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REAL ESTATE CAPITALIZATION BY POLICY INPUT IN THE CLEVELAND EMPOWERMENT ZONE: AN APPLICATION OF ADJUSTED INTERRUPTED TIME SERIES ANALYSIS (AITS)

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This paper reviews select research and metareviews of Enterprise Zones and Empowerment Zones to assess the methods used to measure the impact of these areas on real estate capitalization. The review pays close attention to the policy implications of the empirical findings regarding the impact of EZ policy on housing prices, business location decisions, job growth, industrial output, employee income, and other effects. It closes with a brief application of the adjusted interrupted time series (AITS) method in the Cleveland Empowerment Zone. Controlling for land use type, the study finds a larger one-time increase in real estate values after the EZ designation compared to the year after the introduction of the tax incentives. However, results are not significant when controlling for fixed tract and year effects. The paper closes with recommendations for policy and future research.

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INTRODUCTION

The Cleveland Empowerment Zone/Enterprise Community (EZ/EC) is part of a federal initiative to create jobs in urban neighborhoods. This area is composed of a group of contiguous 1990 census tracts, and contains the Midtown district near Cleveland State, manufacturing areas in the Hough neighborhood, and the Cleveland Clinic. The CEZ/EC had two distinct program phases. Between 1995 to 1999 it received $3 million in social-services block grants and $174 million in section 108 economic-development loan guarantees to implement a strategic plan developed with community stakeholders. During the second phase, beginning January 1, 2000, a package of tax incentives went into effect that included a 20 percent wage credit up to $3,500 per employee to businesses that employed workers who worked and lived in the CEZ/EZ (HUD 2006). Which package of benefits, if any, gave the bigger boost to Cleveland's economy?

In Tiebout's idealized model of competition between cities, households decide where to live based on a bundle of housing products and local services. As families' incomes rise, they trade up for larger housing stock available in low-density suburbs, because they prefer more acreage, dislike the negative externalities associated with large employment centers, and are indifferent to longer commutes because the benefits of suburbia outweigh the costs. The poor, on the other hand, collectively outbid the wealthy to remain in inner cities and be close to work (Tiebout 1956). As more families purchase low-density housing to be away from work, this creates a jobs/housing imbalance known as spatial mismatch. It can also lead to racial segregation and concentrated poverty. For example, Martin (2004) found that although spatial mismatch declined nationwide between 1970 and 2000, most households tended to move away from new employment growth areas, with the exception of African American residents. African Americans followed jobs because jobs did not follow them (Martin 2004). Tiebout would argue that African Americans preferred shorter commutes and denser housing situations. The result of spatial mismatch and residential segregation is that local governments, declining in population, have a low tax base and the burden of providing high-cost services.

These center cities and inner-ring suburbs must find ways to promote economic development in order to address declining neighborhoods. Ladd (1994) terms the range of economic-development strategies as people-based strategies, place-based strategies, or place-based people strategies, a combination of the two (See Table 1). People-based strategies support the incomes, education, and housing of families in need. However, this assistance can result in families trading up to a better area. Place-based strategies, on the other hand, focus on a piece of land. They range from public infrastructure investment to locational tax incentives. Ladd feels that cities have to find the right mix.

Table 1: Simple Typology of Revitalization Programs (Adapted from Ladd, 1994).

Type	Examples	Strategies
People Only Strategy	Welfare to Work, Section 8, Earned Income Tax Credit, Head Start, Jobs Training Partnership Act	Investing in people through ob training and placement, transportation, housing vouchers and financial support.
Place Only	British Model of Enterprise Zone, some US states.	Deregulation and tax incentives for capital investment in small industrial sites
Place Based People Strategy People	Federal Empowerment Zone, most US State EZ programs.	Strategic planning, wage incentives, as well as deregulation.

The Enterprise Zone (EZ) approach is a pure place-based approach in Ladd's taxonomy. In general, an EZ is an area that has favorable services, regulations, or tax advantages for businesses. Sir Peter Hall developed the idea of the Enterprise Zone after reflecting on the successes experienced by capitalist Hong Kong despite being surrounded by centrally planned China. He supposed that fewer regulations allowed Hong Kong to flourish. Likewise, taxes, a financial form of regulation, deter business. The original EZ program in the United Kingdom hoped to stimulate business in areas with a declining manufacturing base by designating small industrial parks to benefit from reduced regulation, streamlined permitting, and lower taxes on investment than normally found in cities (Lavin and Whysall 2004). As this simple concept spread to other parts of the world with different regulatory and tax structures, policymakers had to experiment with finding the most effective menu of benefits to stimulate business.

For example, in the 1980s and 1990s, Jack Kemp, a former Republican congressman and secretary of the Department of Housing and Urban Development, advocated for a national EZ program that would designate areas free of the capital gains tax (Kemp 1990). He noted that 37 states had already enacted similar programs with some success. These states provide tax incentives to businesses to hire workers and purchase capital stock. His dream was realized when the Clinton Administration designated eight Empowerment Zones and seventy Enterprise Community Initiatives in 1994. The Administration added $1 billion in grants for economic development and social services and also required a strategic plan with substantial community participation (Lavin and Whysall 2004). Finally, a business-incentive program advocated by Republicans that languished for years in a Democratic-controlled Congress became law when the Clinton Administration supplemented it with large cash grants and community-based strategic planning and oversight reminiscent of Johnson's Model Cities program. Ladd expressed hope that this Federal program would be an opportunity to combine a place-based strategy with one that also invested in people (Ladd 1994).

This paper reviews select research and metareviews of Enterprise Zones and Empowerment Zones to assess the methods used to measure the impact of these areas. The paper pays close attention to the policy implications of the empirical findings. It closes with a brief application of the adjusted interrupted time series (AITS) method in the Cleveland Empowerment Zone and makes recommendations for further research regarding place-based initiatives (Goss and Phillips 2001; Galster et al. 2004).

LITERATURE REVIEW

Most scholars treat the enterprise zone as a policy experiment and evaluate it using case studies, shift share analysis, microsimulation, or a natural experiment, otherwise known as a quasi-experiment.

Overview of Methods Used to Evaluate Enterprise Zones

Early case studies of EZs involved surveys of businesses and zone administrators, subjecting them to criticism of bias and nongeneralizability (Wilder and Rubin 1996). Some of these case studies used shift-share analysis to decompose changes in employment growth by sector over a two-time period between nested areas by each sector of industry. Although this allowed for estimation of net employment gains, it did not allow authors to make claims of causal statistical inference. Furthermore, shift-share analysis relies on the surrounding jurisdiction as an implicit control, which may not be an appropriate comparison to an EZ (Dowall 1996; Boarnet 2001; Wilder and Rubin 1996; Ladd 1994).

Peters and Fisher (2004) spent a career analyzing EZs using a microsimulation approach. They estimated the impact of specific tax incentives in a given EZ on a hypothetical firm in terms of cash flow and internal rate of return. They took this approach because such simulations predict business behavior and consider differences between various state EZ benefits (Peters and Fisher 2004).

The bulk of recent EZ evaluations use a natural experiment because EZs are purposefully, not randomly, chosen. While a true experiment depends on random assignment of a subject to control or treatment, a natural experiment attempts to find a control area based on similar features in order to distinguish gross and net effects of a policy impact. Such a nonrandom method limits the efficacy of any findings. It is also difficult to find data that match the right time period in sufficient increments and at the right level of geography. EZ boundaries may not match data source boundaries such as census tracts or zip codes. Also, state and local data on employment is difficult to compare at the national level because the definitions used within and the accuracy of each data source vary (Wilder and Rubin 1996; Boarnet 2001; Peters and Fisher 2004).

Researchers have used different methods to develop a control group. One possible control follows shift-share analysis and uses the rest of the jurisdiction as a control area. Alternatively, propensity-score matching is a technique that identifies areas with similar characteristics in order to isolate the policy impact in the noncontrol group. This statistical procedure uses logistic regression to estimate the log odds that a given tract is designated, controlling for variables such as race, poverty, unemployment or vacancy rates (O'Keefe 2004). EZ tracts are matched to non-EZ tracts in the same county that have the nearest score based on the log odds of having been so designated. This controls for the unobserved effect that the county government has on the policy treatment, but cannot be used if the EZ is the only distressed area in that jurisdiction. Consequently, others have built comparison areas from the losing applicants for an EZ designation in other jurisdictions to calculate the propensity score (Busso and Kline 2006). Finally, some argue that matched-pair analysis does not adequately distinguish between policy effects and locational advantage, so they test the differences between the EZ and adjacent tracts

(Imrohoroglu and Swenson 2006). The justification of any given approach is made by an appeal to econometric theory, logic, and an in-depth knowledge of the local conditions.

EZ evaluations estimate the impact of the program on inventory, real estate capitalization, wages, vacancy, and other dependent variables. The estimate of zone impact versus the comparison area is usually controlled using poverty, unemployment, education level, interaction effects, fixed state effects, and fixed year effects as covariates. A fixed-effect control assumes that a given dummy variable has unobserved differences that do not change. Boarnet finds that studies without proper controls overestimate zone effects. Furthermore, studies with fixed-effect controls find no impact. (Boarnet 2001; Busso and Kline 2006; O'Keefe 2004; GAO 2006).

Results and Policy Recommendations from the Literature

Economists have found limited impact of EZ policy on housing prices, business-location decisions, job growth, industrial output, employee income, and other outcomes.

Housing prices

A classic article by Engberg and Greenbaum (1999) tested the theory that the impact of the Enterprise Zone activity would be capitalized in the residential-housing market by comparing changes from 1980 to 1990. This would make sense in Tiebout's world: if part of the housing bundle included fresh employment prospects in the EZ, people should want to pay more for housing. This regression model controls for EZ impact using a vector of 57 initial conditions of the MSA, state fixed effects and the interaction of vacancy and zone impact. The vacancy rate in 1980 was used as a covariate because it is a good measure of demand for real estate and thus an important factor in EZ selection. However, Engberg and Greenbaum found that the class of EZs with a vacancy rate one standard deviation below the mean had housing values grow about .948 percent, holding other values constant. Conversely, EZs with high vacancy rates saw decreasing housing values. To explain this paradox, the authors speculated that the decreased housing values may be due either to more attractive benefits in other areas, the EZ designation creating a bad reputation, or a result of new businesses causing negative externalities of increased traffic or pollution. Although the cause for this effect may vary from city to city, at the mean, the EZs with the highest initial vacancy rates saw declining housing values. Consequently, the authors recommend designating EZs in communities that are slightly better off until additional research could clarify the cost and benefits of EZs (Engberg and Greenbaum 1999).

Business Location

Local governments hope that EZs attract new businesses but are concerned that new tenants may outbid land from existing tenants. However, in a review of a California EZ, two-thirds of businesses reported that zone benefits did not influence location or expansion decisions (Dowall 1996). Why is this true? Peters and Fischer explain that with a -0.3 estimated elasticity of business mobility across jurisdictions and taxes, a 30 percent tax cut would be responsible for only one job out of the ten created in the area. They joke, "Incentives work about 10 percent of the time and are simply a waste of money the other 90 percent" (p. 32). Furthermore, low-income jurisdictions do not have the excess tax base to fund economic development incentives. If businesses choose a community not only on its locational merits—such as access to markets, quality infrastructure and low factor costs—but also because of a subsidy, then the move represents a net social loss from the public to private sector. The jurisdictions with a prime

location and a higher tax base can always offer more incentives than those without (Peters and Fisher 2004).

Fortunately, this "subsidy bidding" does not appear to happen in practice. Wilder and Rubin have found that most job growth within an EZ comes from new businesses and the expansion of business already present. Businesses that had relocated only accounted for 6 percent to at most one-third of businesses in the EZs they reviewed. Wilder and Rubin speculate that relocation was low because of outright prohibition in some states, such as Ohio, or because of the prohibitive fixed costs needed to move (Wilder, 1996 #34). On the other hand, jurisdictions with limited locational advantages should not put much hope in tax incentives alone.

Job Growth

Given that EZs have a marginal impact on attracting new businesses, do they create new jobs at existing firms? An early shift-share analysis of individual firm data reported to the state allowed researchers to calculate a net increase of 1,878 jobs in the Evansville, Indiana, EZ (Wilder and Rubin 1996). In a summary of research prior to 1996, Wilder and Rubin found that EZs created some jobs in the areas they targeted, but the quality of the jobs was unknown. They concluded that in the states whose EZs they reviewed, wage incentives were not targeted to job creation or new hires and mostly benefited workers living outside the EZ. A targeted incentive would require the worker to either live in the EZ, be a new hire, or be a member of a difficult-to-hire class, for example, the homeless, disabled or ex-offenders. Dowell (1996) also estimated the impact of jobs and found that, in the California EZ program, only 2 of 13 sites showed higher than predicted job growth. The report concluded that the effects of the state incentives were marginal and needed to be packaged with comprehensive economic development tools. Revolving loan pools, one stop shops for entreprenurs and job seekers and job training programs are examples of these tools. Furthermore, because state tax credits are taxed at the federal level, a federal tax credit can be more attractive to businesses (Dowall 1996; Wilder and Rubin 1996).

In a recent evaluation of California EZs, O'Keefe (2004) found that employment growth rises 3.1 percent in the first six years of designation, but falls in years 7-13 by 3.2 percent. A plausible explanation of this effect is related to the structure of the California EZ hiring credit, which is worth 50 percent of wages up to $8.62 per hour in year one, but declines 10 percent per year until it becomes valueless in the sixth year of employment. Also, the tenure of an EZ designation is only 15 years with an option to renew for five years. She concludes that businesses are less likely to invest in an EZ that is waning than one that is newly designated (O'Keefe 2004).

Industrial Output

In Nebraska, Goss (2001) tested the assumption that EZ incentives can efficiently stimulate industrial output in high-unemployment areas. He found that such incentives increase output in counties with low unemployment rates to a greater extent than in counties with high unemployment rates. One dollar in tax incentives leads to a $2.83 increase in output for low-unemployment counties but only a $1.83 increase in high-unemployment counties. Unsubsidized capital was more efficient in both cases. Goss suspects this trend is related to Nebraska's low-wage slaughterhouse industry. Indeed, it is reasonable that in a tradeoff between low-wage labor and investment in capital stock, a firm would choose low-wage labor when other factors are held

constant. The subsidy crowds out other sources of financing. Although the incentives appear to trade off efficiency for equity, Goss and Phillips caution against generalizing from Nebraska, given its industry mix. (2001).

Employee Income

Bostic and Prohofsky (2006) conducted the first study of the change of income for workers in EZs. The authors matched EZ participants to a control group based on adjusted gross income, wages, and Earned Income Tax Credit claims. They found that EZ participants are more likely to file taxes than the 1995 control group. For example, in 1996, 93.3 percent of the EZ group filed, compared to 85.1 percent of the control group ($p = .01$). Participation in the EZ predicted a .874 percentincrease in 1995 adjusted growth income relative to the 1995 control group, holding other values constant. The result was also significant for years 1996 and 1997 (Bostic and Prohofsky 2006).

Differences Across EZs

Some of the studies that found no impact of EZ polices drew from a large sample of dissimilar programs. Bondonio and Greenbaum (2005) argue that mean impacts erase true accomplishments in some areas. Over a sample of 11 states, the authors found no EZ impact but proceeded to test differences based on policy. They found that, for every one standard unit increase in zone area, there was a decrease in employment growth (-20.5 percent), capital expenditures (-14.6 percent) and value of shipments (-17.5 percent). Tying incentives to new jobs increased growth by 22.2 percent. A strategic plan increased value of shipments by 25.8 percent and capital expenditures by 17.5 percent. Accordingly, the authors recommend smaller EZs, targeted incentives, and the development of a strategic plan (Bondonio and Greenbaum 2005).

Cost Benefit

Finally, some studies also estimate the cost and benefit of EZ programs using estimates of increased tax revenues or average cost per job. Wilder and Rubin note that the literature did not generally examine the quality of jobs created, except to note that most were in manufacturing and wholesale trade. For example, in Indiana, it cost $1 in EZ tax incentives to produce $2.20 in additional revenues (Wilder and Rubin 1996). Ladd's meta-analysis argued that in the UK each job cost $15,000. Assuming only a quarter of jobs may be attributed to the EZ policy, the cost increased to $60,000 per job. The New Jersey EZ program cost over $13,070 per job. In Indiana, the EZ cost $10,170 for each job, but increased to $53,507 if the job went to an EZ resident. GAO found that no jobs were created by the Maryland EZ, so the cost per job was undefined (Ladd 1994). Using assumptions from the literature, O'Keefe argued that EZs were cost effective from the state's perspective because it paid for designation, training, and monitoring costs in exchange for increased state income-tax revenues from new jobs. The local government, on the other hand, assumed more of the marketing and compliance costs and potentially saw declines in the later years of EZ designation as tax credits expired or lost value (O'Keefe 2004).

ADJUSTED INTERRUPTED TIME SERIES (AITS) IN THE CLEVELAND EZ

The federal Cleveland Empowerment Zone/Enterprise Community (EZ/EC) set up an interesting opportunity for a natural experiment to test the difference between two phases of the program.

Cleveland has a proud history as the birthplace of President Garfield, home of John D. Rockefeller and his company, Standard Oil, as well as Sherman-Williams Paint, American Greetings, and many small businesses that supplied the automobile industry. Its 20th century industrial strength was built on shipping lanes from the Cuyahoga River to Lake Erie and beyond. Ethnic neighborhoods grew as Polish, Slavic, Italian, Hungarian, and German immigrants arrived for jobs. Euclid Avenue, known locally as millionaire's row, was often compared to Fifth Avenue in New York. By the 1960s, the city struggled with many issues that led to suburbanization. The new freeways cut through neighborhoods. The Hough, an industrial and residential area on the north side, experienced riots and arson that left many parcels vacant. The Cuyahoga River had become so polluted that it burst into flames. In the 1970s, when Dennis Kucinich served as mayor, Cleveland shocked the bond markets by declaring bankruptcy and soon became known as the 'Mistake on the Lake' (Wikipedia 2006).

Research at Case Western Reserve University documented pervasive and concentrated poverty, crime, and welfare dependency in Cleveland, characterized by the changes in the regional economy. Although Cleveland developed as a patchwork of ethnically homogenous neighborhoods, as industry shifted to the suburbs, it left behind primarily poor African American neighborhoods. From 1980 to 1990, Cleveland saw increases in drug arrests, personal crime, welfare utilization, teen births, single mother births, juvenile crime, and low birth weight. However, during that same period property crime was reduced by 12 percent and infant deaths by 23 percent (Coulton et al. 1995; Chow and Coulton 1998).

Despite great challenges, Cleveland was able to build on its community development tradition to win an EZ/EC designation. Cleveland is still home to many settlement houses founded in the late 19th and early 20th century to help immigrants find jobs, housing, and organize block clubs (House 2005; Settlement 2006). More recently, the Cuyahoga River recovered ecologically, and by the late 1980s had become a regional destination for nightlife, known as the Flats (Wikipedia 2006). Finally, the City won a major downtown redevelopment coup with the opening of the Rock and Roll Hall of Fame Museum, which further anchored a waterfront already home to the Cleveland Browns Stadium and Great Lakes Science Museum (Wikipedia 2006).

In order to effectively involve existing community assets, each EZ/EC application included a strategic plan that required intensive participation and documentation of conflict with stakeholders and how this conflict was resolved. For the most part, all census tracts that comprised the EZ/EC needed to have at least 20 percent poverty in 1990 and be outside of the central business district (HUD 2002). This gave the City a chance to focus on the very neighborhood that gave birth to its favorite son, John Rockefeller. This area is geographically contiguous and contains part of historic millionaire's row, the Midtown district near Cleveland State, manufacturing areas in the Hough and Lakeshore neighborhoods, and the Fairfax community near the Cleveland Clinic. In the first phase of the EZ/EC, which HUD announced on December 21, 1994, Cleveland won an Enterprise Community designation that came with a $3 million social services grant. A few months later, Secretary Cisneros upgraded the designation administratively to Supplemental Empowerment Zone status that brought Cleveland $174 million in section 108 economic development loan guarantees but not the tax incentives that went to other round one Empowerment Zones. On January 1, 2000, the EZ tax incentives finally

went into effect after a special act of Congress. The incentives included a 20 percent wage credit up to $3,500 per employee to businesses that employed workers who worked and lived in the EZ, tax exempt bonds, increased deductions for purchase of equipment, and limited capital gains relief (HUD 2006). GAO found that from 1990 to 2000, Cleveland saw reductions in poverty and unemployment. Furthermore, the number of businesses in the EZ grew from 1995 to 2004, but the number of jobs declined. GAO notes that Cleveland focused primarily on economic development aspects of the program (GAO 2006). Which package of benefits, if any, gave the bigger boost: the initial grant and loan combination coming after the excitement of strategic planning in late 1994, or Enterprise Zone style tax incentives beginning January 1, 2000?

Data and Methodology

Using the AITS, this study was able to estimate differences in sales base prices and appreciation between the intervention periods. AITS can overcome the selection bias inherent in community development initiatives that are not created randomly but rather only in areas that need them. This paper adopts the model from Glaster et. al. (2000) as follows:

$$\mathbf{I}t = c + d(\mathrm{DIMP}_t) + e(\mathrm{DPOSTIMP}_t) + f(\mathrm{TRIMP}_t) + g(\mathrm{TRPOSTIMP}_t) + h(\mathrm{TRALL}_t) + j(\mathrm{TRPOSTALL}_t) + ([\mathbf{X}])\mathbf{K} + \varepsilon$$

The dependent variable of interest in this case, **I***t*, is the natural log of nonzero sales prices of real estate from 1990 to September 29, 2006, as reported to the Cuyahoga County Auditor's Office. I chose this dependent variable to test the theory of Engberg (1999), who argued that any change in the desirability of an area as a result of a set of tax incentives would be capitalized in the real estate market. Glaster et. al. (2000) extends this theory to include any community development initiative. In other words, if a firm knows about the EZ/EC and believes that this will improve the neighborhood, this knowledge would be factored into real estate sales.

The independent variables are all indicator variables that identify if a census tract is in the EZ/EC area or the control area, and mark time before the policy intervention or after the policy intervention. DIMP_t is a dummy for EZ (1 = yes, 0 = no) at time t, $\mathrm{DPOSTIMP}_t$ is a dummy for in EZ post impact date, and TRIMP_t is a vector of cardinal numbers starting at one for the first time period and increasing by one for each time period. Sales outside the EZ are coded zero. In this case, each day that there is a transfer represents a time period (1 = Jan. 1, 1990, 2 = Jan. 2, 1990, etc.). Glaster used quarters to smooth results and to control for seasonal variation, which should provide more robust results than this study, which used exact day of sale. Likewise, $\mathrm{TRPOSTIMP}_t$ is a similar vector of cardinal numbers but starting at the change date. TRALL_t numbers the sales for both inside and outside the EZ while $\mathrm{TRPOSTALL}_t$ numbers all post change date sales.

With regard to interpretation, the coefficients *d* and *e* measure cross-sectional effects. The coefficient *d* tests whether or not there was a significant difference in home prices between EZ and control areas before the intervention. Coefficient *e* estimates a one-time shift between areas after the intervention. Longitudinal effects are captured by *f* and *g*. The coefficient *f* estimates the difference in trends before intervention and *g* estimates the difference in trends after intervention. The model controls for overall trends, postintervention overall trends, and fixed effects of

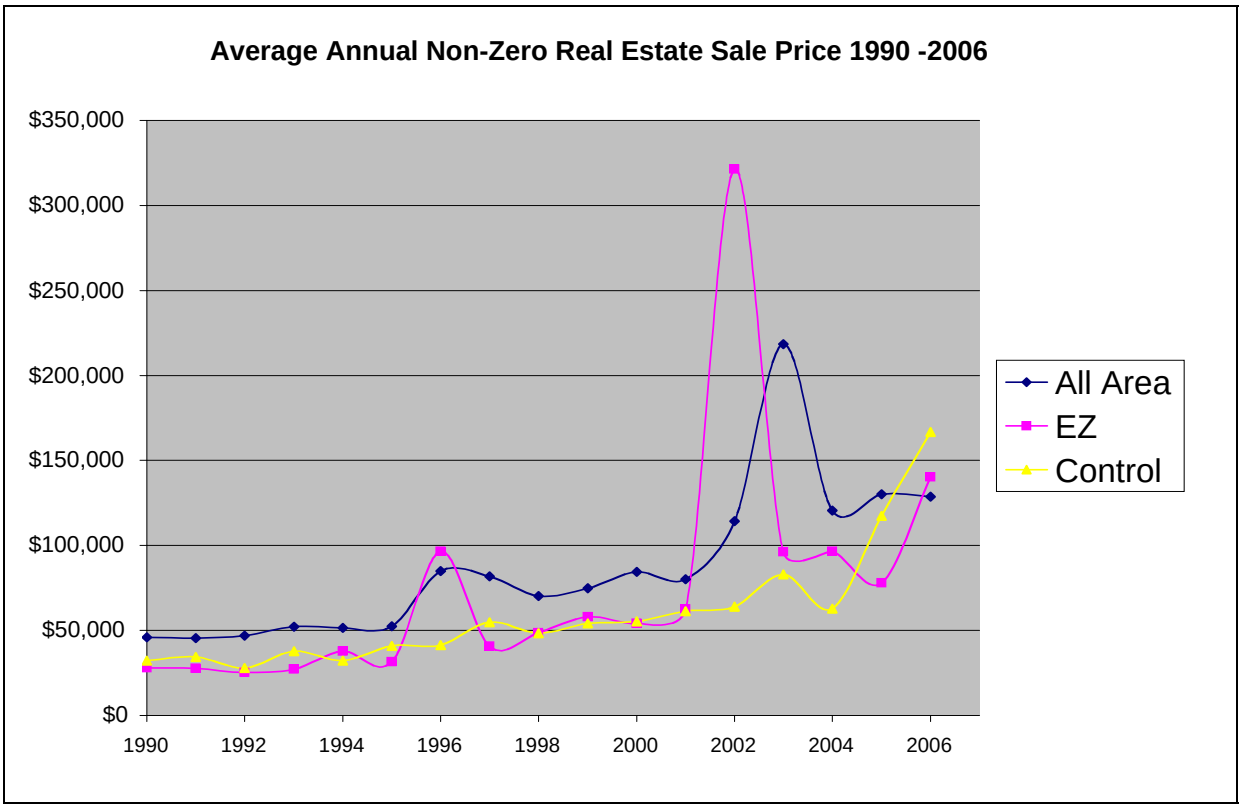
property type, year, and tract. Glaster et .al. use all low-income areas as a control. This study follows O'Keefe and uses a propensity match (see Table 2).

To identify a comparison area, I used propensity score matching on all census tracts in Cleveland. The census tract characteristics used to predict the log odds of having been designated an EZ may be found in Appendix One. The dependent variable was the natural log of the sales price of all nonzero real estate transfers from January 1, 1990 to September 29, 2006, as reported to the Cuyahoga County Auditor's Office on the conveyance form. Data were downloaded from the NEO CANDO system at Case Western Reserve University (CPSC 2006). I dropped zero-value transactions because they were often noncash exchanges between family members that would not send a market signal. In order to test regression assumptions, I plotted residuals against fitted values to check for heteroscadasticity. I used box plots, inverse normal plots, and the Shapiro-Wilk test to check the assumption of a normal distribution of residuals. Although Shapiro-Wilk failed due to the presence of outliers, the OLS assumptions were not grossly violated. Finally, although Glaster complemented data analysis with in-depth interviews with zone administrators, business owners, or community stakeholders, that important qualitative analysis is outside the scope of this study. Therefore, the data is intended only to document relative capitalization in real estate conveyance across two time periods and not make claims regarding the success or failure of the EZ policy or its local administration.

Results

Results of AITS on the natural log of 23,600 real estate sale prices were inconclusive except for a significant 1.5 percent decrease in residential sales price trends in the EZ/EC post designation from Dec. 21, 1994, onward, compared to the propensity-matched control area and controlling for fixed year and tract effects. This finding is a surprise given that two prominent spikes in real estate price occurred in the EZ immediately after the critical dates of implementation that do not show up in the control area. Figure one presents the average sales prices per year.

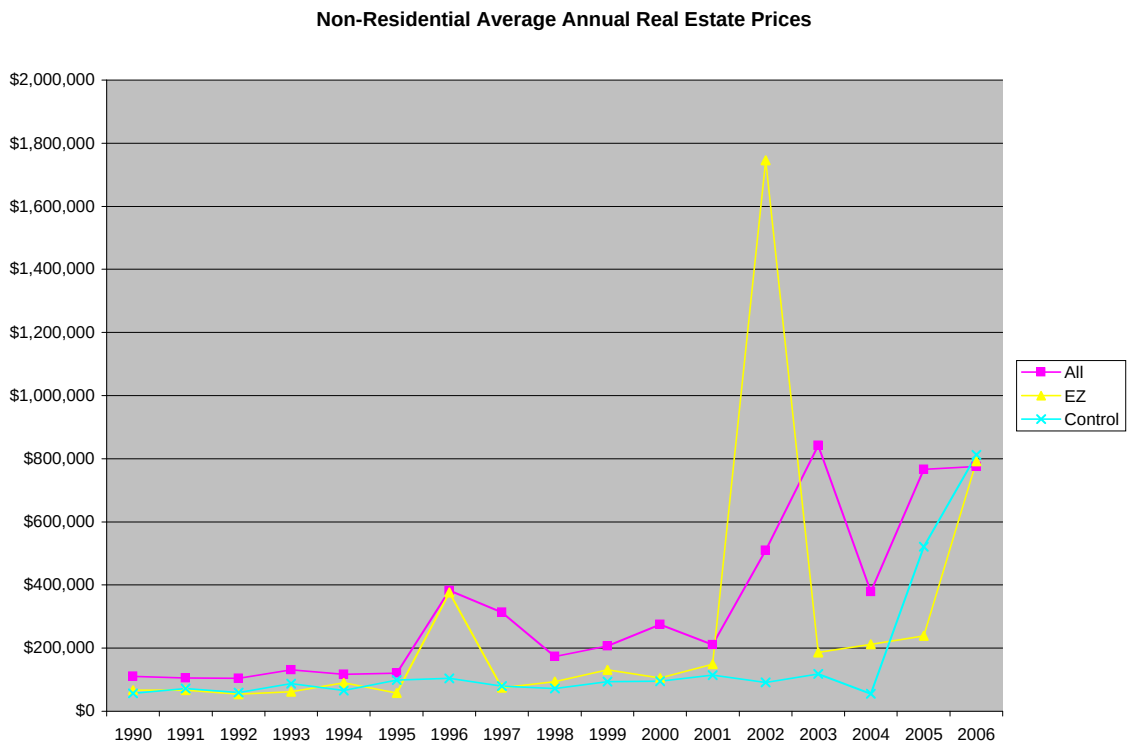
Figure 1: Average Annual Non-Zero Real Estate Amount 1990-2006



The first spike in average prices is the year after the designation of the EZ, and the second spike is after the beginning of the tax incentives. The increase appears larger in amplitude when the tax incentives go into effect. However, an inspection of the data reveals that one $2 million industrial property sale in 2002 in the EZ drives the estimate. Conducting a separate regression for residential real estate allows investigation of that trend without the noise associated with the less-predictable industrial and commercial real estate (see figures two and three).

Figure 2: Average Annual Real Estate Sales

Figure 3: Non-Residential Average Annual Real Estate Prices



Note that the post-tax incentive time period is compared to all transfers after January 1, 1990, in order to measure any second shift in base price or trend with respect to the first shift. Using the base model regression with no controlling variables, I found that after the designation of the EZ on December 21, 1994, the base price of nonzero real estate sales increased 27 percent compared to the control area (r^2 = .1257, $p < .001$). However, the change in trend is not statistically significant (See table 4). After the tax incentives came into effect on January 1, 2000, there was a 19 percent increase in the level of nonzero real estate transfers compared to the control area and a 1 percent decrease in the trend compared to the control area ($p = .004$). This loss of real estate value would be evidence of a negative impact of the EZ/EC tax incentives, but I have yet to control for unobserved effects.

In a second regression, I split industrial, commercial, and other non-residential property (N=4236) from residential land (N=19,634). After the designation of the EZ on December 21, 1994, the base price of residential real estate sales increased 29 percent compared to the control area (r^2 = .1622, $p < .0001$). After the tax incentives came into effect in 2000, there was a 22.94 percent increase in the level of nonzero real estate transfers compared to the control area ($p < .0001$). However, there is a significant slowdown in price trends of 1 percent compared to the control area ($p = .003$). This slowdown would be evidence for negative impact of the EZ/EC tax incentives on the residential real estate market, but again, I have not yet controlled for unobserved effects in the industrial, commercial, and other types of nonresidential property, See Appendix Two for regression tables.

In a third regression, I included year and tract fixed effects and found no significant estimates of changes in base price or appreciation when including all property types. An F-test that assumed that estimates of year and tract fixed effects were all equal to zero could be rejected at $p < 0.001$. Accordingly, I can conclude that the third regression explains more variability than the base model. Glaster et. al used quarter-year effects to smooth out seasonal variation in real estate markets because home sales "cool down" in the winter. However, a comparison of a model with year effects to quarter year effects using the Bayesian Information Criterion (BIC) showed strong support for the fixed-year effects. Also, using quarter-year fixed effects did not improve significance of estimates.

A fourth regression on only the logged residential sales prices controlling for year and tract effects shows about a 1.5 percent decrease in sales trends after the initial EZ/EC designation ($r^2 = .2672$, $p = 0.009$). In the industrial, commercial, and other types of nonresidential property, there is a significant 51 percent one-time increase in base prices following the launch of the tax incentives when other values are held constant ($r2 = .2505$, $p = .007$). The trend does not change significantly at that time, but at the initial designation a 2.8 percent trend increase approaches significance ($r2 = .2503$, $p = .088$). There is not significant price shift at initial designation. Estimates of the nonresidential regression do not substantially change when dropping the $2 million sale. These two significant findings, one of negative impact in residential markets and one of positive impact in other markets, are preliminary because the assumption of spatial independence has not been tested. Due to the large number of observations, attempts at conducting a spatial lag regression, as recommended by Glaster, et. al., failed due to the limits of available computational power.

DISCUSSION

In summary, although a visible increase in average real estate prices happened in the EZ after 1994 and 2000, econometric analysis estimates a slow down in residential prices post designation coupled with a large boost in industrial and commercial property sales. Because these results are only an association and not causal, they should be considered preliminary. The model could be refined using spatial controls or alternate comparison groups. Furthermore, all findings should be approached with caution and interpreted in the light of on-the-ground knowledge of zone administrators, business leaders, and residents. However, such findings are compelling and provide support for Engberg and Greenbaum's hypothesis that the business success stimulated by EZ tax incentives may have the unintended consequence of depressing residential markets resulting from increased traffic, noise, or pollution.

Cleveland had clearly lost its locational advantage for both commercial and residential real estate as evidenced by its initial vacancy rates. Thus, a reasonable, federally-funded public subsidy should not create a net social loss because it should help recycle urban land and infrastructure. Also, the federal investment targets the redistribution to places like the Cleveland EZ area that had been losing economic resources. The benefit to Cleveland has been business growth—including a large boost in average commercial and industrial property values—as well as reductions in unemployment and poverty. The cost to Cleveland includes the startup costs of the application and ongoing costs of administering the loans and grants or marketing the tax

incentives. According to my findings, there may be an additional cost of declining residential property values, which may reduce tax revenues if it is greater than the revenues collected from increasing industrial and commercial property values. Furthermore, as predicted by O'Keefe's hypothesis that EZ-style wage incentives have a waning effect as the program nears expiration, Cleveland may see further declines as the program sunsets in 2009. Additional research could determine if Cleveland's situation is ordinary or unique. Other EZ/ECs, such as those designated in the second round in 1997, did not receive wage credits until 2001. AITS analysis of real estate prices in these areas could begin to document trends in the federal program.

CONCLUSION

Why should a state government have an enterprise zone? At their worst, Enterprise Zones have a negative impact on housing values caused by the negative externalities of business—or no impact due to the low elasticity of business in response to changes in taxation. Furthermore, they cost up to $60,000 per job created and can encourage the substitution of labor for capital. Next, businesses lose money if tax savings at the state level increase taxable federal income. Finally, the attractiveness of benefits may decline with time and lead to employee turnover. Should a rational planner propose such a policy?

Although findings of impact based on natural experiments must be read with skepticism, they document positive elements of EZ policy. Even if the ability of incentives to attract business is marginal, states can use incentives as retention or expansion schemes. States need to structure incentives carefully to achieve desired results. For example, states that want to create new jobs need to give incentives for new hires, keeping in mind that turnover may be timed to the duration of the incentive. EZs should not be too large or too numerous per state in that their attractiveness is diluted. Areas should have a strategic plan that incorporates other economic development tools regarding infrastructure, workforce development, transportation, and other services. This is especially true in very distressed areas, because tax incentives alone are too marginal to compensate for severe externalities. Finally, coordination with federal tax incentives is critical so that businesses do not lose out on their federal returns. This coordination would also help shift the cost burden away from the local government.

A quasi-experiment can test the impact of a given incentive tied to a policy outcome. For example, testing wage incentives can use state unemployment and tax data. In short, if the local government is able to have a targeted employment incentive in a small area, it is feasible to get accurate administrative data to do cost-benefit analysis without resorting to limited econometric techniques. If a local government participates in a place- and people-based strategy like the federal EZ/EC program, it becomes part of the bundle of goods and services available to the public that will either compete or fail in interjurisdictional markets. Research can model relative efficiencies of policy variants, but the true test of economic development success lies in the complex vagaries of the global market.

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APPENDIX ONE: COMPARISON AREAS USED FOR CLEVELAND WIDE PROPENSITY SCORING

Table 2: Independent Variables (1990) Used to Predict EZ Status

Independent Variables	Source
Population density (population per square mile)	Census Summary Tape File 1 - Table P1 & Area land
White, number and percent	Census Summary Tape File 1 - Table P6
Poverty rates	Census Summary Tape File 3 - Table P117
Unemployment rate (percent unemployed)	Census Summary Tape File 3 - Table P70
Percent Persons aged 16+ employed in Manufacturing, durable	Census Summary Tape File 3 - Table P77
Percent Persons aged 16+ employed in retail trade	Census Summary Tape File 3 - Table P78
Percent residential vacant parcels	Cuyahoga County Auditors office
Percent commercial vacant parcels, includes apartments	Cuyahoga County Auditors office
Percent industrial vacant parcels	Cuyahoga County Auditors office

Figure 4: Cleveland EZ (N = 32 Tracts) and Comparison Area (N = 32 Tracts)

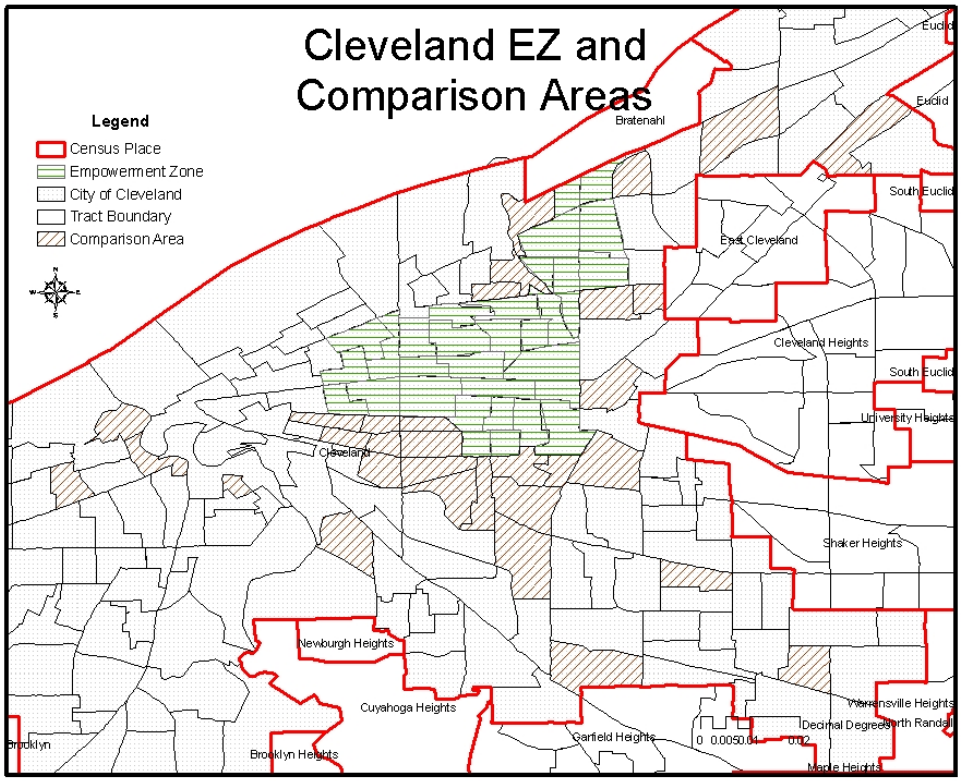

Table 3: Descriptive Statistics 1990

Independent Variables	Mean	Std. Dev.	Min	Max
Cleveland Empowerment Zone (N=32)				
Population density (population per square mile)	8278.45	4763.61	536.35	17827.82
White, number and percent	9.60	20.80	0.22	73.74
Poverty rates	50.68	15.62	20.85	100.00
Unemployment rate (percent unemployed)	28.83	16.53	10.09	100.00
Percent Persons aged 16+ employed in Manufacturing, durable	9.53	5.19	0.00	21.15
Percent Persons aged 16+ employed in retail trade	8.87	5.04	0.00	23.47
Percent residential vacant parcels	29.96	18.53	1.57	70.87
Percent commercial vacant parcels, includes apartments	33.57	12.67	0.00	58.70
Percent industrial vacant parcels	8.19	17.03	0.00	80.00
Comparison Area in Cleveland (N=32)				
Population density (population per square mile)	8946.95	5809.67	0.00	26320.76
White, number and percent	11.92	24.26	0.00	92.24
Poverty rates	52.42	22.04	0.00	89.25
Unemployment rate (percent unemployed)	26.93	17.17	0.00	61.56
Percent Persons aged 16+ employed in Manufacturing, durable	11.32	9.37	0.00	42.45
Percent Persons aged 16+ employed in retail trade	10.71	5.14	0.00	26.67
Percent residential vacant parcels	34.47	26.23	0.00	100.00
Percent commercial vacant parcels, includes apartments	34.66	15.25	0.00	60.87
Percent industrial vacant parcels	13.04	25.00	0.00	100.00
All Cleveland (N=224)				
Population density (population per square mile)	8071.97	4776.44	0.00	26320.76
White, number and percent	46.81	40.67	0.00	100.00
Poverty rates	33.45	21.35	0.00	100.00
Unemployment rate (percent unemployed)	17.24	14.19	0.00	100.00
Percent Persons aged 16+ employed in Manufacturing, durable	13.13	8.11	0.00	55.65
Percent Persons aged 16+ employed in retail trade	12.77	7.49	0.00	75.68
Percent residential vacant parcels	15.01	19.33	0.00	100.00
Percent commercial vacant parcels, includes apartments	19.51	15.07	0.00	85.71
Percent industrial vacant parcels	7.61	15.52	0.00	100.00

APPENDIX TWO: ESTIMATES AND STANDARD ERRORS FOR NAIVE REGRESSION

Dependent Variable: Natural Log of Non-Zero Conveyance Value at Sale from 1990 to September 29, 2006.

Number of Observations = 23,600

Table 4: Base OLS Regression

Federal Tax Incentives 1/1/2000					
		Coef.	**Std. Er.**	**T stat.**	**p-value**
Empowerment Zone (EZ)		-0.216	0.0476	-4.54	0.000
One Time Change in EZ after Incentives		0.192	0.0427	4.49	0.000
Trend EZ Area		0.0025	0.0018	1.42	0.155
Trend in EZ after Incentives		-0.0095	0.0033	-2.91	0.004
Trend in EZ & Control		0.0246	0.0011	22.39	0.000
Trend in EZ & Control after Incentives		-0.0058	0.0022	-2.6	0.009
			R2 =	0.1257	
EZ/EC Designation December 21st 1994					
Strategic Plan, $3 million SSBG Grant and Sec. 108 Loan Guarantees					
		Coef.	**Std. Er.**	**T stat.**	**p-value**
Empowerment Zone (EZ)		-0.1539	0.0689	-2.23	0.026
One Time Change in EZ Post Designation		0.2706	0.0515	5.25	0.000
Trend in EZ Area		-0.008	0.0052	-1.54	0.124
Trend in EZ Post Designation		0.0073	0.0056	1.31	0.190
Trend in EZ & Control		0.0099	0.0029	3.42	0.001
Trend in EZ & Control after Designation		0.0146	0.0034	4.32	0.000
			R2 =	0.1254	

Table 5: Logged Residential Property Sales Controlling for Year and Tract (N=19,364)

Federal Tax Incentives 1/1/2000					
		Coef.	**Std. Er.**	**T stat.**	**p-value**
Empowerment Zone (EZ)		-1.9957	0.5654	-3.5300	0.0000
One Time Change in EZ after Incentives		-0.0320	0.0552	-0.5800	0.5620
Trend EZ Area		0.0046	0.0017	2.6300	0.0080
Trend in EZ after Incentives		-0.0026	0.0030	-0.8700	0.3860
Trend in EZ & Control		0.0433	0.0092	4.7200	0.0000
Trend in EZ & Control after Incentives		-0.0336	0.0130	-2.5800	0.0100
			R2 =	0.2673	
EZ/EC Designation December 21st 1994					
Strategic Plan, $3 million SSBG Grant and Sec. 108 Loan Guarantees					
		Coef.	**Std. Er.**	**T stat.**	**p-value**
Empowerment Zone (EZ)		-2.1292	0.5677	-3.7500	0.0000
One Time Change in EZ Post Designation		-0.0540	0.0660	-0.8200	0.4140
Trend in EZ Area		0.0159	0.0055	2.9100	0.0040
Trend in EZ Post Designation		-0.0145	0.0056	-2.5900	0.0090
Trend in EZ & Control		0.0238	0.0149	1.5900	0.1110
Trend in EZ & Control after Designation		0.0028	0.0166	0.1700	0.8640
			R2 =	0.2672	

Table 6: Logged Non-Residential Prices Controlled for Year and Tract (N = 4236)

Federal Tax Incentives 1/1/2000					
		Coef.	**Std. Er.**	**T stat.**	**p-value**
Empowerment Zone (EZ)		-1.6706	1.5155	-1.1000	0.2700
One Time Change in EZ after Incentives		0.5128	0.1909	2.6900	0.0070
Trend EZ Area		-0.0049	0.0055	-0.9000	0.3680
Trend in EZ after Incentives		-0.0073	0.0108	-0.6800	0.4960
Trend in EZ & Control		0.0868	0.0285	3.0500	0.0020
Trend in EZ & Control after Incentives		0.0385	0.0425	0.9100	0.3650
			R2 =	0.2505	
EZ/EC Designation December 21st 1994					
Strategic Plan, $3 million SSBG Grant and Sec. 108 Loan Guarantees					
		Coef.	**Std. Er.**	**T stat.**	**p-value**
Empowerment Zone (EZ)		-1.5255	1.5215	-1.0000	0.3160
One Time Change in EZ Post Designation		-0.0303	0.2003	-0.1500	0.8800
Trend in EZ Area		-0.0198	0.0163	-1.2100	0.2250
Trend in EZ Post Designation		0.0288	0.0168	1.7100	0.0880
Trend in EZ & Control		0.0503	0.0430	1.1700	0.2420
Trend in EZ & Control after Designation		0.0614	0.0493	1.2500	0.2130
			R2 =	0.2503	

Land use type at time of conveyance is a four digit number that corresponds to the zoning of the parcel. For example, 5100 is for single family homes. The data set contains 180 unique land use codes. Residential property is in the range from 5000 to 5999. Including fixed effects for the 17 years of data and 64 census tracts did not yield results with significance less than p = .05 in a regression with all property types.