | |
| Some college | | | 0.16 | (0.23) | | | 0.31 | (0.25) | | | 0.24*** | (0.05) |
| Bachelor's or Higher | | | 0.52 | (0.30) | | | 1.18*** | (0.30) | | | 0.73*** | (0.05) |
| *Income: (ref= <$30K)* | | | | | | | | | | | | |
| 30,000 ~ 60,000 | | | −0.51 | (0.27) | | | −0.95** | (0.31) | | | −0.30*** | (0.06) |
| 60,000 ~ 100,000 | | | 0.48 | (0.34) | | | −1.13*** | (0.32) | | | −0.39*** | (0.06) |
| >100,000 | | | 0.03 | (0.37) | | | −0.86* | (0.37) | | | −0.33*** | (0.06) |
| *Pew Born Again* | | | −0.62** | (0.22) | | | −1.24*** | (0.25) | | | −1.44*** | (0.05) |
| *Density: (ref = Rural)* | | | | | | | | | | | | |
| Rural-suburban mix | | | −0.57 | (0.33) | | | 0.36 | (0.31) | | | −0.06 | (0.06) |
| Sparse suburban | | | 0.62 | (0.37) | | | −0.30 | (0.34) | | | −0.19** | (0.06) |
| Dense suburban | | | −0.23 | (0.44) | | | 0.23 | (0.47) | | | −0.12 | (0.08) |
| Urban-suburban mix | | | 0.72 | (0.52) | | | −0.72 | (0.50) | | | −0.25** | (0.09) |
| Pure urban | | | 0.60 | (0.66) | | | 0.31 | (0.73) | | | −0.07 | (0.14) |
| *Region: (ref = North~)* | | | | | | | | | | | | |
| Midwest | | | 0.27 | (0.34) | | | −0.09 | (0.37) | | | 0.08 | (0.06) |
| South | | | 0.14 | (0.32) | | | −0.36 | (0.35) | | | −0.16** | (0.05) |
| West | | | 0.03 | (0.34) | | | −0.76 | (0.40) | | | 0.09 | (0.06) |
| *Vep Turnout* | | | 0.00 | (0.02) | | | −0.01 | (0.02) | | | −0.01 | (0.00) |
| *Cook PVI* | | | 0.02 | (0.01) | | | 0.04*** | (0.01) | | | 0.03*** | (0.00) |
| *Constant* | 0.27* | (0.11) | 0.88 | (1.21) | 0.23* | (0.10) | 1.33 | (1.01) | −0.19*** | (0.02) | 0.81*** | (0.19) |

*$p < 0.05$.
**$p < 0.01$.
***$p < 0.001$.

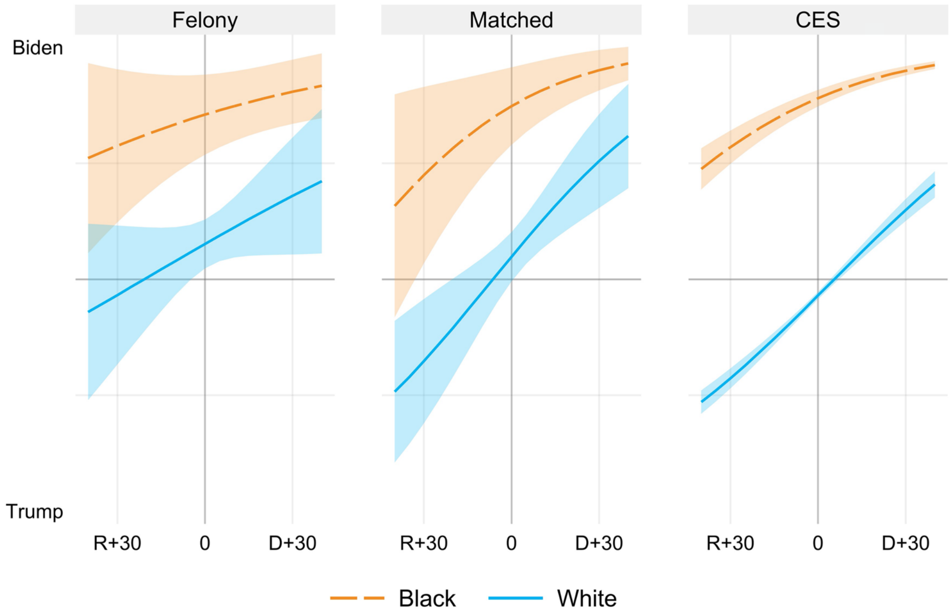


**Figure 3.** Predicted 2020 presidential candidate support for black and white within samples (adjusted for demographics).

## Discussion

This research examines whether felony status independently influences political engagement and party affiliation, or if observed political differences can be explained primarily through demographic factors. We compare survey results of a national sample of people with felony records, a demographically matched sample, and a general population sample. We find, first, that those with felony records report lower turnout in the 2022 midterm election compared to the general population, consistent with prior research (Burch, 2011; Meredith & Morse, 2015; Uggen et al., 2006). Both descriptive results and model-adjusted results indicate that people with felony records are simply less likely to vote, even after excluding those who report ineligibility. This finding remains robust when comparing the self-reported turnout among people with felony records to the demographically matched sample, indicating that felony status exerts influence in addition to demographic factors. Our turnout outcome in this study was on a midterm election; in a presidential election with higher turnout (and potentially different voter enthusiasm), the gap we observed might narrow or shift. Future research should consider whether the felony turnout gap persists in higher-salience elections. We also caution that we do not yet have data that would specify the underlying mechanisms for this finding. Because we evaluated self-reported turnout rather than actual turnout, it is also unclear how overreporting among those with felony records would compare to overreporting in the general population. Incorporating vote validation into the survey process, as is done with the CES, and questions tailored to the felony population about civic (dis)engagement, legal cynicism, and legal barriers would provide additional empirical leverage to more precisely estimate the gap in turnout and assess the potential mechanisms.

Second, contrary to the widespread assumption that restoring voting rights to people with felony records would overwhelmingly benefit the Democratic Party, we find that the felony population—as with any other large group of people with some shared characteristic—is complex. Although we do find that people with felony records are overall somewhat more likely to identify as Democrats compared to the general population, they exhibit a spectrum of partisan identities. Additionally, when comparing the Felony sample to the demographically Matched sample not screened for felony status, we find that felony status may be associated with a more centrist partisan identity, even when controlling for similar demographic factors. Relatedly, almost 20% of the Felony sample identified as politically independent, relative to about 16% of the matched sample and 15% of the CES sample. Fearing backlash (Mendelberg, 2001), candidates and parties rarely make direct campaign appeals to justice-impacted populations. Nevertheless, we find that the votes of people with felony records are demonstrably more likely to be "in play" than the votes of people without felony records.

We also find evidence that partisan identification is moderated by race and local context. For all respondents, the partisan lean of their district was significantly correlated with the direction and intensity of partisan self-identification. White respondents with felony records were more likely to identify as Republican in Republican-leaning districts and as Democrats in Democratic-leaning districts. Although Black respondents with felony records were more likely to identify as Democrats overall, their predicted partisan identification ranged from "Independent" or "Lean Democrat" in Republican districts to "Strong Democrats" in strong Democratic districts.

However, rather than having a partisan radicalizing effect, as some have suggested, felony status may have a neutralizing effect. Compared to the general population CES sample, we found less polarization between White and Black respondents in the Felony sample (we also found slightly less polarization in the Felony sample relative to the Matched sample, though the differences were not statistically significant). Consistent with these findings, we also see flatter trends regarding 2020 vote choice in the Felony sample compared to the Matched or CES sample. Thus, with respect to the question of which party benefits from felony disenfranchisement laws, we echo Traci Burch's response: "it depends" (Burch, 2012, p. 19).

Regarding generalizability, we offer a few caveats. First, we identified people with felony records through a screening process, but it is also unquestionably the case that criminal records generally are stigmatizing. Therefore, it is possible that those who acknowledged their felony record on a survey screening question may be qualitatively different than the general felony population. Second, YouGov is an online survey, people who are willing to disclose their criminal history in such a setting without direct communication with the researchers to build trust might generally have a higher trust in institutions than those who choose not to participate. This could potentially exclude more marginalized individuals who hold greater distrust toward institutions. However, because all three samples used in this study come from the same online survey platform, we expect that these biases would be present across all three datasets and is therefore less likely to meaningfully affect our comparative results. Third, YouGov uses an opt-in panel rather than a probability-based selection design, necessarily resulting in undercoverage. YouGov's sampling frame only includes people with a device that has access to the internet and who choose to join their panel. Thus, it does not include people without internet access, many of whom are socioeconomically disadvantaged and may disproportionately include members of the felony population. Although YouGov follows a rigorous process to create their synthetic sampling frames, no amount of modeling can account for people who simply are not reachable in the sample. That said, these surveys were all conducted by the same online platform within the same general time period using nearly the same methods and relevant questions. We therefore believe the comparisons we make have validity, although a future extension of this work should consider probability-based selection designs or panels.

We also invite survey researchers to more regularly include questions relating to criminal legal involvement and status in their survey instruments. Criminal legal involvement has become a common experience for Americans, and these experiences can significantly restructure political socialization, social networks, residential stability, and economic circumstances (Uggen & Stewart, 2014). To study this population, researchers have developed creative but often imprecise and limited approaches. For instance, many general surveys include individuals with felony convictions within their samples, but social scientists must rely on imperfect questions to identify the felon population for analysis. Including questions about specific forms of criminal legal experience will provide leverage to gain insights into the political, social, and economic behavior of the millions of Americans with criminal records.

## Conclusion

An estimated one in twelve American adults has a felony record, 90% of whom are not incarcerated but instead live in the community (Shannon et al., 2017). As the trend since 2016 has been toward reducing or eliminating disenfranchisement policies across several states, the felony population makes up an increasingly substantial proportion of the U.S. electorate (Uggen et al., 2024). Although this group has urgent needs for housing, employment, safety, and other priorities, political actors rarely campaign for their votes—perhaps because they assume few people with felonies would vote or that their votes are already locked up by the opposing candidate or party. Our findings invite a more nuanced approach to understanding the political identity and behavior of people with felony records.

Individuals with criminal records are indeed less likely to vote compared the general population. Our results align with prior studies, suggesting a potentially profound impact of criminal justice contact on civic engagement. Although individuals in this group do participate in elections—albeit at lower rates than the general population—they cannot be assumed to uniformly support left-leaning candidates or causes. Our findings indicate a modest Democratic lean amongst the felony population, which is largely what we might expect based on their demographic composition, though we find comparatively less partisan polarization than the general population. Thus, felony status may actually moderate partisanship and polarization, especially for White adults. Taken together, these findings suggest that the felony population is hardly monolithic but instead should be recognized as having a diverse range of political attitudes influenced by socioeconomic and contextual factors.

These insights carry important implications for ongoing efforts to restore voting rights to individuals with felony records. Historically, political parties have not had to make specific appeals to this group because of felony disenfranchisement. But as more states move to restore voting rights to more of their residents with felony records, political parties and policy advocates would benefit by taking this group and their views seriously. While our findings indicate that restoring voting rights may not dramatically shift the partisan balance, they highlight the broader democratic imperative of ensuring equal participation in the electoral process. Ultimately, reenfranchisement is less about altering political outcomes and more about affirming the fundamental democratic principle of inclusive citizenship.

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No potential conflict of interest was reported by the author(s).

## Human Subjects

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## Appendix A. Pooled sample interaction analysis

We estimated pooled survey-weighted models combining all three samples to test whether any effects of felon status on political outcomes are moderated by race (Black, Hispanic, Other vs. White). We included indicator variables for sample (Felony, Matched, CES) and interactions between sample and respondent race, while controlling for the same demographic and contextual covariates as in the main text analyses. Pooled models use original post-stratification weights.[2] This approach treats covariate effects as constant across samples, so we interpret these pooled results cautiously as a robustness check rather than as our primary specification.

Table A1 displays coefficients for a pooled logistic regression model of estimated self-reported 2022 turnout with sample and race indicators, sample × race interaction terms, and the same demographic and contextual covariates as in model 2 of Table 3. The F-statistic for the interac-

[2]Because the CES sample is considerably larger, we also estimated versions with within-sample normalized weights (each sample's weights sum to the same constant) to adjust for the influence of the CES. The Sample × Race joint Wald tests and the substantive patterns of adjusted means/contrasts were unchanged (see Table A4).

tion terms ($F(6, 47477)=0.46$, $p=0.837$) indicates that the turnout gap between those with and without felony records is similar for Black, White, Hispanic, and other respondents.

Table A2 shows coefficients for a pooled OLS regression model estimating self-reported partisanship. In a similar pattern to what we observed in Figure 2, White respondents with felony records tend to identify more Democratic than White respondents in the general population, whereas Black respondents with felonies identify slightly less Democratic than Black respondents in the general population, resulting in a narrowed Black–White partisan gap in the Felony sample. The adjusted Wald statistic ($F(6, 47{,}870)=3.51$, $p=0.002$) indicates that race gaps in party ID differ across samples. The pattern is driven mainly by a compression of the Black–White gap in the Felony sample (see Tables A2a–A2c).

Adjusted means in Table A2a show that White respondents in Felony and Matched identify more Democratic than White respondents in CES (+0.58 and +0.44 points, respectively), whereas Black respondents are very similar across samples. Hispanic respondents and Other/Unknown respondents are broadly comparable across samples. In Table A2b, we find that relative to CES, White respondents in Felony and Matched samples are substantially more Democratic (+0.59 and +0.45, both $p<0.001$). Among Black, Hispanic, and Other/Unknown respondents, between-sample differences are small and not significant. Thus, the significant interaction arises primarily because White respondents shift while Black respondents do not. In Table A2c, we find that the Black–White partisan gap is 0.84 points smaller in the Felony sample than in CES ($p<0.001$). The Matched–CES difference is directionally similar but not significant ($p=0.060$). Hispanic–White and Other–White gap differences are imprecise and non-significant. Together with Table A2b, these contrasts show that the Sample $\times$ Race moderation in party ID is concentrated in the narrowing of the Black–White gap in the Felony sample.

**Table A1.** Pooled logistic regression model of self-reported voting behavior (of those eligible).

| | Pooled Sample ($n = 47{,}485$) | |
|---|---|---|
| | Coef. | (SE) |
| *Sample: (ref = CES)* | | |
| Felony | −0.54*** | (0.12) |
| Matched | 0.28* | (0.14) |
| *Race: (ref = White)* | | |
| Black | −0.19* | (0.08) |
| Hispanic | −0.43*** | (0.10) |
| Other/Unk | −0.38*** | (0.10) |
| *Sample × Race*† | | |
| Felony × Black | 0.28 | (0.24) |
| Felony × Hispanic | 0.33 | (0.35) |
| Felony × Other/Unk | 0.00 | (0.33) |
| Matched × Black | 0.20 | (0.38) |
| Matched × Hispanic | 0.07 | (0.43) |
| Matched × Other/Unk | −0.37 | (0.54) |
| *Sex: (ref = Male)* | | |
| Female | −0.19*** | (0.05) |
| *Age: (ref = 18–29)* | | |
| 30–44 | 0.36*** | (0.07) |
| 45–64 | 0.91*** | (0.07) |
| 65+ | 1.62*** | (0.09) |
| *Education: (ref = HS or less)* | | |
| Some college | 0.62*** | (0.06) |
| Bachelor's or Higher | 1.15*** | (0.07) |
| *Income: (ref= <$30K)* | | |
| 30,000 ~ 60,000 | 0.65*** | (0.06) |
| 60,000 ~ 100,000 | 1.11*** | (0.07) |
| >100,000 | 1.39*** | (0.09) |
| *Pew Born Again* | 0.24*** | (0.06) |
| *Density: (ref = Rural)* | | |
| Rural-suburban mix | 0.15* | (0.07) |
| Sparse suburban | 0.18* | (0.08) |
| Dense suburban | 0.00 | (0.11) |
| Urban-suburban mix | 0.08 | (0.13) |
| Pure urban | 0.26 | (0.16) |
| *Region: (ref = North~)* | | |
| Midwest | 0.11 | (0.08) |
| South | 0.14 | (0.08) |
| West | 0.17 | (0.09) |
| *Vep Turnout* | 0.03*** | (0.00) |
| *Cook PVI* | 0.01* | (0.00) |
| *Constant* | −2.12*** | (0.25) |

Notes: Coefficients are log-odds; standard errors in parentheses. Survey-weighted (svy; linearized VCE).
*$p < 0.05$.
***$p < 0.001$.
†Adjusted Wald joint test that all Sample × Race interactions = 0: $F(6, 47{,}477) = 0.46$, $p = 0.837$.

**Table A2.** Pooled OLS regression model of self-reported partisanship.

| | Pooled Sample ($n = 47{,}485$) | |
|---|---|---|
| | Coef. | (SE) |
| *Sample: (ref = CES)* | | |
| Felony | 0.59*** | (0.10) |
| Matched | 0.45*** | (0.09) |
| *Race: (ref = White)* | | |
| Black | 1.69*** | (0.06) |
| Hispanic | 0.58*** | (0.07) |
| Other/Unk | 0.38*** | (0.06) |
| *Sample × Race*† | | |
| Felony × Black | −0.84*** | (0.21) |
| Felony × Hispanic | −0.43 | (0.30) |
| Felony × Other/Unk | −0.26 | (0.28) |
| Matched × Black | −0.53 | (0.28) |
| Matched × Hispanic | −0.51 | (0.36) |
| Matched × Other/Unk | −0.44 | (0.45) |
| *Sex: (ref = Male)* | | |
| Female | 0.30*** | (0.03) |
| *Age: (ref = 18–29)* | | |
| 30–44 | −0.14* | (0.06) |
| 45–64 | −0.32*** | (0.05) |
| 65+ | −0.40*** | (0.06) |
| *Education: (ref = HS or less)* | | |
| Some college | 0.13** | (0.04) |
| Bachelor's or Higher | 0.52*** | (0.04) |
| *Income: (ref= <\$30K)* | | |
| 30,000 ~ 60,000 | −0.21*** | (0.04) |
| 60,000 ~ 100,000 | −0.23*** | (0.05) |
| >100,000 | −0.17** | (0.05) |
| *Pew Born Again* | −1.09*** | (0.04) |
| *Density: (ref = Rural)* | | |
| Rural-suburban mix | −0.12* | (0.05) |
| Sparse suburban | −0.20*** | (0.05) |
| Dense suburban | −0.13* | (0.07) |
| Urban-suburban mix | −0.28*** | (0.08) |
| Pure urban | −0.20 | (0.10) |
| *Region: (ref = North~)* | | |
| Midwest | 0.01 | (0.05) |
| South | −0.17*** | (0.05) |
| West | 0.07 | (0.05) |
| *Vep Turnout* | 0.00 | (0.00) |
| *Cook PVI* | 0.03*** | (0.00) |
| *Constant* | 4.59*** | (0.15) |

Notes: OLS estimates on the 1–7 party-ID scale (higher = more Democratic); standard errors in parentheses. Survey-weighted (svy; linearized VCE).
*$p < 0.05$.
**$p < 0.01$.
***$p < 0.001$.
†Adjusted Wald joint test that all Sample × Race interactions = 0: $F(6, 47{,}870) = 3.51$, $p = 0.002$.

**Table A2a.** Adjusted mean party ID by sample × race (with 95% CIs).

| | Felony Sample ($n = 905$) | Matched Sample ($n = 954$) | CES sample ($n = 46{,}019$) |
|---|---|---|---|
| White | 4.42 [4.23, 4.61] | 4.28 [4.10, 4.46] | 3.84 [3.80, 3.87] |
| Black | 5.28 [4.91, 5.64] | 5.45 [4.93, 5.96] | 5.53 [5.42, 5.64] |
| Hispanic | 4.57 [4.04, 5.10] | 4.35 [3.69, 5.02] | 4.41 [4.28, 4.54] |
| Other/Unknown | 4.54 [4.04, 5.04] | 4.22 [3.37, 5.07] | 4.21 [4.09, 4.33] |

Notes: Values are adjusted means on the 1–7 scale; higher = more Democratic.

**Table A2b.** Pairwise sample differences within race (felon–CES; Matched–CES).

| | Felon - CES | | Matched - CES | |
|---|---|---|---|---|
| White | +0.586 *** | (0.098) | +0.449*** | (0.093) |
| Black | −0.254 | (0.192) | −0.083 | (0.267) |
| Hispanic | +0.156 | (0.280) | −0.057 | (0.344) |
| Other/Unknown | +0.327 | (0.263) | +0.009 | (0.440) |

Notes: Values are differences in adjusted means; SEs in parentheses. CES is the reference sample.

**Table A2c.** Difference-in-differences of race gaps across samples.

| | DiD | (SE) |
|---|---|---|
| Black–White, Felon vs CES | −0.840*** | (0.214) |
| Black–White, Matched vs CES | −0.531 | (0.283) |
| Hispanic–White, Felon vs CES | −0.430 | (0.296) |
| Hispanic–White, Matched vs CES | −0.506 | (0.356) |
| Other–White, Felon vs CES | −0.259 | (0.280) |
| Other–White, Matched vs CES | −0.440 | (0.449) |

Notes: DiD = $(\text{Race - White})_S$ - $(\text{Race - White})_{CES}$, where ($S \in$ {Felon,Matched}). $Y$ is the adjusted mean on the 1–7 party-ID scale (higher = more Democratic). Negative DiD = smaller race–White gap in $S$ than in CES.

**Table A3.** Pooled logit regression model of Biden 2020 support.

| | Pooled Sample ($n = 47,485$) | |
|---|---|---|
| | Coef. | (SE) |
| *Sample: (ref = CES)* | | |
| Felony | 0.79*** | (0.13) |
| Matched | 0.47*** | (0.11) |
| *Race: (ref = White)* | | |
| Black | 2.56*** | (0.11) |
| Hispanic | 0.80*** | (0.09) |
| Other/Unk | 0.35*** | (0.08) |
| *Sample × Race*[†] | | |
| Felony × Black | −0.94 | (0.41) |
| Felony × Hispanic | −0.31 | (0.47) |
| Felony × Other/Unk | −0.38 | (0.42) |
| Matched × Black | −0.30 | (0.43) |
| Matched × Hispanic | −0.51 | (0.42) |
| Matched × Other/Unk | 0.11 | (0.58) |
| *Sex: (ref = Male)* | | |
| Female | 0.43*** | (0.04) |
| *Age: (ref = 18–29)* | | |
| 30–44 | −0.12 | (0.08) |
| 45–64 | −0.53*** | (0.08) |
| 65+ | −0.65*** | (0.08) |
| *Education: (ref = HS or less)* | | |
| Some college | 0.23*** | (0.05) |
| Bachelor's or Higher | 0.73*** | (0.05) |
| *Income: (ref= <$30K)* | | |
| 30,000 ~ 60,000 | −0.32*** | (0.06) |
| 60,000 ~ 100,000 | −0.39*** | (0.06) |
| >100,000 | −0.33*** | (0.06) |
| *Pew Born Again* | −1.41*** | (0.05) |
| *Density: (ref = Rural)* | | |
| Rural-suburban mix | −0.06 | (0.05) |
| Sparse suburban | −0.18*** | (0.06) |
| Dense suburban | −0.11 | (0.08) |
| Urban-suburban mix | −0.24 | (0.09) |
| Pure urban | −0.03 | (0.14) |
| *Region: (ref = North~)* | | |
| Midwest | 0.08 | (0.05) |
| South | −0.16** | (0.05) |
| West | 0.06 | (0.06) |
| *Vep Turnout* | −0.01 | (0.00) |
| *Cook PVI* | 0.03*** | (0.00) |
| *Constant* | 0.80*** | (0.18) |

Notes: Coefficients are log-odds; standard errors in parentheses. Survey-weighted (svy; linearized VCE).
*$p < 0.05$.
**$p < 0.01$.
***$p < 0.001$.
[†]Adjusted Wald joint test that all Sample × Race interactions = 0: $F(6, 40,188) = 1.27$, $p = 0.268$.