Local public goods and ethnic diversity: Evidence from the Immigration Reform and Control Act[+]

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Abstract: This paper uses county-level data from California to test whether ethnic fragmentation and other measures of diversity and social capital are systematically related to spending on productive local public goods that affect rural quality of life. The specific focus of this paper is the impact of the Immigration Reform and Control Act (IRCA) of 1986, which brought about 400,000 new immigrants to California, on demographic composition in that state. This policy change was largely unexpected and created exogenous variation in the ethnic diversity of counties in California, particularly those counties where agricultural land use was most prevalent. Our econometric results suggest a negative correlation between ethnic fractionalization and local public good expenditure. In this context we also find some evidence that civic participation, as measured by voting rates, has the opposite effect of fractionalization on local public good outcomes. Potential implications of this finding for immigration policy reform are briefly discussed.

Keywords: **local public goods, ethnic diversity, immigration, California**

1 Introduction

The Central Valley region of California has been characterized as a region of “poverty amid prosperity” (Martin and Taylor 1998). This region is the source of much of the US’s high value agricultural production; eight of the nation's top ten counties by value of agricultural production are in California and six of these are in the Central Valley (the region composed of the San Joaquin Valley and neighboring Sacramento Valley) (USDA/National Agricultural Statistics Service 1997). Poverty is also prevalent in this region, by both California and national standards. The percentage of persons living in poverty in Fresno and Tulare counties (at the heart of the San Joaquin Valley), for example, is comparable to that in metro Baltimore or Philadelphia. A larger fraction of families live in poverty in these counties than in Mississippi, Louisiana, or West Virginia (US Department of Commerce, Bureau of the Census 2000).

Poverty rates in the Central Valley are closely related to the region’s concentration in agriculture. About 800,000 people are employed as farmworkers in California at some time in the year, with annual incomes of $7,000-$8,000 (Martin 2003). These farmworkers and their families, which make up much of the Valley’s poor, are largely foreign-born Latinos (about 95 percent of farmworkers in California are foreign-born), many of whom are not authorized to be employed in the US (approximately 50 percent of farmworkers lack work authorization) (US Department of Labor 2000).

This paper considers local public good outcomes in the context of agricultural communities in the US. We focus on counties in California with significant agricultural production and investigate empirically the determinants of local public good expenditures per capita. In these counties, with large numbers of immigrants and illegal residents, the community characteristics most salient to voters and residents may differ from those that determine local public good expenditure in urban areas. Our empirical analysis is motivated by a simple model of discriminatory community preferences that is closely related to the model developed by Cutler, Elmdorf, and Zeckhauser (1993) and yields testable predictions consistent with those arising from other models of voting behavior (e.g., Alesina, Baqir and Easterley 1999, among others).

Inefficiently low public good provision may directly affect community welfare (e.g., public health infrastructure) and longer-run economic development prospects (e.g., education).[1]As well as the public finance literature that considers how heterogeneous populations provide local public goods (e.g., Gramlich and Rubinfeld 1982, Rubinfeld 1987), there is a growing body of empirical work that investigates whether local public good outcomes are related to income inequality and ethnic polarization or fractionalization (Vigdor 2004, Miguel and Gugerty 2002, Alesina, Baqir and Easterley 1999, Poterba 1996, Glaeser, Scheinkman and Schleifer 1995, Cutler and Glaeser 1997, Cutler, Elmdorf, and Zeckhauser 1993). In the US, nearly all of this work is motivated by adverse outcomes in racially polarized cities and suggests that more fractionalized or polarized communities have worse local public good outcomes.[2] In contrast, this paper considers relatively rural communities which contain large numbers of recent immigrants. Our motivation is similar to that of other work however; we seek to address

[1] In the context of developing countries, ethnic diversity or fractionalization and other measures of relatively low social capital appear to be correlated with adverse economic and institutional outcomes at the national level (Easterly and Levine 1997, Mauro 1995, Knack and Keefer 1997)

[2] The agricultural counties of California may be as "fractionalized" as the metropolitan areas of major US cities. By income, education, language, and ethnicity, farmworkers and other recent immigrants differ on average from landowners, farm managers, and other generally native-born residents of the Central Valley. Just as Alesina, Baqir, and Easterley (1999) note that popular discussions often compare American cities to "Third World countries," similar language can be found in the popular press when discussing California's Central Valley (e.g., Kassler 2000).

the question of whether demographic characteristics of communities are connected to economic outcomes.

By focusing on counties in a single state we gain access to a relatively long time series of county-level data (covering the years 1979-2000). The time period covered by our data includes a significant policy change, which allows us to use the econometric techniques of program evaluation to treat the endogenous relationship between ethnic diversity, income, and local public goods expenditure. Much of the existing empirical evidence on this question is hampered by concerns about reverse causality. Because of the possibility of Tiebout (1958) sorting, it is difficult to determine whether ethnic diversity, or other measures of heterogeneity, cause a particular outcome to occur (e.g., lower provision of local public goods) or if the fact than an outcome occurs encourages increased fractionalization. This paper attempts to overcome this difficulty. It capitalizes on the fact that in counties in California in which immigrants are a relatively large fraction of the population immigration policy reform created an exogenous variation in the ethnic diversity of residents during the period we study.

The Immigration Reform and Control Act of 1987 (IRCA), gave temporary and ultimately permanent residence to approximately 700,000 so-called Special Agricultural Workers (SAW) in California over a period of three years (US Department of Labor 1993). Perhaps 400,000 beneficiaries were new residents as the amnesty was not restricted to people currently residing in the US and the ex ante prediction of the number of applications was about 300,000 (Orrenius and Zavodny 2003).

Our identification strategy uses the fact that the SAW program acts an exogenous source of variation in ethnic fractionalization in California counties. In particular, our econometric strategy relies on the fact that more SAW participants settle in regions where a larger fraction of land is devoted to agriculture. We use this variation in program intensity across counties and the timing of the amnesty to create instruments for ethnic fractionalization. The validity of the identification strategy is tested.[3]

Our results are generally consistent with the existing literature that has identified a negative correlation between ethnic fractionalization and local public good expenditure. In this context we also find some evidence that civic participation, as measured by voting rates, has the opposite effect of fractionalization on local public good outcomes. As an alternative measure of social capital (Coleman 1990, Putnam 1993), this is consistent with the findings of Miguel and Gugerty (2002) that communities that are able to impose internal social sanctions choose higher levels of public good provision. This finding may also have implications for assessment of alternative future immigration policies, a possibility to which we return in the conclusion of this paper.

The remainder of this paper is organized as follows: Section 2 presents a simple model of individual preferences that predicts a correlation between community characteristics and local public good provision. Section 3 discusses the data used in econometric analysis and IRCA, a major policy reform implemented during the period under consideration. We discuss threats to the validity of the identification strategy and

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[3] Impacts of immigration shocks and policy changes have been studied by Card (1990) and Saiz (2003), who investigate the impact of the Mariel Boatlift on labor and housing market outcomes in Miami. The effect of IRCA on farm labor market outcomes and, indirectly, agricultural commodity prices has been studied by Tran and Perloff (2002), Gunter, Jarrett and Duffield (1992) and Taylor and Thilmany (1993). This work is largely motivated by testing whether the stated goal of IRCA—creating a smaller, more legal farmworker labor force—was achieved. We discuss briefly assessments of IRCA in this respect later in this paper. Our identification strategy is related to that used by Banasak and Raphael (2001) to investigate whether the penalties imposed on firms for hiring illegal immigrants as a result of IRCA resulted in discrimination against Latino workers. They use that fact that penalties for hiring undocumented workers were phased-in at different rates in the agricultural and non-agricultural sectors to create differential treatment groups.

robustness checks. Section 4 presents our econometric strategy and results. Section 5 concludes.

2 Model

To motivate our econometric analysis of the impact of ethnic fractionalization on local public good expenditure outcomes we present a simple model that is related to a model of spending on local public goods first proposed by Cutler, Elmdorf, and Zeckhauser (1993). In this model, individual utility is a function of consumption (income less taxes) public goods (financed through income taxes and provided at the level demanded by the median voter) and, crucially, community welfare where the welfare of members of different groups within the community may be weighted differently. We discuss comparative statics of the demand for local public goods by a representative agent in the case in which there are two groups in a community, perhaps immigrant farmworkers and all other individuals, and the relative proportion of group members changes though the identity of the median voter does not. We also consider a case in which all consumers pay an equal share of taxes and a case in which one group caries a larger portion of the tax burden. The model provides one possible justification for a correlation between local public good expenditure patterns and community characteristics.

Consider a community of fixed population in which there are two groups of individuals, J_1, of size n_1 and J_2 of size n_2 where $n_1 + n_2 = N$, the total population of the community. There are K local public goods provided in the community in total and at least a portion of the benefits associated with these goods are private (i.e., there is a redistributional element to these goods) but the level of benefits do not differ across groups. For a particular good k, a member of group J_1 receives benefits g_{1k}.

Tax revenue is used to pay the cost of providing all K goods to groups J_1 and J_2 and totals the amount G_T. For simplicity, we assume that each member of the broader community pays an equal fraction of taxes, which implies that the group J_1 will pay $(n_1 / N) * G_T$ in total. Thus, total expenditure on local public goods in the community can be calculated by summing across the consumption of goods for individuals in each group and then summing the consumption of groups J_1 and J_2.

To determine the circumstances under which community characteristics may affect local public good expenditure even when all individuals value the same local public goods, we can write down a utility function of the following form:

$$u_1 = U(y_1 - t_1, \boldsymbol{G}_1, c_1). \tag{1}$$

Here, the utility of an individual in group J_1 (the group is denoted by numeric subscripts) is a function of after-tax income ($y_1 - t_1$), the vector $\boldsymbol{G}_1$, in which each element is the amount of good k, g_{1k}, that a member of group J_1 receives, and a community welfare function c_1.[4] Group J_2 individuals have analogous utility functions.

Cutler, Elmdorf, and Zeckhauser (1993) show that the functional form of c_1 determines whether community characteristics affect local public good outcomes in this model. One case in which this will occur is when agents exhibit discriminatory preferences and the function c_1 takes the following form:

$$c_1(n_1,n_2,G) = w_{11}n_1v_1(\boldsymbol{G}_1) + w_{12}\, n_2v_2(\boldsymbol{G}_2). \tag{2}$$

[4] The inclusion of c_1 in the individual utility function makes agents in this model different from the purely selfish agents in the traditional public choice model. Note that if c_1 is not an element of the utility function then member of group J_1 will desire that public good be provided until the marginal utility forgone by paying an additional $1 of taxes is equal to the marginal benefit of the public good k that can be purchased with $1. Mathematically, for good k this will occur when $U_y(n_1 / N) = U_{g1k}$, where the subscripts on the U function indicate derivatives.

In this case, for i = 1,2, w_{1i} is the weight that each member of group J_1 places on the welfare of group J_i and $v_i(G_i)$ is the benefit to each member of group i who receives $\boldsymbol{G_i}$. For an individual in group J_1, desired spending on good k occurs at the point where:

(3) $$U_y(n_1 / N) = U_{g1k} * U_c (w_{11}n_1v_{1gk}(\boldsymbol{G_1}) + w_{12}\, n_2v_{2gk}(\boldsymbol{G_2})).$$

In the case that $w_{11} = w_{12}$, preferences are not discriminatory. When these weights are not equal, agents weight the welfare of people outside their group differently from people within their group.

To investigate the implications of this model for agricultural counties in California, we consider three scenarios. First, consider the case in which the identity of the median voter, whose preferences determine aggregate spending on all K goods, is like that of an individual in group J_1 and does not change if n_1 and n_2 change (perhaps because individuals in group J_2 cannot vote). In this case, as the proportion of individuals in group J_2 increases (holding the total population fixed), desired, and realized, spending on good k falls if $w_{11} > w_{12}$.

A second scenario occurs when the tax share of individuals in group J_1 is larger than the tax share of individuals in group J_2 (perhaps because the members of group J_1 are more likely to own property). If $w_{11} > w_{12}$ desired spending on good k will fall relatively faster than in the first scenario as the proportion of individuals in group J_2 increases.

There is a third possibility that increasing the proportion of individuals in group J_2 shifts the identity of the median voter towards individuals who place a greater weight on the welfare of group J_2. Discriminatory preferences imply declining local public good spending with increasing diversity, by at a slower rate than in the first scenario.

3 Data and program description

3.1 Data

The data used in this paper are a set of county-level statistics for 35 counties in California for the period 1979-2000. The counties in the sample are those for which the value of agricultural production exceeded $100 million in 2000 and, essentially, include all counties in California except the metropolitan core of the Bay Area, timber growing regions of the North, and the largely mountainous eastern edge of the state. All counties that were estimated to have more than 10,500 farmworkers residing in them in 1990 are included in the sample (Bugarin and Lopez 1998). Counties with the highest value of agricultural output have among the highest poverty rates in the state as well.

County financial data, including expenditure on roads, expenditures on sanitation and water infrastructure, and levels of transfers from the Federal and State government (revenue that is often earmarked for particular projects or types of projects) comes from the California Institute for County Government (CICG) for the period 1985-2000.[5] CICG also makes available data on county government revenue by source (e.g., sales or property taxes), but since counties in California have little control over revenue levels (Hill 1998), we do not use this data.[6] We do use information from CICG on whether a county is a "general law" or "charter" county; terminology that distinguishes two possible ways of electing local government in California. Charter counties have somewhat more freedom about the offices that will be filled by election as opposed to appointment and thus spending in these counties may be relatively more responsive to local tastes. Summary statistics are shown in table 1; transfers per capita are much higher than road

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[5] All monetary data is deflated to constant 1995 dollars using the Los Angeles regional Consumer Price Index as reported by the Bureau of Labor Statistics.
[6] Future versions of this paper will analyze water and sanitation expenditure in detail. This analysis is omitted here due to space constraints.

expenditure, which is in turn much higher than water and flood control expenditure. There is significant variation across counties.

County demographic characteristics (also summarized in table 1) come from the US Census City and County Data Books (fraction of the population below the poverty line, fraction of the population with a college degree, and number of votes cast in elections), the California Department of Finance (DOF) (population by age and ethnic classification[7] and population living in unincorporated areas of counties), RAND California (violent crimes per 100,000 population, personal income), and the USDA/National Agricultural Statistical Service (harvested acres).[8] The data from the City and County Data Books is not available for all years that we study, so we include in our analysis information about the income distribution, voting, and education attainment at the outset of the period.

Ethnic fractionalization, a standard measure of ethnic diversity in the empirical literature on this topic is calcualted as $1 - \Sigma_i(Race_i)^2$ and is defined as the probability that two people picked at random in a region will be of different ethnicities.[9] There is noticeable state-wide upward trend in ethnic fractionalization, though for at least some counties there appears to be a change in this trend in the mid to late 1980s. This coincides

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[7] Unlike the US Census data, "Hispanic" is included as an explicit category in the DOF's reported population by ethnic group data. The categories included are: white, black, Asian, Hispanic, and American Indian. These categories sum to the total population figures.

[8] In non-census years DOF population estimates are generated by analyzing driver's license data and birth and death certificates. Thus, it may undercount illegal residents as California law requires proof of legal residency to receive a license to drive. However, personal income data from RAND California is based on estimates made by the Bureau of Economic Analysis (BEA). The wages and salaries component included in this calculation of personal income is generated from data supplied to BEA from state employment security agencies (ESAs) that summarizes quarterly state unemployment insurance contribution reports filed with ESAs by employers. For California, these estimates include farmworkers because state law requires that farmworkers be provided with unemployment insurance (in contrast to all other states except Arizona) (BEA 2001). Thus, we suspect that illegal residents are generally excluded from the population data and generally included in the income data.

[9] Vigdor (2004) derives this fractionalization index, first introduced in the economics literature by Mauro (1995) as an element of an individual's "decision to act" to provide a local public good. See Alesina et al. (2003) for further discussion of measures of diversity in econometric analysis.

with the period in which the IRCA was passed and amnesty was provided to certain agricultural workers, an exogenous source of change in the ethnic composition of the sample counties that we exploit in our econometric analysis.

3.2 The Immigration Reform and Control Act

Among other legislative changes, IRCA gave amnesty to Special Agricultural Workers (SAW), people who had worked as agricultural workers for at least 90 days between May 1, 1985 and May 1, 1986 or for a total of 90 days during the period May 1, 1983 and May 1, 1986. Under the SAW provisions, a total of about 1 million applications for legal permanent resident status were granted, about 700,000 of these were in California (US Department of Labor 1993). At both the national and state level, applications far exceeded predicted levels (about 300,000 in total) leading observers to conclude that the program gave residence to many people living in Mexico in 1986 (Martin 2003, Orrenius and Zavodny 2003).[10] The time line of the IRCA SAW reforms is summarized in table 2.

IRCA had a limited impact on the composition of the electorate relative to the number of people who received amnesty under the program. While temporary residency permits were provided in 1988, and employer sanctions were in place in 1989, the vast majority of IRCA permanent residency approvals were given in 1991 (see table 2). Citizenship through naturalization became possible for participants in 1994. Thus, IRCA did not change the identity of the median voter prior to that year. Even after 1994, not all IRCA long-term permanent residents eligible for citizenship elected to naturalize. As of 2001,

[10] IRCA also introduced penalties for employers that were found to be "knowingly" employing undocumented workers. The amnesty was intended to act as a cushion against predicted labor shortages as a result of these sanctions (Tran and Perloff 2002, GAO 1989).

only about one-third of Mexican-born IRCA immigrants had elected to become US citizens (Rytina 2002).

Long-run impacts of the SAW program appear to differ from the short run effects of the program. Surveys conducted in 1989-91 found that 10 percent of the farmworkers in California were undocumented and about 60 percent of farmworkers were SAW program participants (US Department of Labor 1993). The more farmworkers living in a particular county, the greater the impact of the SAW program would be on the average status of a county's residents.

By 1998, Department of Labor surveys found that about 50 percent of farmworkers were undocumented (US Department of Labor 2000). This is consistent with the contention that SAW program participants gradually moved out of agriculture and were replaced with new undocumented workers (Martin 2003, Taylor and Thilmany 1993). However, using the Labor Department survey data, Tran and Perloff (2002) find that SAW participants are somewhat more likely than undocumented workers to remain agriculture and that IRCA did relatively little to change the dynamics of the agricultural labor market; it simply gave residency rights to a fairly large group of relatively similar immigrants in a short time period. This suggests that the identification strategy we use in this paper will be valid for the period around the time of the reform. However, as more SAW participants move out of agriculture, there will be less correlation between the ethnic diversity of citizens and residents of a county and land devoted to agriculture. We consider this issue further in econometric analysis.

If IRCA was an endogenous policy response to changes in ethnic diversity in agricultural regions, this may impact the validity of our estimation strategy. The stated goal of IRCA was to reduce reliance on new immigrants for labor in the agricultural sector (GAO 1989). Importantly for our identification strategy however, there appears to have been little expectation that the program would change the ethnic composition of the farm labor force; it was hoped that the SAW program participants, overwhelmingly Mexican in origin, would continue to work in agriculture.

The validity of our estimation strategy is also impacted if IRCA was anticipated by amnesty applicants or current residents. For example, suppose that people elected to work in agriculture in 1985 or moved to the US in 1985 because they anticipated it would increase their chances of receiving amnesty. Alternatively, if California residents anticipated future immigrants, this might have caused them to reduce local public good spending because of anticipated free-riding by future residents (Schultz and Sjostrom 2001). In either case we will underestimate the effect of IRCA on ethnic fractionalization by looking at changes in outcome variables before and after 1988. In practice, IRCA passage appears have been unexpected. Legislation to reform immigration policy that failed to become law was regularly before both the House of Representatives and the Senate prior to 1986 (Orrenius and Zavodny 2003).

Perhaps the most important threat to validity is the possibility of omitted interactions that make drawing inferences difficult. The data reflect a significant upward trend in fractionalization in all years in the sample and the pattern of increase in ethnic fractionalization could vary systematically across counties over time by agricultural land use for reasons unrelated to IRCA. If there were other policy changes at the time of IRCA that affect local public good expenditure in agricultural counties, our identification strategy would be inappropriate. There are several possible policy changes that occurred in this period that may affect public good expenditure; however none seem to affect communities differently depending on the importance of agriculture in the communities.

One candidate policy change comes from IRCA itself. Non-agricultural workers also received amnesty as part of IRCA. People unlawfully living in the US since 1982 were granted permanent residency regardless of sector of employment. About 1.5 million people received permanent residency as a result of this element of IRCA. These immigrants lived throughout the US; perhaps 38,000 of them worked in agriculture in California (US Department of Labor 2000). Most so-called pre-1982 immigrants worked in low-skilled labor or service sectors at the time of the amnesty and at the time they received permanent residence (Rytina 2002).

A state ballot measure called Proposition 99 raised cigarette taxes in 1989 in California with the additional revenue earmarked for spending on public health by counties additional to 1988 levels (California Legislative Analyst's Office 1995). The impact of this policy would vary across counties according to the volume of transactions eligible to be taxed. Thus, Proposition 99 does not disproportionately affect agricultural counties.[11] In light of this policy change however, our econometric analysis does not consider public health programs and we focus on expenditure per capita rather than local public goods spending as a fraction of total expenditure because counties in California received a new revenue source in 1988 as a result of the passage of Proposition 99.

[11] We test this proposition by calculating the correlation between the fraction of land devoted to agriculture in 1988 and the change in public health expenditure per capita between 1988 and 1990. The correlation coefficient is -0.0074, with a significance level of 0.95. Over this period, public health expenditure per capita increased by 10 percent on average.

4 Econometric estimation strategy and results

4.1 Empirical specification

Our estimation strategy focuses on explaining local public good expenditure per capita in California agricultural counties. The explanatory variable of primary interest is a measure of ethnic diversity. The model presented in section 2 predicts a negative relationship between ethnic diversity and local public goods expenditure per capita if people have discriminatory preferences. This effect should be smaller if the identity of the median voter changes as community composition changes.

Instrumental variables techniques are used to address the endogenous relationship between ethnic diversity and local public good expenditure. The primary instrument used in the first stage is the interaction of the variables "after IRCA" and the fraction of a county's land devoted to harvested agriculture. This is a valid instrument if (1) IRCA did not directly affect local public good expenditure, (2) the policy reform was an exogenous source of variation in county ethnic composition, and (3) the intensity of the intervention varied across counties with agricultural land use.

The main empirical specification is presented in equation 4. The outcome measure Y_{it} is defined as road expenditure per capita in county i at time t. We choose this class of local public goods because they have been the subject of study in other papers related to ours, thus making our results relatively comparable to other work (i.e., Alesina, Baqir and Easterley 1999). The variable $FRAC_{it}$ is a measure of local ethnic diversity. M_{it} is personal income per capita and $\boldsymbol{X}_{it}$ is a vector of county demographic and financial control variables. In some specifications we control for county fixed effects, (μ_i) to account for time-invariant county characteristics that may affect local public good expenditure levels (e.g., topography in the case of roads). We do control for time fixed effects ($\Sigma_t \tau_{t1} YEAR_t$) in most specifications, but also replace these fixed effects with a variable $AFTER_t$ that is an indicator variable that takes the value "1" if the year is after the IRCA reforms (we define this year as 1988 because this was the first year that permanent residency was awarded and the first year that employers faced sanctions for knowingly hiring illegal workers) in the simplest regression specifications. The disturbance term e_{it} is allowed to be correlated across years for the same county. Equation 4 is:

$$(4) \qquad Y_{it} = \mu_i + \beta FRAC_{it} + aAGAREA_{it} + cAFTER_t + \gamma M_{it} + \boldsymbol{X}_{it}` \delta + e_{it}.$$

Equation 4 estimated using ordinary least squares (OLS) produces biased estimates of β because the ethnic composition of a particular community are likely endogenously related to local public good expenditure. To treat this endogeneity, we estimate a first-stage regression, the main variant of which is presented in equation 5. A similar concern exists for the estimated coefficient on income and we also use IV techniques strategy to treat this endogeneity. We begin by focusing on explaining the instrumental variables strategy in detail in the case ethnic diversity. Equation 5 is:

$$(5) \qquad FRAC_{it} = \mu_{i1} + a_1 AGAREA_{it} + b_1(AFTER * AGAREA)_{it} + c_1 AFTER_t + \gamma_1 M_{it} + \boldsymbol{X}_{it}`\delta_1 + v_{it}.$$

In this specification (where the additional coefficient subscript of the number "1" is included to emphasize that this is the first-stage regression) we estimate the impact of the IRCA program on ethnic diversity in county *i* at time *t*, conditional on the other independent variables. This equation can be interpreted as a "differences" regression in which differential "treatment" groups are created depending on the fraction of land devoted to agriculture. The variable *AFTER*, defined above, can be replaced by time fixed effects to allow for year-specific impacts of IRCA. The variable AGAREA is measured as thousands of harvested acre per square mile in county *i* at time *t*.

4.2 Testing the first-stage identification strategy

As a first step to determining if this identification strategy is valid, table 3 presents a series of regressions that are closely related to equation 2. The specification is parsimonious; no $\boldsymbol{X}_{it}$ control variables are included. These regressions are intended to determine whether there is a change in ethnic fractionalization in the sample counties as a result of IRCA. Consider the regression in column 1; in this simple framework, the overall impact of IRCA is captured by the coefficient on *AFTER*, the differential impact on agricultural counties is captured by the coefficient on *AFTER * AGAREA* plus the coefficient on *AGAREA*, and the difference in outcomes between more agricultural counties and less agricultural counties prior to the policy change is captured by the coefficient on *AGAREA*. In this regression all coefficient estimates are positive, as expected, but only the coefficient on *AFTER* is significant.[12]

The remaining regressions in table 3 build upon the simplest specification, replacing *AFTER* with time fixed effects, including income per capita as a control variable, and, to address the concern that income per capita and ethnic fractionalization are themselves endogenously related, instrumenting for income with its five-year lagged value in the regressions in columns 4 and 5. In all specifications, the point estimates of b_1 are positive, as expected given the nature of IRCA, though not always individually significant. In regressions in columns 4, and 5, a_1 and b_1 are jointly significantly different than zero. After controlling for income per capita, these regressions provide some suggestive evidence that ethnic diversity in the sample counties was higher after IRCA than before

[12] Simply regressing ethnic fractionalization on a series of time fixed effects is another means of testing whether IRCA affected ethnic diversity. When we estimate this regression, we find that the coefficient estimates on the year dummy variables are positive and increase in absolute value at a decreasing rate. There is no obvious year in which the trend changes. This finding is robust to including income per capita and county fixed effects as additional explanatory variables and confirms the pattern seen in figure 1; increasing ethnic fractionalization in California counties prior to IRCA.

and the effect was greater in counties with a higher fraction of land devoted to agriculture.[13]

The key identifying assumption of our econometric model is that, in the absence of IRCA, $b_1 = 0$. This assumption should not be taken for granted. To test the identification assumption we take two complementary approaches, both of which are available to us because of the relatively long period of data available.

As a first test of the identification assumption, we restrict our attention to the data for the years prior to 1987 when IRCA had not been passed and run a series of "false experiments." That is, we estimated versions of equation 3 in table 3 for assuming that *AFTER* does not refer to 1988 but instead refers to the years beginning 1981-1986 sequentially. Finding significant coefficient estimates on the variable *AFTER * AGAREA* in these regressions would be evidence against our identifying assumption that a "treatment" occurred in 1988 that did not occur in earlier years. In these regressions, not presented due to space constraints, the coefficient on the variable *AFTER * AGAREA* is always insignificantly different from zero (the largest t statistic is 0.40 when the false treatment year is 1986 and the point estimate is 0.02) and is significantly different than the average of the point estimates on the interaction term in the regressions in columns 3-5 ($F(1,34) = 1.42$, Prob > F=0.24 when the false treatment year is 1986).[14] This is some

[13] To provide a sense of the order of magnitude of change in ethnic diversity implied by these estimates, consider the coefficient estimates in the regression in column 4. For a county of sample average fractionalization, income per capita of $20,000 in 1987 and 1988, and300 harvested acres per square mile in both years, these estimated coefficients imply a 4 percent increase in ethnic fractionalization as a result of IRCA.

[14] The sample size of the regressions for the period before IRCA is 215 county-year observations, significantly smaller than the sample size in the unrestricted regressions. However, if the regressions in table 4 are estimated with the period restricted to 1985-1990 (sample size 205 county-year observations), the significance of the coefficient on the interaction term is increased all cases, not reduced. Point estimates remain roughly similar to those reported in table 4. This is consistent with the discussion of IRCA in section 3.2; SAW program participants gradually move out of agriculture after 1988.

suggestive evidence that the identification strategy is reasonable though the coefficient estimates in table 3 are somewhat imprecise.

Our second approach to testing the identification assumptions underlying the regressions in table 3 uses all years of data. We estimate a regression of the form:

$$(6)\ FRAC_{it} = \Sigma_t a_{t1}` AGAREA_{it} + \Sigma_t b_{t1}` (AFTERYRX * AGAREA)_{it} + \gamma_1` M_{it} + \Sigma_t \tau_{t1}` YEAR_t + u_{it},$$

in which the single interaction term in equation 3 is replaced with a series of interaction terms (and coefficients now have a “prime” appended to them to differentiate their values from those in equation 5). This test relies on the fact that the estimated coefficient on variables that are the interaction of *AFTER YR X*, where *AFTER YR X* takes the value 1 for years other than 1988, and AGAREA should equal zero after controlling for *AFTER YR 1988*AGAREA* if 1988 is in fact the year in which exogenous change in ethnic fractionalization occurred. The estimated values of $b_{t1}`$ and the 95 percent confidence interval are plotted in figure 2. The estimates appear to be equal to zero for all years expect 1988 and 1994.[15] Because it provides support for the contention that an exogenous change occurred in 1988 did not occur in other years, this graph is additional support for our identification strategy.

Given the fairly imprecise coefficient estimates in table 3, our results may be sensitive to the choice of a function form (Meyer 1995). Figure 2 plots ethnic fractionalization against *AFTER*AGAREA*, conditional on harvested area, “after IRCA” and income per capita. The linear regression line corresponds to the OLS estimate of equation 2 excluding county fixed effects and the vector $\boldsymbol{X}_{it}$ (and the coefficient estimates

[15]Interestingly, 1994 is the year that Proposition 187 was passed, a state ballot measure that denied public services to illegal immigrants. While the proposal was never implemented, the passage of Proposition 187 encouraged many immigrants in California to change their status, becoming either permanent residents or citizens if possible (Public Policy Institute of California 1999).

reported in column 3 of table 3). I also present a locally weighted regression of these variables; the shape of this regression line appears to justify a quadratic specification rather than a linear specification. Thus, a specification of equation 2 that is quadratic in *AFTER*AGAREA* will be used in later sections of the paper.[16]

Finally, because of the relatively low F statistic (around 20) on the excluded exogenous explanatory variables, *AFTER*AGAREA* and $(AFTER*AGAREA)^2$ in regressions like those in equation 5, we include lagged ethnic fractionalization as an additional explanatory variable in the first-stage regression when we run regressions to explain the determinants of local public good expenditures. We do this to address the concern that, when using weak instruments coefficient estimates on the variable of interest, ethnic fractionalization, will be biased towards OLS (Staiger and Stock 1997). Because we have more excluded exogenous variables than included endogenous variables (see the discussion of the instrumentation strategy for income per capita below), we can use a test of over-identifying restrictions to test the exogeneity of the instruments.

A test of over-identifying restrictions is also important because of the possibility that the IRCA SAW program had a direct effect on public good expenditure and affected more agricultural counties disproportionately. This could happen as a result of congestion effects. Congestion can reduce the private benefits of local public goods and thus willingness to pay for these goods. New California residents were added as a result of IRCA, and anecdotally at least there is some suggestion that this was a concern in California.[17]

[16] The intuition for this quadratic specification is that relatively urban counties with little agricultural land area are more diverse that counties at the middle of the agricultural land distribution.
[17] For example, deterioration of quality as a result of congestion is one justification for proposals, like Proposition 187, to restrict immigrants' access to public services (Clark and Schultz 1997).

The strategy that we use to identify an exogenous source of variation in ethnic fractionalization is also a potentially plausible strategy to identify an instrument for income per capita because IRCA resulted in approximately 300,000 new residents of California who were relatively poor. However, other elements of IRCA also gave citizenship to existing California residents, whose wages likely increased in the short run as a result of this change in their residency status.[18] The net impact of IRCA on average incomes is theoretically ambiguous. We can estimate an equation analogous to equation 5 in which the dependent variable is income per capita and ethnic diversity is included as an explanatory variable. Table 4 presents a series of regression results in which the dependent variable is income per capita. The coefficient estimates of interest, on the variables *AFTER*, *AGAREA*, and *AFTER* * *AGAREA* can be interpreted as before. In this case, counties with relatively more land in agriculture are relatively poor, incomes were higher after IRCA, though somewhat less so for counties with more land in agriculture. This is consistent with our understanding of the SAW program, new relatively poor residents at least initially worked in agriculture.

Tests of the identification strategy for income proceed as outlined above for ethnic fractionalization. In this case however, there is reason to reject the hypothesis that an exogenous change occurred in 1988. For example, in figure 3 we present the coefficient estimates analogous to those in figure 1 in the case in which income per capita is the dependent variable. We see little evidence that 1988 differs from other years in the sample. Thus, we will proceed with a strategy in which income is instrumented for using

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[18] Kossoudji and Cobb-Clark (2002) estimate a wage premium of about 6 percent for legal immigration status among Latino workers using the IRCA reforms as a source of variation.

its lagged value, rather than relying on the IRCA program to identify exogenous variation in this variable.

4.3 Estimating the determinants of public good expenditure

Table 5 presents regression estimates of the determinants of public good expenditure using our instrumental variables identified in section 4.2. The first equation in column one is a simple regression of roads expenditure per capita on ethnic fractionalization and income per capita using two-stage least squares (2SLS).[19] The coefficient on ethnic fractionalization is negative and significant. The magnitude of this coefficient estimate implies that for a county with a measure of ethic fractionalization equal to the sample mean (0.46) and road expenditure per capita equal to the sample mean ($55), a five percent increase in ethnic fractionalization is correlated with an 8 percent decrease in road expenditure per capita. While a fairly small amount, average expenditure per capita is also fairly low in this sample. The coefficient on income per capita is also negative in the first regression. It is close to zero.[20]

The regressions in columns 2 through 5 introduce additional control variables to the basic equation. The choice of control variables was made so that our regressions could be largely comparable to work that has been done comparing outcomes across US cities. This allows us to understand whether determinants of local public good spending

[19]Two-stage least squares (2SLS) is consistent but inefficient in the presence of heteroskedasticity (Hanson 1982). In small samples, if heteroskedasticity is not present, 2SLS is preferable to GMM (Hyashi 2000). Thus, we test for heteroskedasticity as recommended by Baum Schaffer and Stillman (2002). In the regression in column 1 of table 6, the Pagan and Hall (1983) test statistic is 37.66. The null hypothesis of homoskedasticity is rejected at conventional significance levels. However, the coefficient point estimates are very similar when the regressions in table 6 are performed using GMM and are significant in every regression specification.

[20] For comparison, in the analogous regression estimated using OLS, the coefficient estimate on ethnic fractionalization is -168.9 (standard error 69.9) and the coefficient estimate on income per capita is -0.002 (standard error 0.001).

agricultural areas differ in important ways from those in cities. This approach should also increase the comparability of our results with those of other authors. In each regression, the test of over-identifying restrictions using Hansen's J statistic fails to reject the joint null hypothesis is that the excluded instruments are uncorrelated with the error term and are correctly excluded from the estimated equation. The estimated coefficient estimates on ethnic fractionalization and income are quite stable across specifications.

In column 2 we introduce variables that measure the freedom of county government to allocate spending between categories. Counties with higher transfers from other levels of government have higher spending on roads, while so-called home rule or charter counties have less. This suggests that earmarked spending is often targeted to roads; county governments that have more discretion choose to spend less on this class of public goods regardless of ethnic composition. It is also possible that government transfers are directed to the most ethnically diverse and/or poorest counties, or, alternatively, that transfers disproportionately flow to counties in which residents are citizens. Vigdor (2004) presents evidence that households living in homogeneous communities are more likely to complete the census forms. Census responses in turn have implications for the levels of transfers because they are partly determined by population estimates. When transfers per capita are instrumented for using their five-year lagged values, coefficient estimates are very similar to those in table 5.

In column 3 we introduce variables that account for some time-invariant characteristics of counties. We control for initial ethnic composition, education levels, poverty levels, and voting participation rates at the beginning of the sample period. While these variables are not strictly time invariant, education and poverty rates are likely to change relatively little over time and annual data on these county characteristics is not available. The most striking coefficient in this set is the strongly positive coefficient estimate on the fraction of the population voting in the 1980 election. One possible interpretation of this coefficient is that civic participation, or citizenship in particular, as measured by this variable, can counteract the negative effects of ethnic fractionalization on public good expenditures. This is consistent with the predictions of the model in section 2 in the case in which the identity of the median voter changes with average demographic characteristics of a community.

Additional control variables are introduced in column 4. We add controls for the fraction of the population over age 65, total population and the violent crime rate. The coefficient estimate on the fraction of the population voting remains positive and significant in this regression.

County fixed effects are included in the regression in column 5. Time-invariant control variables drop out of the equation in this case and all but one of the remaining control variables are insignificant. The estimated coefficients on ethnic fractionalization and income per capita are essentially unchanged from the initial specification.

5 Conclusions

This paper uses county-level data from California to test whether ethnic fragmentation and other measures of diversity and social capital are systematically related to spending on productive local public goods that affect rural quality of life. Our motivation is the "poverty in the midst of plenty" that characterizes California's Central Valley.

Using an instrumental variables strategy, we find that ethnic fractionalization is correlated with reduced local public good expenditure, but that increased civic participation or citizenship is correlated with higher levels of future public good expenditure. This may suggest that immigration policy reform that creates a class of guest workers will have more negative impacts on local public good expenditures than reform that provides the opportunity for naturalization.

Future extensions of this work should investigate the strength of the IV strategy more; in particular, the intensity of the IRCA intervention that we use to identify changes in ethnic fractionalization should vary across counties according to the labor-intensity of harvested crops. Other public good should also be considered. The model in section 2 is most appropriate for public goods that have a redistributional element, as such water and sanitation investments will be important to study.

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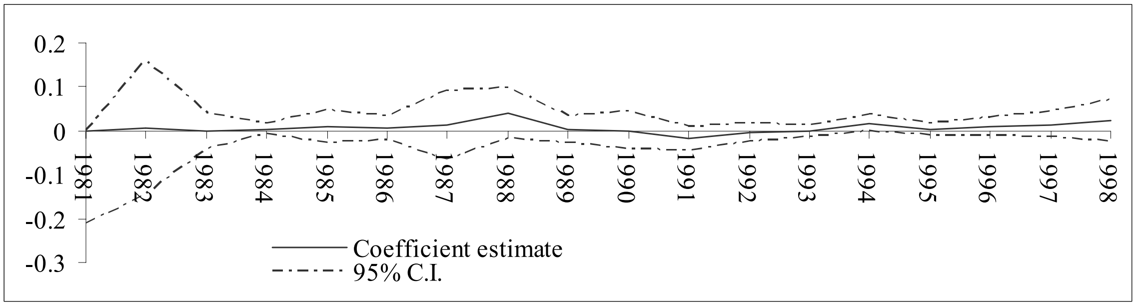
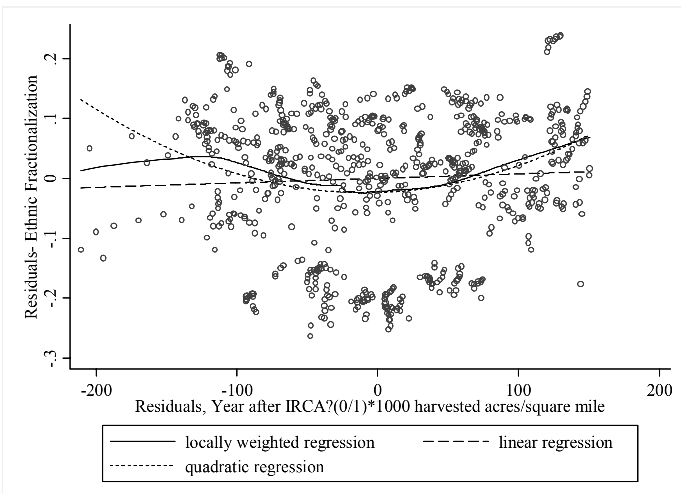
Figure 1: Coefficient on interaction terms in "false treatment" regressions (ethnic fractionalization)



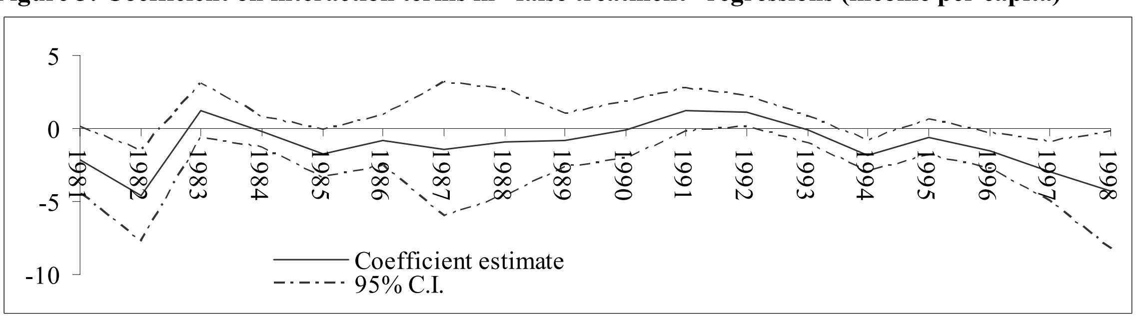
Notes: Graph shows the coefficients on the interaction term (Year after Year X?(0/1)*1000 harvested acres/mile^2) in a series of regressions that are identical to regression XX in table 4 Temporary residency was awarded to SAW program participants in 1987 and 1988.

Figure 2: Locally weighted regression- ethnic fractionalization



Note: Non-parametric regression conditional on harvested area, Year after IRCA (1989) and income per capita.

Figure 3: Coefficient on interaction terms in "false treatment" regressions (income per capita)



Notes: Graph shows the coefficients on the interaction term (Year after Year X?(0/1)*1000 harvested acres/mile^2) in a series of regressions that are identical to regression 3 in table 5 except that "after 1989"is replaced with year X =for all X= 1985-1999. The vertical line at year=1989 indicates the first year in which permanent residency was awarded to agricultural workers under IRCA. The last year of awards was 1992.

Table 1 Descriptive statistics

	Mean	Std dev.	Min.	Max.	Obs.
County financial characteristics					
Expenditure on roads per capita ($1995)	54.97	49.36	4.14	431.16	560
Expenditure on water/flood control per capita ($1995)	1.01	2.24	0.00	17.77	560
Transfers from state/Fed. Gov't per capita ($1995)	344.51	92.47	144.81	771.80	560
County has self-rule charter	0.26	0.44	0.00	1.00	770
County demographic characteristics					
Ethnic fractionalization	0.46	0.12	0.14	0.67	770
Income per capita ($1995)	21,212	4,903	13,951	55,064	770
Harvested acres (1,000) per square mile	0.30	0.16	0.00	0.71	735
Violent crime rate (per 100,000)	641	287	212	2,101	665
Fraction of population in unincorporated areas	0.37	0.19	0.05	0.78	770
Fraction of population over age 65	0.11	0.02	0.07	0.18	770
Log total population	12.45	1.41	9.46	16.10	770
Fraction of population below poverty line, 1979	0.12	0.03	0.06	0.17	726
Fraction of population with BA, 1980	0.16	0.05	0.09	0.27	770
Fraction of population over age 18 voting, 1988	0.49	0.7	0.34	0.63	770

Sources: County financial characteristics from California Institute for County Government, demographic characteristics from City and County Data Books (US Census), RAND California Business and Economic Statistics/ Bureau of Economic Analysis and California Department of Finance.

Table 2 Special agricultural worker program time line

Year	Events
1986	Immigration Reform and Control Act signed.
1987	Amnesty application filing begins June 1. Amnesty and temporary residence granted to persons who demonstrate evidence of having worked on perishable crops (specifically, in "seasonal agricultural services") for at least "90 person days" between May, 1985 and May, 1986. Applicants could apply from outside the US.
1988	Application filing window closes November 30. 1.3 million applications received; 700,000 in California. Agricultural employers face penalties for employing illegal immigrants beginning in December.
1989	SAW applicants become eligible for permanent residency December 1.
1990	57,000 SAW program persons granted permanent residence.
1991	920,000 SAW program persons granted permanent residence.
1992	117,000 SAW program persons granted permanent residence.

Source: US Citizenship and Immigration Services (2003), Ryntina (2002), Rosenberg et al. (1992).

Table 3 Ethnic fractionalization and IRCA; Differences vary by agricultural land use

Dependent variable: Ethnic fractionalization, it

	(1) OLS	(2) OLS	(3) OLS	(4) IV	(5) IV
1000 harvested acres/mile^2 (*AGAREA*)	0.049	0.053	0.117	0.142	0.144
	[0.100]	[0.102]	[0.110]	[0.125]	[0.125]
Year after IRCA?(0/1) (*AFTER)*	0.061		0.033	0.016	
	[0.015]***		[0.018]*	[0.014]	
Year after IRCA?(0/1)*1000 harvested acres/mile^2 (*AFTER * AGAREA*)	0.011	0.007	0.075	0.079	0.078
	[0.041]	[0.040]	[0.047]	[0.036]**	[0.036]**
Income per capita (1995$)			0.007	0.009	0.0005
			[0.004]**	[0.004]**	[0.0002]**
Constant	0.414	0.373	0.241	0.213	0.226
	[0.040]***	[0.040]***	[0.102]**	[0.118]*	[0.135]*
Year fixed effects?	NO	YES	NO	NO	YES
Observations	735	735	735	595	595
R-squared	0.07	0.10	0.14		
Number of counties	35	35	35	35	35
root MSE	0.12	0.12	0.11	0.11	0.11

In instrumental variables (IV) regressions current income per capita is instrumented for using the 5-year lag value. Coefficients and standard errors on income and interaction term are multiplied by 1000. Huber robust standard errors in parentheses. Significantly different from zero at 90% (*), 95% (**) 99% (***) confidence. Regression disturbance terms are clustered at the county level.

Table 4 Income per capita and IRCA; Differences vary by agricultural land use

Dependent variable: Income per capita, it

	(1) OLS	(2) OLS	(3) IV	(4) IV	(5) IV
1000 harvested acres/mile2	-9,134.44	-9,087.84	-9,630.62	-12,273.43	-12,209.19
	[3,108.84]***	[3,143.98]**	[3,213.49]**	[3,581.28]**	[3,576.71]**
Year after IRCA?(0/1)	3,826.61		3,201.72	2,035.91	
	[935.56]***		[835.76]***	[694.96]***	
Year after IRCA? (0/1) * 1000 harvested acres/mile^2	-8.52	-8.53	-8.63	-5.92	-5.90
	[2.57]***	[2.59]***	[2.44]***	[2.03]***	[2.01]***
Ethnic fractionalization			10,167.30	9,077.98	8,296.67
			[5,335.89]*	[5,799.35]	[5,827.01]
Constant	23,168.80	23,034.28	18,958.86	20,642.66	25,576.05
	[1,298.61]***	[1,254.76]**	[2,041.46]**	[2,405.76]**	[2,912.31]**
Year fixed effects?	NO	YES	NO	YES	NO
Observations	735	735	735	595	595
R-squared	0.23	0.27	0.29		
Number of counties	35	35	35	35	35
root MSE	4378.19	4312.01	4211.64	4406.50	4312.80

In instrumental variables (IV) regressions current ethnic fractionalization is instrumented for using the 5-year lag value. Huber robust standard errors in parentheses. Significantly different from zero at 90% (*), 95% (**) 99% (***) confidence. Regression disturbance terms are clustered at the county level.

Table 5 Instrumental variables estimates- roads

Dependent variable: Road expenditure per capita ($1995), it (1985-2000)					
	(1) IV	(2) IV	(3) IV	(4) IV	(5) IV
Ethnic fractionalization	-173.24	-106.65	-346.73	-204.51	-327.10
	[62.63]***	[63.61]*	[108.41]***	[86.20]**	[132.14]**
Income per capita (1995$)	-0.002	0.002	0.001	-0.001	0.005
	[0.001]	[0.001]	[0.003]	[0.003]	[0.002]***
Fraction pop. in unincorporated areas		18.90	14.95	-1.94	-93.32
		[37.33]	[34.23]	[30.83]	[54.17]*
Government transfers per capita		0.35	0.36	0.27	0.02
		[0.12]***	[0.09]***	[0.08]***	[0.03]
County has charter? (0/1)		-24.11	-14.21	-6.21	
		[10.12]**	[8.60]*	[9.03]	
Fraction 1980 pop. over 18 voting			414.12	302.07	
			[118.41]**	[120.13]**	
Fraction 1980 pop. with BA			-328.28	-151.47	
			[203.05]	[230.35]	
Fraction 1979 pop. below poverty line			-671.63	-651.51	
			[334.84]**	[342.62]*	
Ethnic fractionalization (1979)			404.14	304.45	
			[97.73]***	[75.90]***	
Fraction of pop. over 65				149.94	0.94
				[321.61]	[442.99]
Log of total pop.				-13.63	58.13
				[3.63]***	[47.99]
Violent crimes rate				0.00	0.00
				[0.01]	[0.01]
Year fixed effects?	YES	YES	YES	YES	YES
County fixed effects?	NO	NO	NO	NO	YES
Observations	560	560	528	528	560
No. counties	35.00	35.00	33.00	33.00	35.00
Root MSE	42.65	36.27	32.74	31.27	20.67
1st -stage F stat (ethnic frac.)	8247	8704	1743	1743	1247
1st -stage F stat (income per capita.)	630	1151	737	119	100
Hansen J stat (test of OIR)	1.36	0.47	0.83	1.54	1.94
Hansen J dof	2.00	2.00	2.00	2.00	2.00
Hansen J p-value	0.51	0.79	0.66	0.46	0.38

In instrumental variables (IV) regressions current ethnic fractionalization and current income per capita are instrumented for using the 5-year lag values, 1,000 harvested acres per square mile, the interaction of “after IRCA” and 1,000 harvested acres per square mile and the square of this interaction term. Huber robust standard errors in parentheses. Significantly different from zero at 90% (*), 95% (**) 99% (***) confidence. Regression disturbance terms are clustered at the county level in regressions that exclude county fixed effects.