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Abstract

The United States has more immigrants than at any time since the 1920s and immigration rates remain high. Past research unequivocally predicts that the resulting increase in ethnic and racial diversity will reduce local investments in public goods. By analyzing a new, comprehensive data set on U.S. cities from 1950 to 2002, this article challenges those predictions. In the 1950s and early 1960s, the percent Black had no strong impacts on local public goods. Since the 1970s, the impact of diversity has been limited chiefly to criminal justice, an issue that has remained racially coded, nationally salient, and relevant to localities. Contrary to past work, diversity's influence on local public goods is neither pervasive nor consistent. These findings challenge static conceptions of local ethnic and racial divisions, and they suggest a connection between diversity's local impacts and trends in national politics.

Keywords

ethnic and racial diversity, public goods, city spending, crime, agenda setting, urban politics

A growing body of scholarship leaves little doubt about how America's rising ethnic and racial diversity will influence localities. In recent years, researchers have shown that ethnically and racially diverse polities redistribute less

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(Alesina & Glaeser, 2004; Luttmer, 2001), produce fewer public goods (Alesina, Baqir, & Easterly, 1999; Goldin & Katz, 1999; Habyarimana, Humphreys, Posner, & Weinstein, 2007; Miguel, 2004; Poterba, 1997; Vigdor, 2004), and have lower levels of social trust and civic engagement (Alesina & La Ferrara, 2002; Leigh, 2006; Letki, 2008; Putnam, 2007; Rupasingha, Goetz, & Freshwater, 2006; Soroka, Johnston, & Banting, 2005).[1] This work echoes long-standing research in political science demonstrating the distinctive organization of politics in racially diverse parts of the United States (e.g., Glaser, 1994; Hero, 1998; Key, 1949). Looking specifically at the connection between racial and ethnic divisions and public spending, scholars have argued that racial animus has been an engine of the antitax politics prominent since the 1970s (Edsall, 1991; Kinder, Burns, & Vieregge, 2005; Schrag, 1998; Sears & Citrin, 1985). In a similar vein, research shows that American states with larger Black populations have less generous welfare programs (Hero, 1998; Howard, 1999; Peterson, 1995; Soss, Schram, Vartanian, & O'Brien, 2001) and lower spending (Cutler, Elmendorf and Zeckhauser 1993). Ethnic and racial diversity can also lead to a shift among spending priorities, as diverse localities spend more money on policing and criminal justice (Jackson & Carroll, 1981). In short, the association between diversity and reduced or reshaped public investments appears quite robust.[2]

If diversity really dampens public investment as suggested by this extensive research, current demographic changes have powerful implications. With diversity rising markedly, the tragedy of the commons should be worsening, and we should expect declining investments in roads, libraries, and other public goods. Certainly, those are the implications of Alesina et al. (1999). In the decade since its publication, Alesina et al. (1999) has ignited scholarly interest in diversity and public provision, garnering 990 citations.[3] Yet that seminal article is based on cross-sectional data, leading us to question when and how those correlations came about. Correlations visible in the present between local priorities and demographics do not necessarily mean that diversity has a causal impact today. Indeed, given the changing role of race in American politics since the 1950s, and given that diversity has a new meaning in an increasingly multiracial society, one might suspect that diversity's impact has changed over time.

This article thus reexamines the question originally asked by Alesina et al. (1999): how does ethnic/racial diversity affect public provision in large U.S. cities? Its central contribution is to look not at a single cross section but instead at a 52-year time span from 1950 to 2002. When viewed over time, we see that *ethnic and racial diversity is not the stable influence on U.S. localities' spending priorities that past work contends*. Roads, libraries, and hospitals do

not necessarily receive less support in diverse locales. This conclusion is critical in assessing the likely political impacts of contemporary immigration and in accounting for the causes of unequal public good provision across cities. Comparativists have already challenged the assumption of stable ethnic cleavages in many contexts (e.g., Laitin, 2007; Posner, 2005; Wilkinson, 2004). This article shows that time-varying, context-dependent notions of ethnic and racial divisions might apply to U.S. localities as well.

Identifying how ethnic and racial divisions vary in their impacts is the work of a research program, not a single article. Nonetheless, this article offers a novel explanation for *when* local ethnic and racial divisions are likely to influence local spending. Synthesizing studies of public opinion, racial and ethnic politics, and urban politics, its "politicized places" hypothesis contends that diversity shapes local priorities most consistently when salient national rhetoric connects those issues to race and ethnicity. Like their national counterparts, local leaders and residents rely on frames to condense information and focus attention on certain aspects of an issue. But unlike their national counterparts, local leaders have little capacity to shape national discourse. A counterintuitive claim follows: local politics will divide not necessarily on the most pressing local concerns but on those concerns that are politicized and racialized by national rhetoric. This pattern is exactly what we observe with cities' anticrime spending, which became more strongly related to ethnic and racial diversity as crime became a nationally salient issue.

This article also offers a methodological argument. In 2002, U.S. local governments spent 1.14 trillion dollars (U.S. Census Bureau, 2005), more than the 2004 Gross Domestic Product of Canada and all but seven countries (World Bank, 2006). Yet research on local governments has been plagued by incomparability among units, in part because states accord their localities very different responsibilities (Clarke & Ferguson, 1983; Peterson, 1981). This article uses multilevel models to account for the dependence among the outcomes within states. It also directly measures potential confounding factors, including each locality's state aid and its breadth of functional responsibility. Where possible, this research specifies the dependent variable as changes from the previous period to deal with unobserved confounders that are fixed over time. With these improvements, the spending decisions of 992 large cities should provide fertile ground on which to test hypotheses about diversity's shifting impact.[4]

This article's next section outlines historically grounded approaches to diversity's impact, developing observable implications for four time-varying hypotheses. These hypotheses are synthesized from research on race, public opinion, and urban politics, but they are novel as applied to explaining cities' spending patterns. The section "Data: U.S. Cities, 1950-2002" discusses the data as well

as methodology. The section "Results: Diversity's Dynamic Impacts" presents evidence that diversity's impact on contemporary U.S. cities is quite different from its effect in earlier decades. It begins with the period from 1973 to 2002, where the data are far more extensive, and then considers the more limited data available from 1950 to 1965. The section "Discussion and Conclusion" discusses what these results suggest for local public spending in the decades to come.

Hypotheses on Diversity's Varying Impact

Ethnic and racial diversity is widely thought to dampen redistributive spending (e.g., Alesina & Glaeser, 2004; Edsall, 1991; Gilens, 1999; Schrag, 1998; Sears & Citrin, 1985), as members of one group prove unwilling to extend assistance to members of another group. Alesina et al. (1999) stands out in part because it contends that diversity can negatively affect public good provision as well. In its view, this effect on public goods operates through two pathways. The first is voters' anticipation of differing preferences. Knowing that his or her preferences will be aggregated with those of others who favor different kinds of spending, a rational individual will anticipate fewer benefits from collective spending and support less of it. The second is a more straightforward notion of outgroup aversion, wherein voters receive diminished utility from public goods shared with members of other groups. Using data on U.S. cities, counties, and metropolitan areas for 1990, Alesina et al. (1999) demonstrates a robust cross-sectional association between diversity and reduced budget shares devoted to roads, sewers, and schools.[5] In light of America's growing diversity (Frey, 2006), its theory leads us to expect that the gaps between diverse localities and homogeneous localities will grow in the coming years. One of its core assumptions, in fact, is that the specific identities of the racial and ethnic groups matter less than the presence of diversity in any form.

Yet is diversity's impact immutable, or does it shape public good provision only under certain circumstances? There are powerful reasons to think that diversity's impact varies. Outside of the United States, Miguel (2004) illustrates that nation-building policies eased the impact of ethnic divisions in Tanzania. Other comparative scholars have found that shifting elite incentives influence how ethnic cleavages are politicized (e.g., Posner, 2005; Wilkinson, 2004). These findings make sense: racial and ethnic divisions are seen by many as social constructions (Chandra, 2006; Fearon, & Laitin 2000; Green & Seher, 2003), and both the constructions themselves and their political relevance should be subject to change. Yet this possibility has not been widely acknowledged among researchers of diversity's policy impacts.

This section outlines four time-sensitive hypotheses of diversity's impact and their observable implications. One hypothesis emphasizes changing demographics, another highlights changing racial attitudes, a third points to the national salience of racialized frames, and a fourth focuses on changing local conditions and resource competition. All of the hypotheses are grounded in past research on race, public opinion, or urban politics, literatures that have remained separate from debates over diversity's public policy impact. But they are nonetheless hypotheses: they are not true or false *ex ante*, but instead plausible mechanisms through which diversity's impacts might shift over time. These are far from the only hypotheses about diversity's policy impacts, but they are all simple and time-varying, making them well suited to direct comparison. The notion—advanced by Alesina et al. (1999)—that ethnic and racial divisions are static and pervasive is a fifth hypothesis that shadows these discussions. That hypothesis is referred to as "outgroup aversion."

Changing Demographics

One explanation is that diversity has a very different meaning today than it did in the Civil Rights era, as Latinos have become the largest minority group in the country (Segura & Rodrigues, 2006). The Alesina et al. (1999) approach equates diversity and race relations, but that is increasingly inappropriate. In 1970, the median city was 1.6% Hispanic, whereas today that figure is 7.5%. In cities, the index of diversity employed here had a Pearson's correlation of .72 with the percent Black in 1970, a figure that dropped to .38 in 2000. In other words, today's diversity is multiracial and multiethnic. If past findings are specific to African Americans, contemporary diversity should have smaller impacts on local spending.

Research on the capacity of Latinos and Asian Americans to generate threatened responses has grown in recent years, and the findings are less consistently negative than those for African Americans. Taylor (1998, pp. 528-531) finds that Whites' views of Latinos and Asian Americans do not vary with the percentage of those groups in their metropolitan area.[6] Furthermore, Cain, Citrin, and Wong (2000) do not find a contextual effect of ethnic minorities on support for a California ballot measure to ban affirmative action, although Tolbert and Grummel (2003) do. If it was tension between African Americans and others that led to reduced public good provision, influxes of other groups should not matter. Here, *the observable implication is that the percentage of African Americans should continue to negatively affect specific public goods, but that diversity as a whole should see reduced impacts as it becomes less synonymous with the percentage of Blacks*. If correct, this hypothesis inveighs

against the use of composite measures of diversity since they equate very different groups.

Changing Racial Attitudes

Another dynamic explanation is that Americans' racial attitudes have moderated considerably, at least in their outward expression. Recent debates over the nature of American racial attitudes have not reached a consensus, but they start with the same observation: outward displays of racial prejudice have declined dramatically since the 1950s (Lassiter, 2006; Schuman, Steeh, Bobo, & Krysan, 1997; Sears, Hetts, Sidanius, & Bobo, 2000). As one example, the percentage of Whites saying that "white and black students [should] go to the same schools" increased from 32% in 1942 to 96% in 1995 (Sears, Hetts, et al., 2000, p. 11). Certainly, there are fundamental debates about the extent and the sincerity of this change (e.g., Mendelberg, 2001; Sears, Henry, & Kosterman, 2000; Sniderman & Carmines, 1997), but no one denies that some attitudinal change has occurred—and that it has become unacceptable to publicly advocate racism, however much racism remains in private (Kuklinski, 1997; Mendelberg, 2001).

If interethnic and interracial animus is an important source of attitudes toward local policies, then as the public expression of that animus declines, so too might spending differences in diverse locales. Political elites will be constrained in tapping racial attitudes to advance their goals; citizens will be constrained in expressing their racial attitudes (Mendelberg, 2001). If the hypothesis of changing outward attitudes is correct, we should expect *that diversity was a strong predictor of local spending in the 1950s and 1960s*, when overt racial appeals were more acceptable and more common in urban politics (Hajnal, 2007; Lassiter, 2006). We should also expect *few impacts of diversity since the 1960s*. Yet this hypothesis assumes that attitudes translate directly into political outcomes, without any place for local political elites or agenda-setting processes. It also assumes that constraints on elites' rhetoric are politically influential and that they cannot simply use coded language to tap racial sentiments. These are assumptions that the following hypotheses avoid.

Politicized Places

The politicized places hypothesis claims that the extent to which local politics are racially charged is derived partially from national politics. This claim draws on several literatures. It begins with the observation, well-grounded in survey research, that the salience of specific issues shapes how voters think about politics (e.g., Druckman, Jacobs, & Ostermeier, 2004; Iyengar & Kinder, 1987;

Krosnick & Kinder, 1990). This is especially true with race, as even subtle cues can effectively racialize issues (e.g., Hurwitz & Peffley, 2005; Hutchings & Valentino, 2004; Kellstedt, 2003; Mendelberg, 2001; Valentino, 1999). Considered together, these strains of research suggest that the extent to which attitudes are racialized might hinge on the set of issues salient at the national level, and on their racial codings.

How could this affect localities? One channel is through voters, who draw on nationally salient issues when responding to local events (Hopkins, 2010; Hopkins, in press). Voters do not distinguish perfectly between the responsibilities of the various levels of government, so it is quite plausible that salient issues and rhetoric at the national level might prime their local concerns. Indeed, this effect of nationally salient rhetoric has been demonstrated in multiple countries and on multiple policy issues (Hopkins, 2010; Hopkins, in press). Local activists and elites are another potential channel. Recognizing their limited capacity to frame issues and offer an agenda that is outside the national mainstream, local leaders might instead draw on connections between issues and race/ethnicity that are already salient nationally—or might be forced to confront issues that are prominent across the country. In part, this hypothesis builds on recent political histories (Kruse, 2005; Lassiter, 2006; McGirr, 2001; Self, 2003), all of which illustrate an interplay between local and national politics. Whether it was African Americans mobilized into Oakland politics by the Civil Rights movement or Southern suburban Whites responding to court-ordered desegregation through the Charlotte Board of Education, this work details how local political leaders were forced to address issues in the wake of national events. As with national leaders, local leaders have an endless set of problems they could confront (Jones and Baumgartner 2005). The low salience of their work means that they are likely to toil in obscurity (Palmgreen & Clarke, 1977). Yet nationally salient issues offer a potential solution to those problems, as they can make it easier to mobilize support and attract attention. For both voters and local leaders, nationally salient issues have an edge in commanding local attention.

For the politicized places hypothesis to be useful in explaining policy outcomes, the frames must be pervasive in the media and relevant to local governance. One such frame links African Americans to crime. Even today, crime in non-Black minds is often racialized, an utterly consistent finding from decades of research. Separate research projects have demonstrated the connection from 1965 to 1974 (Beckett, 1997; Jacobs, 2000; Weaver, 2007), from 1974 to 1988 (Baumer, Messner, & Rosenfeld, 2003), in 1989 (Entman, 1990), in 1991 (Peffley, Hurwitz, & Sniderman, 1997), from 1993 to 1994 (Entman & Rojecki, 2001), from 1995 to 1997 (Gilliam & Iyengar, 2000), in 1996 (Valentino, 1999),

and from 2000 to 2001 (Barkan & Cohn, 2005; Hurwitz & Peffley, 2005). This period saw different trends among policymakers and the general public. As Figure 1 in the online appendix shows, crime grew as a topic of elite political discussion (proxied by *New York Times* articles) with some consistency from the mid-1960s until the late 1990s (see also Jones & Baumgartner, 2005). Over the same period, the percentage of people citing crime as the most important national problem in Gallup polls leapt from 2% to 16% in 1967 and remained above 10% until after 1973. It would rise above 10% again in 1977 and from 1993 to 2002.

As Rae (2003, p. X) notes, crime itself and the fear of crime are actually two separate problems.[7] Anticrime politics can be driven not only by local crime rates but also by *perceptions* of crime rates gleaned from local and national information. The politicized places hypothesis is compatible with these stylized facts. It suggests that national attention and rhetoric on crime will help politicize the issue locally, potentially influencing localities even where crime rates themselves remain low. Overall, then, the politicized places hypothesis predicts that *diversity should encourage anticrime spending beginning in the mid-1960s and continuing through the 1990s*. In diverse environments, the anticrime frames that became prominent in the late 1960s seem especially applicable. To be sure, endogeneity is a concern, as local issues are bound to feed the national agenda. But for any given locality, the causal arrow will run mostly from national to local, as local politicians respond to the issues raised by national politics.

Realistic Conflict

The final hypothesis is largely a foil to the previous one. Whereas the politicized places hypothesis contends that local ethnic and racial divisions are the product of salient national frames, one can extrapolate a time-varying alternative based on theories of realistic group conflict (e.g., Glaser, 1994). This approach holds that intergroup contention stems from local competition for scarce resources. Furthermore, resource scarcity has increasingly characterized policymaking in large U.S. cities. The period from 1950 to 2002 has seen marked changes in the demands placed on cities, as many have faced a declining manufacturing base, reduced federal aid, depopulation, riots, rising crime rates, and a disappearing tax base (e.g., Rae, 2003; Sugrue, 1996). In the face of declining resources, and as crime and other social problems command increasing resources, intergroup contention could sharpen considerably.

This perspective is grounded in local realities rather than national perceptions. One observable implication is that racial and ethnic divisions should have the

sharpest policy impacts in especially resource-constrained cities. Alternatively, construing the hypothesis to be about local conditions more broadly, *diversity's impact on spending should be most pronounced in places with the highest levels of unemployment, crime, or other adverse local conditions*, as residents respond to the realities they face with increased competition and animosity. These become dynamic hypotheses when crime rates or other local conditions vary over time. For a given spending area, we should not expect uniform impacts across localities. Instead, we should expect interaction effects, as cities that are confronted with certain intractable issues divide along ethnic or racial lines.

Categorizing Local Priorities

One way to test these hypotheses is to observe over-time variation in diversity's impacts since they sometimes make differing predictions. Yet some of the hypotheses—including those originally offered by Alesina et al. (1999)—also make testable predictions about specific types of public spending that should be especially influenced by ethnic or racial diversity. Here, we briefly outline different categories of urban public spending before summarizing the hypotheses.

To connect theories to data, urban researchers commonly group urban public spending into one of a handful of categories (Green, 1992; Hajnal, 2007; Jacoby & Schneider, 2001; Peterson, 1981), such as the "productive public goods" that are the focus of Alesina et al. (1999). Yet if diversity really operates through citizens' dislike of sharing public goods with members of other groups, we might hypothesize that its impact will not be the same across all public goods. Instead, its impact might be more pronounced on parks, libraries, transit systems, and other social-use public goods that require people to share physical space. Roads, sanitation, and fire prevention are public goods that can be used in private, making them less susceptible to impacts through that channel. This suggests that diversity's impact might differ even within the class of public goods based on whether they put different groups into close proximity.

Redistributive goods are a third category. In U.S. localities, public housing and health care are redistributive goods that are utilized largely by the disadvantaged (Hajnal, 2010), and investments in them will be dampened to the extent that people or politicians are less focused on the disadvantaged in more diverse environments (Luttmer, 2001).[8] Spending on criminal justice merits a fourth category of punitive and protective spending. Preventing crime is certainly a public good, but the police can also serve as a source of social control. Past research has shown that ethnic and racial tension can generate a punitive response (Hurwitz & Peffley, 1997; Jackson & Carroll, 1981), leading to potential increases

in criminal justice spending. Since most cities leave school spending to separate, single-purpose school districts, school spending does not compete directly with other local public goods and cannot be studied here.[9] To be clear, there are many reasons that cities' spending will vary across policy areas: some areas may be viewed as necessities, making their spending less responsive to political pressures. Other areas may operate under state or federal mandates, again limiting the potential impact that diversity could have (Gerber and Hopkins in press). Yet these are fixed characteristics of spending categories, and cannot typically explain why diversity's impacts would grow or shrink over time. In short, it is certainly the case that these spending categories vary along multiple dimensions, but that fact is more of a threat to static approaches than to the longitudinal approach taken here.

Table 1 summarizes the hypotheses and their competing predictions on three dimensions: whether diversity's impact varies with local conditions (such as the crime rate or the percent Latino), how diversity's impact is likely to have changed over time, and whether specific spending areas (such as criminal justice spending) should be most closely connected to diversity. The static notion that diversity undermines the provision of public goods is titled "out-group aversion," and "social-use" public goods are those that put people from different backgrounds into direct contact.

Data: U.S. Cities, 1950-2002

To test hypotheses about when ethnic differences influence public provision, this article studies U.S. cities. Doing so allows us to observe locality-level outcomes of interest and to make the political process part of our inquiry. Diversity is an emergent property, something that pertains only to aggregates, and as researchers have found in other contexts, aggregate-level results can differ markedly from individual-level results (Erikson, MacKuen, & Stimson, 2002; Sampson, McAdam, MacIndoe, & Elizondo, 2005). The focus here is on testing the hypotheses outlined above using variation across time and space from 1950 to 2002. By observing nearly 1,000 local governments, and with hundreds of available covariates, we can compare those that have diversified to those that have not. The dependent variables are the shares of city operating and capital expenditures devoted to various spending categories, such as criminal justice or libraries. This section discusses data and methodology.

The primary data set includes the 992 cities above 25,000 in population as of 1990 that reported nonzero spending in 1992 and 2002. The 25,000 cutoff is commonly employed; below that size, data availability is less consistent.

Table 1. Observable Implications that Differentiate the Hypotheses

Hypothesis	Varies with local conditions	Diversity's impact over time	Impacted spending areas
Changing demographics	Yes	Declined	All
Changing attitudes	No	Declined	All, especially social-use
Politicized places	No	Depends on category	Crime, salient and racialized issues
Realistic conflict	Yes	Increased	All
Out-group aversion	Yes	Static	Social-use, productive public goods

This appears to be the largest and most far-reaching data set on U.S. cities in use. The hypotheses vary over time, and the data set does as well: it unifies the 1973 Annual Survey of Governments, the 1992, 1997, and 2002 Census of Governments, and decennial U.S. Census data from 1950 to 2000. The 1973 Survey was chosen as the earliest year for which data were available in a format compatible with later years. Analyses after 1970 include current operating expenses, construction expenses, and other capital expenses.[10] To augment its political variables, the data set also includes county-level measures of social capital (Rupasingha et al., 2006) as well as the 2001 International City/County Management Association's (ICMA) survey of cities. The *County and City Databook* from 1994, 1997, and 2000 largely compiles data from other sources, but provides a valuable check on the data set's compilation. Data for cities from 1950 to 1965 is supplemented through the 1944-1977 *County and City Data Book* (U.S. Census Bureau, 2000). Data on riots come from Carter (1983). Finally, the U.S. Bureau of Justice Statistics (1997) provided location data. Missing observations are rare, with a few key exceptions. For instance, the 1973 Survey of Governments lacks data on 151 cities' spending, so those missing values are multiply imputed (King, Honaker, Joseph, & Scheve, 2001). The data set is discussed more comprehensively in the appendix.

How to measure diversity? One widely used measure is the Herfindahl index (e.g. Alesina et al. 1999; Branton & Jones, 2005; Putnam, 2007; Vigdor, 2002, 2004), which can be interpreted as the probability that two randomly selected individuals are from the same group.[11]. Diversity is one minus the Herfindahl index. This article employs the Herfindahl-based index to be as comparable with past work as possible, although it is important to note that the Herfindahl-based index correlates with an entropy-based measure of diversity at 0.87. The

core assumption built into any such index is that groups are exchangeable, an assumption probed below. Other independent variables are straightforward and are detailed in Table 2. Since the key causal variable is a city's change in diversity, and since population movements are often governed by economics, a variety of economic independent variables are included as levels and changes.

Method

Quantitative research on local government faces considerable hurdles. While statistical approaches commonly invoke the assumption of ignorability—that the unobserved differences between treatment and control units are ignorable accounting for differences we do observe (Imbens, 2004)—that assumption is patently false in the case of local governments. Legally, local governments are creatures of the states (Morgan & Watson, 1995). If states that happened to be diverse also happened to delegate more police power than the average state, we could easily misattribute key effects.

One answer is to model the data using a multilevel model with random intercepts for each state (Gelman & Hill, 2006). More flexible than Ordinary Least Squares (OLS), this approach draws on all of the observations to estimate individual-level parameters without assuming either complete pooling or no pooling across states (Gelman & Hill, 2006).[12] Such an approach provides us with accurate uncertainty estimates in light of clustering.[13] By modeling the state-level intercepts, we can identify the proportion of the variance explained by state-level factors, assuming that unobserved random effects are independent of the covariates.

Past studies conclude that diversity commonly dampens certain public investments. Yet the fact that diverse communities spend smaller shares of their budgets on public goods does not imply a causal relationship in the present. The analysis thus considers both the impact of the level of diversity and the impact of changes in diversity. If the relationship is truly causal, we should expect the independent variables to predict *subsequent* changes in the dependent variable. At the same time, modeling changes reduces cities' unobserved heterogeneity since any factors that are unchanging are implicitly taken into account by differencing the dependent variables. For example, local institutions change only rarely—20 of 620 cities surveyed by the ICMA in 2001 had undergone any institutional change since 1996—meaning that this technique holds constant the institutional context in the vast majority of cases. Table 3 details the dependent variables specified as changes from 1973 to 2002 and highlights the substantial changes in local public investment that were underway. In this period, cities saw declines in their spending on roads, health care,

Table 2. Summary Statistics for Key Independent Variables for the 992 Cities Studied

	Min	Median	Mean	Max	*SD*	Missing
Herf 2000	0.23	0.55	0.58	0.95	0.17	0
Δ Herf 1970-2000	–0.64	–0.20	–0.22	0.39	0.15	28
Herf 1970	0.36	0.83	0.80	1.00	0.17	28
Logged population 2000	9.94	10.90	11.10	15.90	0.78	0
Δ Logged population 1970-2000	–0.80	0.28	0.45	3.28	0.64	28
Logged population 1970	7.94	10.60	10.70	15.90	1.00	28
Inc. Gini 2000	0.30	0.42	0.42	0.57	0.04	0
Δ Inc. Gini 1970-2000	–0.02	0.08	0.08	0.20	0.03	28
Inc. Gini 1970	0.20	0.35	0.35	0.49	0.05	28
Percent poor 2000	0.01	0.13	0.14	0.47	0.07	0
Δ Percent poor 1970-2000	–0.21	0.04	0.05	0.41	0.05	28
Percent poor 1970	0.01	0.08	0.08	0.41	0.05	28
Percent above 65 2000	0.04	0.13	0.13	0.39	0.04	0
Δ Percent above 65 1970-2000	–0.20	0.04	0.04	0.21	0.04	28
Percent above 65 1970	0.01	0.09	0.09	0.48	0.05	28
State rev. per capita 2002*	0.00	0.13	0.27	3.64	0.40	0
Δ State rev. per capita 1973-2002*	–0.12	0.11	0.27	3.52	0.42	163
State rev. per capita 1973*	0.00	0.02	0.03	0.30	0.02	163
Functional breadth	0.08	0.17	0.26	1.00	0.24	0
Ward elections	0.00	0.00	0.13	1.00	0.33	406
Council-manager	0.00	1.00	0.72	1.00	0.45	391
Mean income 2000	29,000	52,500	58,000	170,000	18,451	0
Mean income 1970-2000	–37,800	–1,330	765	62,900	11,571	28
Mean income 1970	31,500	54,400	56,900	138,000	12,632	28
Median home value 2000	35,700	114,000	144,000	1,000,000	104,674	0
Crime rate 1991	187	6,570	7,090	37,900	3,189	112

Note: The asterisk denotes variables in thousands. The Herfindahl index, abbreviated "Herf," is a measure of homogeneity, and is the inverse of diversity. The crime rate is crimes per 100,000 people. Δ indicates changes.

sanitation, fire, and libraries. They saw concomitant increases in their spending on criminal justice, transit, and housing.[14]

The third answer to unobserved heterogeneity comes from model specification. The models here condition on measures of state aid per capita, a key confounder.[15] The median city received $128 in state aid per capita in 2002, and that funding is sure to influence local public provision.[16] In addition, these models condition on a measure of the breadth of the locality's spending that is calculated in the same way as the Herfindahl index.[17]

Table 3. Summary of Key Dependent Variables

	Cities 1973		Δ Cities 1973-2002	
	Mean	*SD*	*Mean*	*SD*
Roads	0.144	0.082	–0.035	0.084
Criminal justice	0.160	0.060	0.009	0.069
Health	0.035	0.103	–0.014	0.096
Sanitation	0.152	0.115	–0.031	0.115
Parks	0.073	0.058	0.001	0.064
Fire	0.109	0.051	–0.015	0.055
Libraries	0.020	0.024	–0.004	0.021
Housing	0.026	0.058	0.024	0.081
Transit	0.007	0.026	0.007	0.030

Note: Spending on roads has declined significantly, and health spending is down as well. Spending on crime and housing have both climbed.

Results: Diversity's Dynamic Impacts

This section illustrates how diversity's impact varies in ways predicted by the shifting salience of national issues. It begins where past work did, examining the cross-sectional correlation between diversity and local priorities. Here, we expand on past work by looking at four separate cross sections from 1973 to 2002. Yet it is criminal justice and housing that have the most reliable cross-sectional relationships with diversity, not the productive public goods emphasized by Alesina et al. (1999). Also, these cross-sectional relationships have not changed much since 1973, prompting another question: has diversity continued to have an impact in recent decades?

Time-invariant theories of diversity's impact imply that the gaps between diverse and homogeneous localities should grow as the U.S. diversifies. Given the fast pace of diversification from 1973 to 2002—the mean Herfindahl index dropped by almost a standard deviation from 1970 to 1990—impacts during that period should surely be visible if the time-invariant approach is correct. But they are not. After 1973, several spending areas were not affected by either changes in diversity or its baseline level, and those areas that were affected sometimes saw *positive* impacts of diversity. However, criminal justice showed the continued impact of increasing diversity, just as the politicized places hypothesis would predict. This section then breaks out the impacts for large racial or ethnic groups. It also considers data from the 1950s and 1960s before providing several robustness checks. To be clear, without an exogenous instrumental variable to draw on, the goal here is to use conditional expectations from observational data to assess the relative plausibility of several hypotheses.

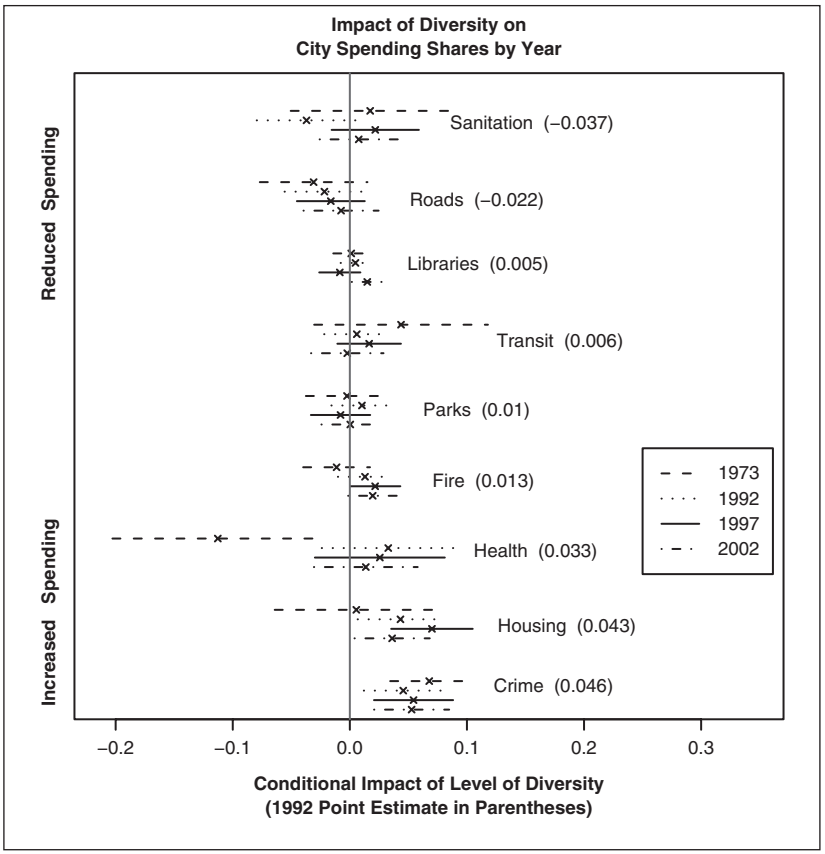

Figure 1. Impact of Diversity on City Spending Shares by Year.
Note: Each line represents a coefficient from a multilevel model indicating the cross-sectional relationship between diversity and that spending share. The lines are 95% confidence intervals, and show that diverse cities have spent more on criminal justice and housing. Road spending appears to be negatively related to diversity. For a full list of covariates, see end note 18. In parentheses next to each label is the coefficient for 1992.

Cross-Sectional Results

Alesina et al. (1999) use data from 1990 to 1992 and OLS to estimate the relationship between diversity and U.S. localities' spending on productive public goods.[18] After extending their set of covariates, we estimated multilevel models of spending shares where the independent variables are from the closest census

year prior to the outcome.[19] For instance, the 1973 spending share is regressed on independent variables from 1970. Observations with zero spending in a given category are dropped, as such zeroes reflect functional responsibilities rather than choices and priorities.

The key coefficient, β_1, reports the change in a certain spending category given a shift from an entirely homogeneous city to one where everyone is a member of a different group. In actuality, there are nine types of spending of interest here, and they are observed at four points in time, yielding 36 separate cross-sectional estimates of the coefficient for diversity, β_1. Figure 1 presents each of these 36 estimated coefficients graphically, with an "x" indicating the coefficient and a line indicating its 95% confidence interval. For example, the top line in the figure indicates that in 1973, the cross-sectional relationship between diversity and sanitation spending was 0.017, but with a 95% confidence interval that includes zero.

From Figure 1, we learn that many of the relationships are not statistically different from zero. Intriguingly, the public goods that demand shared space—including transit, parks, and libraries—show no negative impacts at all. That weighs against straightforward theories about people's unwillingness to share public goods across ethnic and racial lines.[20] Together, these weak correlations suggest that the mechanism through which diversity operates is not a mass-level unwillingness to use public goods alongside those of other backgrounds.

However, cities that are more diverse spend larger portions of their budget on criminal justice in all 4 years—and the same appears true for city spending on housing after 1973. In 1973, health spending was negatively related to diversity in cities although that distinction has since disappeared. Road spending is lower in more diverse cities, but this relationship seems to fade with time and was never significant. Looking cross-sectionally, the key finding is that diversity seems to redirect resources to crime prevention and housing. Yet cross-sectional estimates tell us nothing about when the conditional correlations came about. Before delving more deeply into these patterns and their theoretical implications, we turn to analyzing changes in spending shares over time, from which we can more reliably draw causal inferences.

Diversity's Anticrime Impact, 1973-2002

The analysis now focuses only on those spending categories that showed a potentially important relationship with ethnic and racial diversity above. To get leverage on whether diversity continues to influence spending patterns in recent years, the dependent variables are now the *changes* in spending shares. The analysis respecifies the models above to include both levels in 1970 and

changes from 1970 to 2000 for each independent variable. For instance, we predict the change in the share of each city's budget devoted to criminal justice from 1973 to 2002 as a function of the 1970 level of diversity, its change from 1970 to 2000, and a set of other covariates that are either levels as of 1970 or changes during those 30 years. Although imposed by the structure of the data, the lagging of the explanatory variables by 2 years should reduce concerns about endogeneity. To facilitate readers' understanding of this model, Table 1 in the online appendix includes a fitted model. The intraclass correlation is .04, meaning that the overwhelming majority of the variation is at the level of cities rather than states. Modeling state-level variation is unlikely to be productive.

Analyzing changes in spending shares as the dependent variables raises the methodological bar since there is often far more unexplained variability in data on changes. However, these models differ from difference-in-difference estimators in that the independent variables include both baseline levels and changes for all variables over the relevant period. Given the theories being tested, including baseline levels of diversity is critical. It might be that shifts in national politics make a certain level of diversity politically relevant when it had not been so before. This was potentially the case in the 1960s, when the mass enfranchisement of Blacks led existing levels of diversity to take on newfound political relevance. The models also include changes in diversity and other independent variables. Through a separate mechanism, sudden changes in demographics might threaten long-time residents and reconfigure the local agenda. Rising diversity could also incrementally increase the effect of high levels of diversity.

Figure 2 shows estimates and 95% confidence intervals for the predicted change in spending given a substantial shift in the level of diversity or the change in diversity.[21] Of those results that differ from zero, we see that both high levels of diversity and large increases in diversity can induce criminal justice spending. On average, a city that was more diverse than 80% of its counterparts saw a 2.6 percentage point relative increase in its criminal justice spending, and a city that was diversifying faster than 80% of its counterparts saw a 1.9 percentage point relative increase.[22] Increases in anticrime spending were especially pronounced in cities like Detroit that were diverse at the beginning of the period and also in cities like Charlotte that became increasingly diverse. Also, diverse cities increased health spending by 2.8 percentage points.[23] However, diversifying cities did not differ on their sanitation or road spending. Indeed, no other results are much different from zero.[24]

It is tempting to use realistic group conflict or other locally oriented theories to explain the anticrime spending results. Yet if local resource competition, crime, or city-specific events are driving this relationship, the impact of diversity should not be even across cities. Instead, it should be concentrated where intergroup

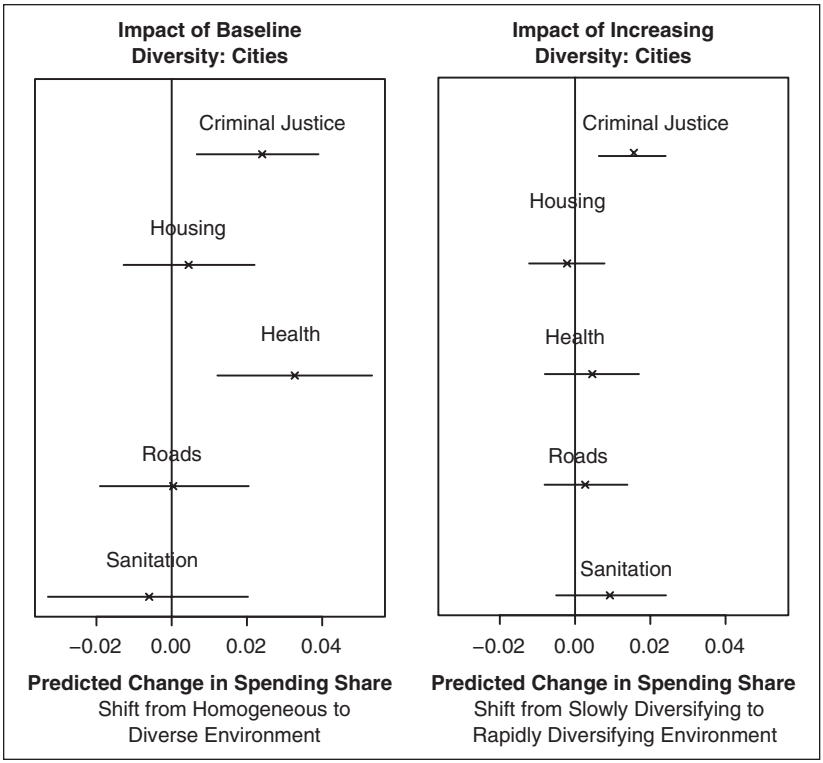

Figure 2. Impact of Baseline and Increasing Diversity
Note: Changes in spending shares for key categories were modeled as a function of the baseline level of diversity, the change in diversity over the period, and a host of other covariates shown in the online appendix. The lines are the 95% confidence intervals for the predicted change in spending shares when we shift either levels of diversity (left) or changes in diversity (right) from the 20th percentile to the 80th percentile. From 1973 to 2002, higher baseline levels of diversity encouraged increased spending on criminal justice.

competition is highest: in places with high crime rates, a recent history of riots, high levels of poverty, or low income. Using covariates measured in 1970 or in the late 1960s, models not shown test these interactions. They find no evidence at all that diversity's influence on anticrime spending is moderated by local conditions. In results below, we will confirm the same null impacts of actual crime rates. The findings on anticrime spending appear to be a national phenomenon rather than a simultaneous response to deteriorating local conditions.

The health care result indicates a positive relationship between diversity and public goods provision, which is certainly not what we would expect given past research. We saw above that diverse cities spent smaller fractions of their

budgets on health in 1973, but from Figure 2, we see that this gap closed after 1973, as diverse communities saw larger relative increases. Since this is a period when some localities shifted away from direct health care provision, it is possible that this result reflects those changes in responsibilities. For city spending in the post-1973 period, only criminal justice spending shows consistent evidence that diversity mattered in the way we would expect. With that exception, it is safe to conclude that diversity's impacts were negligible or positive, even during a period when diversity was increasing rapidly. *Given its tremendous demographic changes, the period from 1973 to 2002 is a most-likely case for hypotheses linking rising diversity to declining public good provision, and yet the evidence points in other directions*.

A broader picture is beginning to take shape. As early as 1973, diverse cities were distinctive in their spending patterns. They were more punitive and less oriented toward health care. Since then, that distinctiveness has declined for health care but grown for criminal justice spending. The latter finding is what the politicized places hypothesis would predict. Given how inertial and incremental budget-making can be (Fuchs, 1992), it is understandable that a causal impact at some point in the past could be reflected in cross-sectional relationships for quite some time. The most consistent case of diversity's influence is not a social-use public good such as parks or a productive public good such as roads but instead criminal justice.

Disaggregating Diversity

The changing demographics hypothesis suggests that we risk oversimplification by using a single measure of diversity such as the Herfindahl index. Ethnic and racial minority groups in the United States have profoundly different histories and relationships to the political system: they may have different impacts on local spending as well. We now consider whether the emerging pattern of results differs for the two largest groups, Blacks and Hispanics. These multilevel models are similar to those used above, with the changes in spending shares from 1973 to 2002 modeled as a function of 1970 levels and 1970-2000 changes in a host of independent variables. However, these models do *not* include the Herfindahl index, and instead include measures of levels and changes for the percent Black and the percent Hispanic.

Figure 3 presents the results when predicting a shift from the 20th percentile to the 80th in the level or change in the percent Black or Latino.[25] The results for levels are presented at left, while the results for demographic changes are presented at right. In both cases, the top line is for the percent Black while the bottom line is for the percent Hispanic.

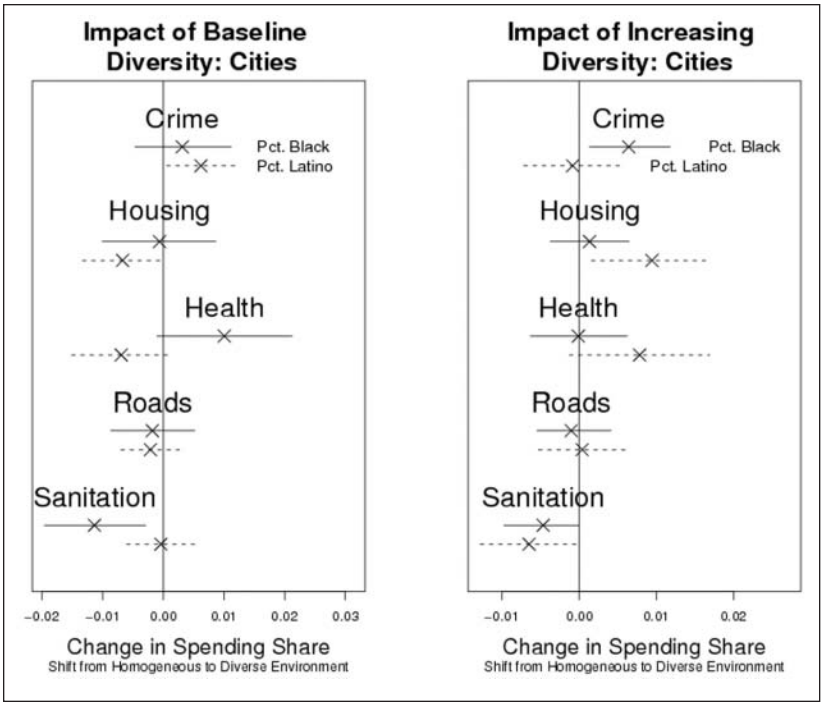

Figure 3. Results of multilevel models predicting changed spending shares by group. Note: This figure shows the results of multilevel models predicting the changed spending shares from 1973 to 2002 separately by ethnic/racial group. At left, we see the impacts of the baseline percent Black (at top) and Hispanic (at bottom). At right, we see the impacts of the changing percent Black and Hispanic.

The results are not only suggestive but also mixed for several hypotheses. *In almost no case do we observe effects across groups whose differences are statistically significant*. That weighs against the claim that Blacks and Hispanics have different local impacts on spending. On anticrime spending, we learn that both the baseline percent Black and the baseline percent Hispanic are associated with increases in anticrime spending, but that the impact of the percent Hispanic is slightly stronger (0.6 percentage points) and is statistically significant. Turning to demographic changes, we see that an increase in the percent Black over this period is associated with a 0.6 percentage point increase in anticrime spending, with a 95% confidence interval from 0.1 to 1.2 percentage points. Here, the impact of the changing percent Latino is estimated with considerable uncertainty. It is certainly the case that *both the percent Hispanic and the percent Black are related to increased anticrime spending in some capacity*.

For other spending categories, these disaggregated results are generally indistinguishable from zero. For spending on roads and highways—a key category for the Alesina et al. (1999) approach—we see no evidence of impacts for either group or either variable. In fact, the average impact never rises above 0.2 percentage points. No impacts are notably different from zero for health either. Yet for sanitation, there is some suggestion of diminished investment, as all four impacts are negative and three of the four are substantively significant. For housing, cities with higher baseline Hispanic populations saw reduced spending by 0.7 percentage points on average, but increases in their Hispanic populations were associated with a 0.9 percentage point *increase*. This final result is not predicted by any of the hypotheses and might reflect unobserved regional differences or growing Hispanic political strength. Still, we can be more confident that for anticrime spending, the results are not driven exclusively by the Hispanic or Black populations. Moreover, the null effects on road spending remain when looking separately at Blacks and Hispanics.

Little Influence, 1950-1965

The 1950s and 1960s provide another opportunity to test the hypotheses. Hypotheses of intergroup animosity and changing attitudes would predict that diversity's influences on spending should be more pronounced at a time when overtly racist political appeals and racial strife were widespread (Hajnal, 2007; Lassiter, 2006; Sugrue, 1996). By contrast, this was the period just before race was closely linked to concerns about criminal justice, so the politicized places hypothesis predicts little influence of local diversity on criminal justice spending. Although data for the 1950s and 1960s are not as rich as more recent data, and although they are not always comparable with more recent data, this section confirms that diversity does not dampen the provision of public goods even during periods of overt racial contention.[26]

Data on ethnicity are not available for cities in 1950 or 1960, so the analysis focuses on the share of African Americans. This was a period when the percent foreign born in the United States was just 6.9%, and it was also a period of high mobility among Blacks, meaning that this omission is unfortunate but not prohibitive. As of 1970, the correlation between the percent Black and the index of diversity was .70. For 576 U.S. cities larger than 25,000 people as of 1960, data are available through the *County and City Data Book's* 1944-1977 City File (U.S. Census Bureau, 2000). The observed variables are similar to those used above, including the percent African American, the logged population, median family income, and the percent above 65. To predict the change in the share of local spending from 1950 to 1960, the initial models use these covariates measured in 1950. They also include measures of the change in each variable from 1950 to 1960. Clearly, fewer control variables are available.

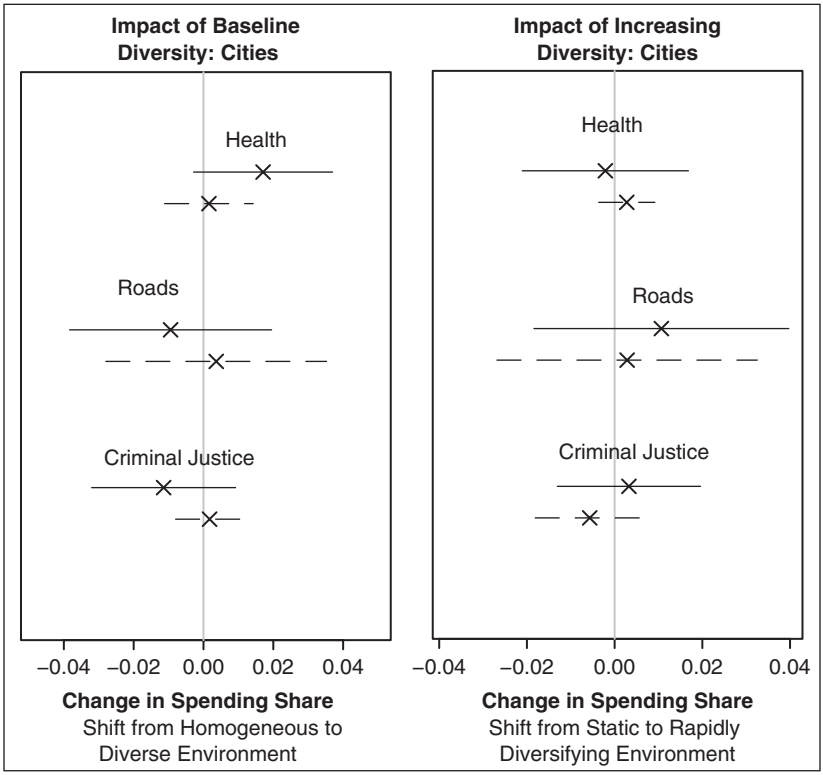

Figure 4. Coefficients for Percent Black Predicting Spending Changes, 1950-65.
Note: Each line represents an estimate and 95% confidence interval for the predicted change in the city's budget given a demographic change. For each spending category, *the solid lines on top depict the relationship for 1950 to 1960 while the dashed lines depict the relationship from 1960 to 1965*.

Three dependent variables are available: the share of operating expenditures used on criminal justice, health care, and roads.

Even in this reduced sample, some data are missing. In 1950, 30% of cities' crime spending is unobserved, a figure that drops to 4% for 1960. As a result, the analysis uses multiple imputation. We again model spending outcomes using multilevel models with state random effects and then predict the outcome under two scenarios. The first scenario sets the key independent variable to its 20th percentile while the second uses the 80th percentile.[27]

The estimated coefficients predicting spending changes between 1950 and 1960 are displayed in Figure 4 with solid lines at the top of each pairing. Although

we would expect to see strong negative relationships between diversity and increases in road spending under an animosity-based hypothesis, we see nothing of the sort. Instead, the relationship between diversity and spending on roads and health is always close to zero. There is also no strong evidence that diverse cities in the 1950s saw increases in their anticrime budgets. In fact, the estimated effect of having many African Americans on anticrime spending is negative (but insignificant). As the right panel of Figure 4 illustrates, cities with rising shares of African Americans do see larger increases in anticrime spending, but the result is substantively small (0.4 percentage points) and statistically insignificant.

Even that small positive relationship disappears when we consider changes in anticrime spending from 1960 to 1965, which is depicted by dashed lines in Figure 4. Looking at both levels and changes from 1960 to 1965, during a period of tremendous mobilization on the issue of civil rights, the estimated effects for all three dependent variables are almost exactly zero. The 1950s and early 1960s were a time when the arrival of Blacks in many cities provoked heated and sometimes violent opposition (e.g., Hirsch, 1995; Kruse, 2005; Sugrue, 1996), so the absence of any effects challenges both the realistic group conflict hypothesis and the hypothesis of changing racial attitudes. Locally rooted explanations cannot explain why the arrival of African Americans would produce little policy change in the early 1960s. But the politicized places hypothesis makes sense of these weak effects by pointing out that the salient frame connecting race and crime only became prominent during the mid-1960s. Past research suggests that diversity dampens the provision of productive public goods. Yet by observing a critical moment in U.S. race relations, this analysis shows that there were no strong effects. Levels of expressed racial animosity do not influence diversity's impact in a consistent or straightforward way.

Robustness

One source of robustness is the depth of the data set. We draw similar conclusions from models estimated from different decades and sources. In addition, analyses not shown replicate many of these results for 3,025 U.S. counties, with broadly similar conclusions. There, too, productive public goods do not show the strong relationship with diversity that past research suggests. Still, this subsection explores just how robust the core finding of higher proportional crime spending in diverse cities is. It also analyzes a single decade, demonstrating that the crime–diversity relationship persisted even in the 1990s. This period is deliberately chosen as a novel and hard test since conditions in many American cities improved markedly over this decade. Crime rates dropped by

Table 4. Testing Potentially Omitted Variables

Model includes	$\beta_{Diversity90}$	*SE*	$\beta_{Other\ variable}$	*SE*
% Immigrant 1990	0.029	0.015	–0.069	0.030
% Black 1990	0.035	0.017	–0.019	0.018
% Black squared 1990	0.031	0.015	–0.043	0.026
% Hispanic 1990	0.027	0.014	–0.009	0.018
% Hispanic squared 1990	0.026	0.015	–0.003	0.023
Δ % Immigrant 1990s	0.037	0.016	–0.112	0.067
Δ % Black 1990s	0.026	0.015	0.037	0.055
Δ % Hispanic 1990s	0.037	0.015	–0.114	0.052
Taxes per capita 1992	0.026	0.015	0.010	0.008
Index of responsibilities 1992	0.027	0.014	–0.001	0.002
South	0.022	0.015	0.006	0.006
West	0.028	0.014	–0.008	0.007
Midwest	0.025	0.015	–0.004	0.006
Imputed data				
Crime rate 1991	0.024	0.016	0.362	0.900
Δ Crime rate 1991-1999	0.028	0.015	0.371	1.294
Ward elections	0.029	0.014	–0.005	0.008
Council-manager	0.029	0.014	–0.001	0.005
Partisan elections	0.031	0.014	0.010	0.007
Fixed effects	0.027	0.015		

Note: In the left two columns, this table provides the impact of the 1990 level of diversity on changes in city criminal justice spending from 1992 to 2002, conditional on the potential confounders outlined in endnote 18. Each line represents another multilevel model where the variable listed on the right-hand side is included as one additional covariate. The right two columns report the coefficient and standard error for the newly included covariate. The crime rate coefficients were multiplied by 10^6.

17% on average, and cities' median household incomes grew by 4% after adjusting for inflation. Thus the results—which hover at or near conventional levels of statistical significance—are taken as yet more evidence of how diversity influences anticrime spending.

Another way to probe the robustness of the results is to include potential omitted variables in the basic model specification and then see how the coefficient on the key variable of interest—1990 levels of diversity, in this case—changes. That is precisely the set of results presented in Table 4. These models use the full set of independent variables listed in endnote 18, specified both as baseline 1990 levels and as changes during the 1990s.[28] For 19 different model specifications with potentially omitted factors included one at a time, the table presents both the estimated coefficient for diversity (the first two columns) and for the

omitted variable in question (the third and fourth columns). For instance, an alternative explanation in line with realistic group conflict is that the result stems from rising crime rates. If diversity is related to crime, then higher criminal justice spending might be a natural result.[29] Yet the table shows that the 1991 crime rate is not in itself a significant predictor of the share spent on criminal justice, and that the coefficient on diversity is only slightly smaller (0.024) when it is included. The same is true when we examine diversity's influence conditioning on the *change* in crime rates.

In fact, Table 4 demonstrates that the core result is robust to a wide range of potentially omitted variables, including measures of cities' political institutions and their tax levels. To be sure, the impact of diversity is dampened when we condition separately on an indicator variable for Southern cities, but that makes sense. Their distinctive racial composition might partly explain higher levels of anticrime spending.

Aside from the general robustness of the finding that diverse environments saw larger increases in their anticrime budgets, Table 4 provides a few other results of note. First, the impact of the level of diversity is not especially dampened when we condition on the percent Black or the percent Hispanic, so again we should not see this finding as masking the particular impact of a single group. That said, an influx of Latinos or immigrants does appear to dampen spending on crime, perhaps because those populations have other needs or preferences. Notice, too, that the coefficient for the squared proportion of African Americans is negative and near conventional levels of significance. Those cities that have considerable Black populations, and thus can achieve significant Black political representation, see smaller increases in their proportional crime spending. This is strong evidence against the alternative hypothesis that African Americans demand higher levels of anticrime spending. The estimated impact of diversity is not simply a proxy for African Americans' political preferences.

The results were probed in a wide variety of other ways, including with more limited model specifications and with fixed-effects models. Again and again, the general pattern of results emerged. Diversity has no effect on most types of spending, including those that support parks, libraries, and other shared spaces. Criminal justice spending remains an important counter-example since we have consistent evidence that cities with higher levels of diversity in the 1970s, 1980s, and 1990s saw disproportionate increases in the share of their budgets devoted to criminal justice.

Of the hypotheses discussed, the politicized places hypothesis is borne out most consistently by the data, since only it predicts that spending on criminal justice would remain affected by diversity as recently as the 1990s.

The politicized places approach can also explain why changes in local demographics have an impact even conditional on levels of diversity, as we saw above: demographic changes generate attention, uncertainty, and are one factor that can politicize ethnic divisions. This represents an important departure from past work since it suggests that it is often uncertainty rather than realistic group conflict that leads ethnic divisions to be incorporated into politics.[30] The arrival of an out-group can reshape local politics even if that out-group has little chance of obtaining political power or redirecting other resources.

The hypothesis about changing attitudes is not helpful in making sense of the patterns uncovered. It fails to explain shifts in criminal justice spending, and cannot make sense of the weak relationships between diversity and key spending categories in the 1950s and 1960s. Furthermore, since we do not observe many impacts for African Americans or for Latinos separately, it is hard to argue that this simply reflects a shift from a biracial politics to a multiracial politics. Still, the influx of Latinos could shape the national agenda on issues of difference and diversity, and so have an influence on local politics through that mechanism.

Discussion and Conclusion

Certainly, there are limitations to this study. With spending data that are measured only at 5-year or 10-year intervals, and with increasingly sparse data as we examine earlier periods, it is impossible to identify year-to-year changes. Also, since we only observe four discrete time periods, there are surely alternative explanations for why diversity's impact varies. On their own, the longitudinal results linking the change in diversity's impact to national political rhetoric are at best suggestive. To further probe them, future research should collect year-to-year measures of not just attention to crime and others issues but their racialization as well. Where possible, it should also be attentive to the distribution of public goods *within* cities.

Even so, this article has compiled thousands of available measures from five decades to demonstrate that beneath the cross-sectional finding that diverse localities spend money in distinctive ways lies a more complicated and dynamic reality. Results since 1950 demonstrate that *the dire conclusions of past diversity research are overstated, time-bound, or incorrect*. Ethnic and racial diversity does not consistently dampen the provision of public goods. Indeed, in recent years the impact of diversity has been inconsequential for most spending categories and positive for health. Even during the racially charged period of the early 1960s, those cities with rising Black populations did not demonstrably differ in key categories such as roads or criminal justice. This is strong evidence

against any notions of diversity that are time-invariant and that predict a consistent dampening of public investments in diverse locales.

As one potential explanation for these patterns, this article presents a new hypothesis about local politics that emphasizes their dependence on national politics for some of their key divisions. Even ethnic and racial divisions, a fundamental cleavage in local politics (Hajnal, 2007; Kaufmann, 2004), could be influenced by national politics. Those issues that are consistently connected to race and ethnicity at the national level are most responsive to demographics at the local level. This approach alone explains why it is criminal justice, and not any other spending category, that is the most commonly influenced by local diversity. The notion that local politics have national origins also has broader implications. As one example, future research might probe the extent to which local political mobilizations are displaced responses to national stimuli. The article also provides a new perspective on the consistently inconsistent findings on racial threat and racial contact. Perhaps scholars have been unable to agree on the effects of racial and ethnic contexts precisely because those effects have varied over recent years. What is true for one place at one time may not generalize.

We return to the question that motivated this research: what is the likely impact of today's influx of immigrants on public investment in U.S. localities? Clearly, the influx of immigrants will not categorically widen the gaps between diverse and homogeneous communities that are already visible. The massive increase in diversity in the 30 years from 1970 to 2000 did not lead to a widespread change in local spending. On most spending categories, if current trends continue, the impact of increasing diversity will be marginal as well.

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Notes

1. But also see Marschall and Stolle (2004), Oliver and Mendelberg (2000), and Hopkins (2009).
2. Throughout, the term *public investment* is used to capture both public goods and redistribution, and *diversity* refers only to ethnic and racial diversity.
3. This figure is from a Google Scholar search on December 24, 2009. A similar Web of Knowledge search reports 196 citations.
4. The data to replicate these analyses and the online appendix are available at www.danhopkins.org
5. Given high levels of racial segregation in the U.S., an ideal test of diversity's impact on city spending would focus on spending by neighborhood. However, no such data exist for most spending categories.
6. See also Fox (2004).
7. See also Liska et al. (1981) on the relationship between crime rates and concerns about crime.
8. Temporary Aid to Needy Families and other means-tested programs are also redistributive, but they are chiefly federal and state programs, and so are excluded from consideration here.
9. Readers interested in schools should instead see Berkman and Plutzer (2005) and Poterba (1997).
10. Due to data availability, analyses prior to 1970 include only operating expenses, although changes across years are always evaluated between comparable measures.
11. Formally, it is defined as $\Sigma_{i=1}^{n} p_i^2$, where p_i is the proportion of the population in each of the i groups.This analysis uses four mutually exclusive groups that account for the vast majority of American residents: non-Hispanic whites, non-Hispanic blacks, non-Hispanic Asian Americans, and Hispanics. Non-Hispanic Native Americans represent such a small fraction of the urban population that the addition of that group to the index is almost undetectable: the five-category index with Native Americans and the four-category index without are correlated at more than 0.999. Strikingly, the correlation is 0.985 when analyzing just Blacks, Hispanics, and non-Hispanic whites.

12. Using OLS for all cities or counties makes the prior assumption; estimating impacts separately within each state makes the latter assumption.
13. The initial dependent variables are spending shares, and thus are related to one another, since an increasing share of spending on roads must imply a decreasing share of spending in another category. The analyses were replicated using the procedure for compositional data found in Katz and King (1999), and later analyses were replicated using Seemingly Unrelated Regression. The results are not notably different. The reason is simple: there are dozens of city spending categories, from those covering public utilities to those covering airports. Only a small subset of the largest spending categories receive our attention here. As a result, the money shifted to a given spending category could have come from many different places.
14. Most judicial functions are funded by counties and states, meaning that city-level criminal justice spending is on average 90% police spending. The share of operating expenditures devoted to the police correlates with that devoted to criminal justice overall at 0.99. Thus the results would be no different if we analyzed only police spending.
15. Three of the cross-sectional models do not rely on data from the 1970s, and so include federal aid per capita as well.
16. All dollar figures reported are standardized to 2002 dollars using the Consumer Price Index.
17. Even given these improvements, it is worth noting that the problem of unobserved differences in federal and state mandates will be most pronounced for areas including housing, roads, transit, and health spending, and far less pronounced on core local functions such as policing, sanitation, fire, parks, and libraries.
18. Alongside a measure of diversity, their independent variables are the logged population, income inequality, the percent of the population over 65, household income, and the percent with a bachelor's degree. This analysis adds other potential confounders, including the percentage of homeowners, the percent poor, the percent in urban areas, the median home value, and measures of the locality's functional breadth and intergovernmental aid per capita. All are valuable. For instance, in communities with fewer homeowners, property taxes are less visible, and so local tax rates might be higher (Oates, 2005). The percent poor is a proxy for the level of need in the community and the percent in urban areas should capture functional differences between urban and rural areas. Given heavy reliance on property taxes, local home values are another constraint on local budgets.
19. Of the new covariates, only the percent poor and state aid per capita are added to the 1970s model, since the others are not available that far back. Also, the percentage with a bachelor's degree is unavailable for 1970. However, the pattern

of results does not change noticeably if all models for all years are limited to the covariates available for the 1970s.

20. One complication comes from the residential segregation that is pronounced in many American metropolitan areas (La Ferrara and Mele, 2006). Given high levels of segregation, residents might know that they will not have to share public goods like parks, eliminating this potential mechanism. Using dissimilarity indices from Cutler et al. (2001), this paper explored whether homogeneity interacted with the level of segregation in the community for 459 large cities in the 2002 data. We see such an interaction only when the dependent variable is the share of spending on parks: there, the impact of diversity is less negative given high levels of segregation. Still, there is no main effect.
21. In both cases, "substantial" was defined as shifting from the 20th percentile to the 80th percentile.
22. These results for criminal justice spending hold even in a fixed-effects model, where we place no constraints on the state-level intercept.
23. To address the possibility that these results are driven by homogeneous cities with tiny minority populations, the analyses were re-estimated for the subset of 371 cities that were more than 10% black in 2000. The findings on criminal justice spending grow slightly stronger as well as more uncertain. The estimated effect of the 1970 level of diversity is 2.9 percentage points, with a 95% confidence interval from -0.3 percentage points to 6.2 percentage points. The estimated effect of the change in diversity over this time period is similar, at 2.6 percentage points (0.4, 4.7). However, the health finding is no longer substantively or statistically significant in this subsample.
24. The pattern is substantively quite similar if we focus on changes in operating expenditures, with one key difference: rising diversity predicts increasing operating expenditures on roads.
25. For the 1970 percent Black, the 20th percentile is 0.2% and the 80th percentile is 14%. For changes from 1970 to 2000, the respective figures are 0.4 percentage points and 8.2 percentage points. For the 1970 percent Hispanic, the 20th percentile is 1.1% and the 80th is 17.0%. There, the respective figures for changes are 1.2 percentage points and 15 percentage points.
26. Specifically, the data compiled for 1950, 1960, and 1965 by the *County and City Data Book* do not include capital expenditures, making it impossible to compare the 1965 figures directly with those from subsequent years reported above. Also, looking only at operating expenditures, there is a noticeable drop in the Pearson's correlations when comparing 1965 and 1973 to other periods of similar length. Hence we analyze the two data sets separately.
27. Here, that shift in the African American population share is from 0.7 to 15.7 percent black. For changes in the black population share, that same shift means

comparing a city that saw no change to one that saw an in-flow of 3.4 percentage points from 1950 to 1960.

28. A total of 112 cities are missing observations for the change in the crime rate over the 1990s, so those missing rates were multiply imputed (King et al. 2001). The analysis also used multiple imputation to impute binary institutional indicators for the 417 cities where such information was not available via the 2001 Form of Governments survey.
29. To some degree, this rests on the mistaken impression that diversifying cities are cities in trouble; in fact, cities that were diversifying saw higher income growth in the 1990s.
30. Hajnal (2007) makes parallel claims about the central role of uncertainty during elections when blacks contend for the mayoralty.

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