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IS THE ENDANGERED SPECIES ACT ENDANGERING SPECIES?

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ABSTRACT

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Abstract

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JEL: D8, K2, Q2
Key words: decisionmaking under risk and uncertainty, land use, preemption, endangered species act

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1 Introduction

Few pieces of environmental law have created as much controversy as the federal Endangered Species Act (ESA). The ESA was passed in 1973 by large majorities in both houses of Congress, following discussion focused on such charismatic wildlife as bald eagles and grizzly bears. Federal agencies were given a broad mandate to constrain activity likely to harm a species thought to be in danger of extinction, with minimal scope for cost-benefit analysis. Little thought appears to have been given to issues that might arise when habitat is on private land, and the conservation of such habitat has turned out to be of critical importance—nine out of ten listed species are found on private land, and most have more than 80% of their habitat on private land (Innes et al. 1998). It is clear that any serious effort to achieve the ESA's purpose of conserving "the ecosystems upon which endangered species and threatened species depend" must persuade numerous landowners, whether through coercion or contract, not to develop much of that habitat.

This situation has led to numerous controversies over the years, both over species listings and the design of remedies. These controversies have been well publicized in the affected region, serving as a warning to landowners that, if their land is likely to be valuable to the species in question, they may lose their development rights. The result is an incentive to make the land less habitable to the species, as per a "scorched earth" approach or to exercise the development rights prematurely, which may also have the perverse impact of degrading the habitat the ESA seeks to protect.

We believe that our study is the first to measure the extent to which landowners act to preempt regulation during the urban growth process, which is widely considered to be the leading cause of habitat loss in the United States (NWF 2001).[1] The empirical challenge in this case is especially thorny, since the preemptive act we seek to observe is a shift in the timing of development activities in which the landowner was likely to engage in any case – otherwise, there would be no motive for preemption. Such a shift can, however, carry a considerable economic cost, and in some circumstances the landowner might not have opted to destroy the habitat had he observed how land prices actually evolved. Preemptive development may also forestall the negotiation of conservation plans between landowners and government or private conservation groups.

In this paper, we develop empirical strategies derived from our theory to measure preemptive development along an expanding urban fringe and apply them to the case of the Cactus Ferruginous Pygmy Owl near Tucson, Arizona. Our identification strategy revolves around whether the plot of land was included as part of the owl's designated critical habitat (CH). Preemption in this case is measured by the difference between CH and non-CH land in the timing of development permit applications.

Using data drawn from more than sixty thousand plots, we find that CH parcels were developed on average about a year earlier than similar non-CH parcels. Alternative empirical analyses provide overall support to the proposition that CH parcels were developed earlier, indicative of preemption on a scale likely to matter for conservation and efficiency goals. Further support in favor of these findings is obtained in an empirical analysis of sales price data of undeveloped land. Our hedonic land price model suggests that market prices for plots

[1] We know of only two other studies examining whether agents respond to the ESA's perverse incentives: a survey in which about one in four landowners admitted to managing land in such a way to discourage mouse colonies (Brook et al. 2003); and a forestry study which found that trees close to colonies of an endangered woodpecker that nests in old trees were harvested earlier than they otherwise would have been (Lueck and Michael 2003).

of land identified as likely CH parcels become depressed during the period of uncertainty before a designation is made. Combining these insights with the fact that the ESA has been regarded by some scholars (Peltzman, 2004, p. 10) as "a colossal failure, having thus far produced a net recovery rate of under ½ percent (6 of 1300+) of listed species" points to the distinct possibility that the endangered species act is actually endangering, rather than protecting, species.

While these empirical results are interesting in their own right, we view a major contribution of our study as methodological. The methods we develop can be widely applied—from any particular economic environment that has the general sequential-move nature associated with agent interaction to the more narrow case of any restriction that is known to threaten a group of landowners but requires regulatory action before applying to any individual units. Besides species conservation, the most important examples of the latter include "smart growth" and anti-sprawl laws, which often ban or limit land conversion on particular regions of the urban development frontier. Furthermore, the underlying model permits comparison of such analyses across widely disparate times, locations, and types of development restriction. Combined with field work designed to gather appropriate data, this approach is capable of yielding a substantial body of evidence from which the economic and environmental significance of land use policies in general can be measured.

The remainder of our paper is organized as follows. Section 2 presents a model of preemptive development in the context of a growing city. Section 3 provides a brief description of the Endangered Species Act, and contains a description of the data. Section 4 summarizes the results of our econometric analysis. Section 5 concludes.

2 Theoretical Model

A landowner deciding whether to preempt a regulatory act weighs the benefits of preserving development rights against two types of loss. The first loss is the extra interest paid because the investment is made somewhat earlier than would otherwise be the case. The second loss is the result of uncertainty about future land values: it might turn out that the land was not worth developing after all, or not worth developing until a much later time than was expected when the preemption decision was made.

Let $r(t,\mathbf{z})$ denote the rent from a developed parcel with characteristic-vector $\mathbf{z}$ at time t. (The dependence on $\mathbf{z}$ is suppressed in much of what follows). Assume that landowners expect rent to follow a Geometric Brownian Motion (GBM) process with growth rate g and variance σ^2

$$dr = grdt + \sigma rdu \tag{1}$$

where u is a standard Wiener process.[2] The growth rate g corresponds to the most common expression for the growth of an asset value, as a percentage per year. The variance parameter σ measures the extent to which a given day's rent change is likely to depart from expectation. For σ=g, for example, developed land actually falls in value about 16% of the time, and just as often it rises by more than double the expected rate g.

Note that the growth rate and variance parameters do not vary over land; this is intended to represent the time path of demand in a single urban housing market (Capozza and

[2] In Appendix 3 we provide evidence that the growth of residential housing values in our sample is approximately GBM.

Helsley 1990). Let ρ be the interest rate, and assume $\rho > g$.[3] At time t the expected discounted value of rents is

$$V(t,\mathbf{z}) = \frac{r(t,\mathbf{z})}{\rho - g} \tag{2}$$

which evolves according to

$$dV = gdV + \sigma du\,. \tag{3}$$

That is, V evolves according to geometric Brownian motion with the same parameters as r.

$V(t,\mathbf{z})$ is the price for which a parcel with characteristics $\mathbf{z}$ would sell at time t if it had already been developed. For a landowner deciding when to develop, V is the termination value in an optimal stopping time problem. Prior to development, the value $F(t,\mathbf{z})$ of the land follows the Bellman equation

$$F = \max\left\{V - C, \left(y + \frac{1}{dt}E\left(dF\right)\right)\frac{1}{\rho}\right\} \tag{4}$$

where C is the cost of development, assumed constant over land and time, y is the income from undeveloped land, and E is the expectations operator. Intuitively, this implies that the land is worth the greater of what you get by developing now or the expected discounted value of what it will be worth tomorrow undeveloped. The analysis below will be conducted in terms of a simplification, the continuation-region Bellman equation, which holds up until the moment of development, and with y held at zero

$$\rho F dt = E\left(dF\right). \tag{5}$$

[3] Violation of this assumption leads to the case of no landowner ever building except, perhaps, preemptively. This is a general feature of option exercise problems: as long as the value of the underlying asset is growing faster than the interest rate, it is optimal to wait to invest.

Let $D(t)$ be defined as equal to one if the owner has permission to develop, zero otherwise. Assume that landowners believe the evolution of D is governed by a Poisson process

$$dD = \begin{cases} -1 & \textit{with probability } \pi dt \\ 0 & \textit{with probability } (1-\pi)dt \end{cases} \tag{6}$$

evolving from initial condition $D(0) = 1$. The most important limitation to this assumption is its inconsistency with a political or bureaucratic process of known duration – i.e., the case in which it is known that development rights will be lost on a particular day or not at all. It is straightforward to show that in such a case no landowner would act preemptively prior to the last day. In the present case, there is no such known duration, but there are several roughly anticipatable breakpoints, the significance of which we discuss below.

The right-hand side of (5) is an expectation over two sources of uncertainty: the possibility that development rights go to zero, and the stochastic growth of V. The analysis is greatly simplified by assuming that land yields nothing until developed.[4] Under this assumption, the loss of development rights will cause the land to be worthless, which implies $\partial F / \partial D = F$. Making use of this observation and Ito's lemma,[5] the expectation of dF is

$$E(dF) = \left(gV \frac{\partial F}{\partial V} + \frac{1}{2}\sigma^2 V^2 \frac{\partial^2 F}{\partial V^2} - \pi F \right).$$

Substituting this into the continuation-region Bellman equation (5) and rearranging gives

$$(\rho + \pi) F = gV \frac{\partial F}{\partial V} + \frac{1}{2} V^2 \sigma^2 \frac{\partial^2 F}{\partial V^2}. \tag{7}$$

[4] This seems tenable in our sample, and it appears (but we have not proven) that the results we use are robust to sensible generalization (i.e., treating R as the rent differential between developed and undeveloped land).
[5] Ito's lemma is the formula for the expected value of the differential of a function of a stochastic process. This version is discussed in Dixit and Pindyck (1994).

Thus, the risk of preemption acts very much like an increase in the discount rate. The only difference is that this added discount term disappears once the investment is made. That is, the rents, r, are discounted using only ρ to derive the time path of current value conditional on development, V; but to get F, the value prior to development, V is discounted with $\rho+\pi$.

This second step is then exactly the classic investment timing problem first analyzed by MacDonald and Siegel (1986). Dixit and Pindyck (1994: Ch. 4, Appendix B) derive sufficient conditions under which the solution to this problem can be characterized by a critical value $V^*(t)$ such that development occurs the first time $V = V^*$. The assumptions already made are sufficient to ensure that those conditions are fulfilled (in particular, that there is no continuation payment, that the termination value is monotonic in V, and that the resolution of uncertainty on any given parcel is a one-time event). That the threshold varies over time but not over **z** values will become clear below.

Equation (7) is a second-order differential equation in F, subject to boundary conditions

$$F(\cdot,0)=0 \tag{8}$$

$$F(1,V^*)=V^*-C \tag{9}$$

$$\left.\frac{\partial F}{\partial V}\right|_{V^*}=D \tag{10}$$

Condition (8) follows from the observation that the geometric evolution of V implies that once it goes to zero, it stays at zero. Condition (9) is known as the *value matching* condition and simply states that the value of undeveloped land on the day of development is the value after it is developed minus development cost.

Condition (10) is known as the *smooth pasting* condition, and the intuition behind it is somewhat more subtle. Suppose that by waiting for V to rise to $V^* + dV$ the value of the option would rise by more than dV. Waiting would then increase the value of the unexercised option to a level above that of the exercised option, since the value matching condition requires that two values are equal at V^*. Waiting another moment to invest would therefore be optimal, which would violate the definition of V^*.

From the general theory of differential equations, all solutions to (7) are linear combinations of exponentials in the two roots of the quadratic equation that results from plugging $F(1,V) = AV^{\beta}$ into (7), where A is any constant. Denote these roots β_1 and β_2. Dixit and Pindyck (1994) show, with an argument that is not changed by the presence of π, that $\beta_2<0$. This implies that the boundary condition $F(1,0)=0$ can be satisfied only if the coefficient of the β_2 term equals zero (since 0 raised to a negative exponent cannot be a real number).

The key constant governing the behavior of this system is thus the positive root

$$\beta_1 = \frac{1}{2} - \frac{g}{\sigma^2} + \sqrt{\left[\frac{g}{\sigma^2} - \frac{1}{2}\right]^2 + 2\frac{\rho + \pi}{\sigma^2}} \tag{11}$$

The value of the undeveloped land then follows

$$F(1,V) = \left(\left(V^*\right)^{-\beta_1}\left(V^* - C\right)\right)V^{\beta_1} \tag{12}$$

and the threshold value is

$$V^* = \frac{\beta_1}{\beta_1 - 1}C. \tag{13}$$

It is clear from (11) that preemption risk increases β_1, and since $\beta_1>1$ (see (Dixit and Pindyck 1994), 5.2.A), (13) implies that any increase in β_1 lowers V^*. In particular,

$$\frac{\partial V^*}{\partial \pi} = -\frac{\left(\beta_1 + \frac{g}{\sigma^2} - \frac{1}{2}\right)^{-\frac{1}{2}}}{\left(\beta_1 - 1\right)^2 \sigma^2} C < 0 . \qquad (14)$$

This formally establishes the intuition motivating our study: a higher perceived probability that development rights will be lost lowers the threshold value for conversion of land and leads to preemptive development of land.

2.1 *Implications for estimation*

In this section, we focus on how our theory structures the appropriate interpretation of the data and how particular matching estimators can lend insights into the evaluation. Because V follows a geometric Brownian motion process, the moment at which it is expected to be V^* for the first time given $V(t)$ is $\frac{1}{g}\left[\ln\left(V^*\right) - \ln\left(V\left(t,z\right)\right)\right]$.[6] Therefore, the expected time of conversion changes with $\Delta\pi$ by (to a first-order approximation) $\frac{1}{gV^*}\frac{\partial V^*}{\partial \pi}\Delta\pi$. This shift is independent of the current value $V(t,\mathbf{z})$, which means that preemption shifts the expected hitting time back by the same amount for all parcels, regardless of their **z** values. We refer to this hastening of development time as the "timeshift" due to preemptive development of land.

The invariance of the timeshift is due to the geometric nature of the assumed housing-value growth, and is orthogonal to the stochastic part of the GBM assumption. This result is illustrated in

[6] The distribution for the first hitting time under geometric Brownian motion was derived long ago and we borrow it from Bar-Ilan and Strange (1996) who cite Cox and Miller (1965). We note for future reference that higher moments of the distribution of first hitting times have not, as far as we know, been derived analytically; however, simulation of this model indicates a lognormal distribution.

Figure 1, which shows the growth of V over time for a high-value parcel (with $\mathbf{z}^{H}$) and a low-valued parcel ($\mathbf{z}^{L}$). The two threshold values correspond to distinct levels of π. Because a geometrically growing value increases at a rate that is determined by its level, the lower-valued parcel is rising at exactly the same speed when it reaches the lower threshold as was the high-valued parcel when it reached the same threshold much earlier. Intuitively, this suggests that it takes the same amount of time for it to rise to the higher threshold associated with a lower π. With a normal stochastic component, this equality holds in expectation.

The invariance of the timeshift implies that it is inappropriate to measure preemption by including a π-proxy in any of the popular accelerated failure-time models. In such models, a particular parametric form is assumed for the distribution of (in our case) development times and the control variables are assumed to hasten or slow the passage of time. In the above model, the impact of π is additive, not multiplicative — metaphorically, this means that it does not increase the speed of a particular runner in the race, but gives that runner a head start.

The other class of models commonly applied to duration data (i.e., data about the time elapsed until some event) is also inconsistent with our theory. The maintained assumption in these models is that each independent variable has a multiplicative effect on the hazard function $\lambda(t)$ – the instantaneous probability of development given that it has not yet occurred.

In our model, the increase in V required to induce development at each t is $V^{*} - V(t)$. From the definition of geometric Brownian motion, the probability of such an increase during a small interval dt has normal cdf with mean gV and variance $\sigma^{2}V$. Therefore, conditional on not having been developed or subject to takings until time t, the instantaneous probability that a parcel will be developed is

$$\lambda(t) \equiv \Pr\left(dV > V^* - V(t)\right)$$
$$= 1 - \Phi\left(\frac{V^* - V(t) - gV(t)}{\sigma V(t)}\right) \tag{15}$$
$$= \Phi\left(\frac{V(t)(1+g) - V^*}{\sigma V(t)}\right)$$

where Φ is the standard normal cdf.[7]

A first-order approximation of the change in λ induced by a change in the takings probability of $\Delta\pi$ is

$$\Delta\lambda(t) \approx -\phi\left(\frac{V(t)(1+g) - V^*}{\sigma V(t)}\right)\frac{\partial V^*}{\partial \pi}\frac{\Delta\pi}{\sigma V}.$$

Letting $w = \dfrac{V(t)(1+g) - V^*}{\sigma V(t)}$, the ratio of the hazard under high risk of takings to that under low risk is

$$\frac{\Phi(w) - \phi(w)\dfrac{\partial V^*}{\partial \pi}\dfrac{\Delta\pi}{\sigma V}}{\Phi(w)} = 1 - \frac{h(w)}{\sigma V}\frac{\partial V^*}{\partial \pi}\Delta\pi \tag{16}$$

where $h(w)$ is the failure rate of the standard normal distribution.[8] The hazard ratio will therefore be constant iff

$$\xi(V) \equiv \frac{1}{\sigma V} h\left(\frac{(1+g)}{\sigma} - \frac{V^*}{\sigma V}\right)$$

is constant. The standard normal failure rate has positive slope, however, and the slope is less than the failure rate itself -- i.e., $h' < h$ (Evans et al., 1993).[9] Therefore

[7] The final equality, which is purely for convenience, exploits the symmetry of the standard normal density function.
[8] The failure rate is also called the hazard function, but we wish to reserve that term for λ.
[9] The standard normal failure rate is an upward curving function of its argument, near zero and with slope near zero at w=-3, rising to about 3.2 by w=3, with the slope asymptotically approaching 1.

$$\xi'(V) = -\frac{h - h'\frac{V^*}{V}}{\sigma V^2} \tag{17}$$

is strictly negative in the neighborhood of V^*.

This result implies that if we have parcels of many different **z** values affected by the same π, the multiplication of the hazard will be less on those for which the hazard was already high. These are the parcels with V very close to V^*; they will therefore have lower $\xi(V)$ than parcels a bit less valuable. The value $\frac{\partial V^*}{\partial \pi}\Delta\pi$, which multiplies $\xi(V)$ in (16), is the same for all parcels, and is negative.

It is conceivable, of course, that the distribution of **z** values might be of just the sort to yield a standard duration model, but that seems a rather extreme coincidence on which to condition the analysis; hence we focus on four estimators suggested by our theory.[10]

The most familiar of these four estimators is a double-bounded Tobit regression of days to development on the CH dummy variable and control variables – the censored-data equivalent of an ordinary least squares regression in raw time. This model is incorrect for our problem in at least two senses, but we include it as a familiar analog to the less familiar approach discussed next.[11] The second empirical approach is maximum-likelihood estimation

[10] Nevertheless, we have explored some of the duration models that we show as inappropriate. The estimated impacts are statistically significant and roughly consistent with our findings in most contexts. These results are available upon request.

[11] The first problem is that the censoring mechanism maintains the assumption that unobserved development times are distributed normally, rather than lognormally; this could perhaps be dismissed by a central limit argument about the distribution over **z**-values, but to our knowledge no such argument has been developed. The second problem is that the feature of the data that prevents observations of development times prior to January 1997 is not censorship, as Tobit assumes, but truncation. That is, the development times are not coded as occurring at our zero date, they are simply unobserved. In general, one would like to approach such a problem by constructing a likelihood reflecting incidental truncation on the left and censoring on the right, but since our goal is to provide a familiar analog to the less familiar approach discussed next, that would not be appropriate. We should note, however, that we inspected the results of Tobit regressions assuming no problem on the left side of the distribution; results were almost identical to those reported below.

of parameters derived directly from our theory, controlling for all other variables with a semi-parametric matching method discussed in Appendix 1. Under the assumptions already made, the distribution of development times is log-normal with a mean that depends on z and on whether the parcel is CH. Identification of the parameter of interest requires that we further assume that the variance σ_D of log development time is common across parcels. Then we have

$$\ln\left(T-\alpha H\right) \sim N\left(\mu\left(z\right),\sigma_D\right) \tag{18}$$

Now suppose that we match each parcel in the CH with a non-CH parcel that has the same **z** values. Let t_0^* be the development time for a non-CH parcel and t_1^* be the equivalent for the CH parcel. We are not, in general, able to observe every t^* value since development will continue beyond our sampling period, t^+ (February 26, 2001). From (18), however, the likelihood of the sample is given by

$$\begin{aligned} p\left(t_0,t_1 \mid \alpha,\sigma\right) = & \sum_{t_0,t_1<t^+} \phi\left(\frac{\ln\left(t_0\right)-\ln\left(t_1+\alpha\right)}{\sigma_D}\right) \\ & + \sum_{t_0=t^+}\left(1-\Phi\left(\frac{\ln\left(t_0\right)-\ln\left(t_1+\alpha\right)}{\sigma_D}\right)\right) \\ & + \sum_{t_1=t^+}\left(\Phi\left(\frac{\ln\left(t_0\right)-\ln\left(t_1+\alpha\right)}{\sigma_D}\right)\right) \end{aligned} \tag{19}$$

where t_0 and t_1 are the actual, observed times of development or censoring (the end of the sample period), and ϕ and Φ are the standard normal density and probability functions. We will refer to maximization of (19) as the *timeshift* estimator.

The Tobit and timeshift models yield estimates of days accelerated; our remaining two approaches do not, but provide alternative contexts in which to test the hypothesis of no difference between land classes. Our third estimator is simply the count of permit applications during designated intervals following potentially important events, again controlling for observable confounders by matching. Matching also yields treatment and control samples of equal size, so simple counts are also frequency measures. Our theory indicates that any such difference in counts should be interpreted as evidence of a *change* in $\Delta\pi$ at the start of the interval, or at most Δt days prior to the start (where Δt is the number of days by which $\Delta\pi$ hastens development – i.e. α in the timeshift estimator is an estimate of Δt). A persistent $\Delta\pi$ will shift as much development out of later intervals as into them.

Our fourth, and final, measure is a rank-based statistic to compare the survival curves for each of two matched samples – i.e., for each date, the fraction of the sample that remains undeveloped. Our model predicts that the survival curves for well-matched samples should diverge around the onset of treatment, and eventually stabilize parallel to one another in the horizontal (i.e., time) dimension. We test this in the most general way possible by examining the sample statistic corresponding to $P(t_1 < t_0)$. Call this statistic μ. A traditional Wilcoxon test is based on

$$W_\chi = \frac{\left(\mu - \frac{1}{2}\right)^2}{V(\mu)}$$

for some estimate of the variance of μ, $V(\mu)$. Under the null hypothesis that the two survival curves are equal, W_χ has χ^2 distribution with one degree of freedom.

We present estimates of W_χ using a variance estimate derived under the assumption that permits are sought according to a logistic distribution of some monotonic transform of time. Such an assumption is necessary in order to deal with censoring, yet we should note that in our case the alternative assumption of an extreme value distribution produces qualitatively identical results. In calculating bootstrap intervals, we focus directly on a straightforward analog of the above μ adjusted for ties, the sample analog of $P(t_1 < t_0) + \frac{1}{2}P(t_1 = t_0)$ (Halperin et al. 1989). No further distributional assumptions are required, because no variance estimate is needed.

3 ESA Background and Data Description

The exact consequences of designating land as CH remains one of the ESA's "most contentious, ambiguous, and confusing concepts" (Bean and Rowland, 1997, p. 251). Yet, Houck (1993, p. 1436) seems to summarize accurately the general public's perception when he notes that "there is no doubt that critical habitat designations are a red flag to the development community and that community's representatives in Congress." Because of the complexities of potential restrictions on development, we focus on the response of builders to the "red flag" revelation of information that might signal an increased probability of future restrictions to a landowner, rather than assume mechanistic legal or biological linkages.[12] In this spirit, for our purposes, an important feature of the ESA is Section 9, which prohibits any individual from "taking" an endangered species.[13] The only way to avoid liability under Section 9 is described in Section 10 of the ESA. Section 10 requires individuals to obtain an incidental takings permit (ITP), which must be accompanied by a habitat conservation plan detailing how the proposed activity will affect the species, what steps will be taken to minimize such impacts, what alternatives that would not result in takings were considered,

[12] Designation of critical habitat boundaries may be perceived as influencing the treatment of a parcel of land under many development restriction processes, including under potential habitat conservation plans.

[13] A "taking" is usually defined quite broadly, following the original elucidation of the Secretary of Interior, who, in 1975, declared that any significant environmental modification and degradation of habitat was included under the definition of a taking. This general definition has survived judicial scrutiny in the *Palila v. Hawaii Department of Land and Natural Resources* case as well as the 1995 Supreme Court case *Babbitt v. Sweet Home Chapter of Communities for a Great Oregon*. Furthermore, in the latter, the justices ruled that the actor's intent is irrelevant.

etc. Since penalties for violating the ESA depend on the violator's state of knowledge (see, e.g., Bean and Rowland, 1997, p. 227-229), whether the land is within or outside CH boundaries might be critical in determining whether a violator can be held liable.

In our case study, as in most endangered species cases, the endangered species was listed, and a proposed CH map was published, months before the CH went into effect, allowing landowners ample time to respond. The Pygmy Owl was listed as an endangered species on March 10, 1997, with little scrutiny or local press coverage in Tucson, Arizona. On November 14 of that year, protests were held and lawsuits filed against the Amphitheater school district in Tucson which was proposing to build a new high school on a site thought to be valuable habitat for the pygmy owl. Local newspapers subsequently began major coverage of the issue. The proposed CH boundaries, which are depicted in Figure 2, were drafted by the U.S. Fish and Wildlife Service and presented to the public on December 30, 1998. Official designation occurred more than seven months later, on August 11, 1999.[14] The CH designation displayed in Figure 2 covers roughly 1.2 million acres and was based on the biological requirements of the species.[15]

3.1 Data

[14] In our figures and tables, these dates are referred to as Listing (List), Protests (Pro), Map, and Final respectively.

[15] In practice, the boundaries designated were unchanged from the draft boundaries presented during the hearings (U.S. Department of the Interior, Fish and Wildlife Service 1999). This outlines the status quo when our data were generated. Later, however, an Arizona District Court vacated the critical habitat designation, citing deficiencies in the economic analysis. There will soon be a proposed rule issued in response to the court's order. Before the final publication of the CH designation, the Department of the Interior will evaluate the economic impact of the CH designation and may exclude parts of it for economic and other considerations. The new proposal covers more area than the 1999 CH designation, but less of it is private land. The reader should bear in mind that, if such a reversal is anticipated, landowner incentives to preemptively develop land are considerably less than otherwise would be the case. A landowner with perfect foresight would preemptively develop only if she had been otherwise planning to develop during the two and a half years following August 1999; otherwise, it would potentially pay more to wait to preemptively develop the land during the new window of opportunity opened by the reversal.

Of more than 300,000 parcels in Pima County, Arizona, 62,379 constitute the sample analyzed herein (58,706 non-CH and 3,673 CH parcels). This lower figure was obtained because all parcels that were already developed prior tom our sample period were removed, in the belief that loss of development rights, and therefore the motivation for preemption, is largely limited to land on which nothing has yet been built. A parcel was thus removed from the sample if the Pima County Assessor's office recorded non-zero value of improvements at the start of 1997. The partitioning of the land into parcels corresponds to records maintained by the county's Department of Transportation and Planning and Zoning Office. Parcels are of various sizes and shapes and are the units in which building permits are requested. For each parcel, we identified the date that a construction permit was sought; this is our measure of development timing.[16]

The variables used to correct for differences between CH and non-CH parcels consist of an "ecological" and an "economic" group. The former variable type includes aspects of landscape topology calculated from maps – elevation, slope, and aspect (the compass direction a slope faces) – as well as soil type, plant coverage, hydrological class, plant cover, satellite measures of greenness in 1996, and USDA estimates of suitability for twelve types of habitat. These were taken from the U.S. Geological Survey and the U.S. Department of Agriculture, and are measured on a fairly coarse grid.

Most of the economic data are from the 1990 Census and are measured on an even coarser grid – the block or block group. These data consist of mean commute time to work, rental rate, population, housing vacancy, median home value, median rent, unemployment

[16] According to the permit database, the vast majority of permits applied for are awarded, and construction begins soon after the permit award date. An interesting analysis would be to estimate whether in- and out-of-state landowners reacted similarly. We reserve this examination for another occasion.

rates, and mean salary. Also included in the economic data are distances to amenities and disamenities identified from the county land-use maps – golf courses, prisons, highways, parks, and downtown areas -- and for these data each parcel has its own value. The distance calculated is to the nearest example of each entity.

Actual regressions were run on the principal components derived separately from ecological and economic variables. Ten components from ecological variables and nine from the economic variables are used in the results presented, capturing about 99% of the variance in the data (a more detailed explanation of our data collection is contained in Appendix 2).

Over our time period, construction permits were issued to 1,409 of the 3,673 CH parcels, and to 16,028 of the 58,706 non-CH parcels. If the plots were identical, this statistic alone would provide interesting evidence, since the proportions are quite uneven. More compelling, however, is an examination of the temporal development rates across CH and non-CH parcels. Consider Figures 3a and 3b, which provide development rates across CH and non-CH parcels around events likely to raise fears among owners of habitat land. Trends in Figure 3a for non-CH reveal that construction permits were roughly stationary—around 325 permits during our time period. There is little evidence of a large and meaningful effect of the various events in these data.

Alternatively, Figure 3b, which plots construction permits on CH parcels, shows a sharp increase in permits just prior to final designation of the habitat area on August 11, 1999: from December 1998 to just before August 11, 1998, permit applications tripled, whereas they slightly decreased for non-CH parcels over this same time period.

While these data provide compelling insights, such a comparison of the treatment and control groups might be misleading. CH parcels are somewhat higher in altitude and considerably less steep than surrounding land. And, CH parcels are in wealthier parts of the region—the Census tracts within which CH parcels lie have average salaries more than twice as high and median housing values about 30 percent higher than other tracts; and the assessed values of the tracts are about 10 percent higher for CH parcels. Parcels outside the CH area are roughly twice the size of those within, and the USDA classifies CH land as significantly more suitable for wildlife requiring open land. Furthermore, CH land is predominantly (>90%) of the hydrological class B, a classification representing the best-drained regions.[17] Thus, it is possible that certain observable variables affect which units get treated and the outcome of interest. This potential nuance motivates us to consider the empirical models derived theoretically above.

4 Empirical Results

4.1 Preliminary: Some aspects of the matching procedure

In the empirical models below we make use of the entire data set and various matched samples. To obtain the matched pairs we first estimate a probit regression model to predict CH status, and then match each CH parcel to the non-CH parcel with the closest predicted probability of being CH (known as the "propensity score").[18] By matching every CH parcel with a non-CH parcel, we obtain the "Nearest Neighbor" sample identified by the tag "NN" below. Other more narrowly defined samples are formed by restricting the match to parcels with propensity scores that are sufficiently similar *and* that are near to one another

[17] For the matching estimators discussed below, this component of difference was dealt with by removing from the sample all the tracts that were not class B.

[18] Empirical results based on propensity score matching are typically preceded by a discussion of the probability model used to generate the propensity scores. In the present case, substantial multicollinearity required that these models be estimated on principal components (see Appendix 1), which renders the coefficient estimates uninteresting. Hence, discussion is omitted, but we make the results available upon request.

geographically. Of course, the purpose of matching is to develop a "balanced" sample – that is, one in which the distribution of covariates is similar in the CH and non-CH sub-samples; this two-dimensional criterion was found to yield balance superior to that achieved with propensity score matching alone. Thus, we provide several estimates from samples with propensity scores not more than *X*% apart and locations not more than *X* thousand international feet apart, identified by "PX GX" below. The use of the same *X* for the geometric and propensity bandwidths was decided upon after initial exploration indicated that no great improvement in balance statistics was available using different bandwidths

The only matched sample that can be said truly to have "passed" a balance test consists of pairs that had propensity scores separated by no more than 1%, and were within 1000 feet of each other. As the matching criteria become more liberal, the number of matched pairs rises and the balance deteriorates (see Table 1a). In the case of NN matching, which by construction matches all CH parcels, the hypothesis of equal first moments between CH and non-CH samples could be rejected for any of the 26 control variables at the $p < .10$ level. Therefore, in addition to the simple comparison of CH and matched non-CH sub-samples, we consider regression-adjusted estimates in which the variables that were not balanced by that criterion ($p < .10$) are included as covariates together with a CH dummy in a suitable model of development timing.

Finally, note that we provide for most estimates two confidence intervals. The first, based on standard analytic formulae, would be correct if the matching were perfect – i.e., if the non-CH parcels were identical to the CH parcels. These are almost certainly too narrow. The second, which is omitted if the first showed no significant impact, is based on bootstrap replications of the entire data-processing sequence. These are likely too wide. A more detailed discussion of these points is provided in Appendix 1.

4.2 Estimates of timeshift

In Table 1 we present measures of preemptive acceleration in the natural unit, days of difference between CH and non-CH plots. The first column in Table 1 consists of two-sided Tobit models using the entire sample, which are maximum likelihood estimates of β_0 in

$$
\begin{aligned}
& t = \alpha + \beta_0 CH + \sum_{i=1}^{19} \beta_i PC_i + \varepsilon \\
& \varepsilon \sim N\left(0, \sigma^2\right) \\
& t_+ \leq t \leq t_-
\end{aligned}
$$

where t is the time of the first permit application in days from start of observation, CH is a dummy variable equal to one if the land is inside the CH area (shaded portions of Figure 2), and $PC_i, i = 1, 2, ..., 19$ are the principal components described above. We include three distinct time frames in the rows of the table: in the top row, Full, t_-=0 and t_+=1516, the final day of observation; in Pre-map we explore from t_-=0 to t_+=727, the day the map was published (December 1998); and in Post-map, t_-=727, t_+=1516. Maximum likelihood 95% confidence intervals are in parentheses; for comparison to later matching results, we also present non-parametric bootstrap intervals.

The remaining three columns are from maximization of the likelihood function (19) with the variance parameter σ set to 500 or 1000 for two of the matched samples.[19] The final

[19] Results with σ left free could not be obtained because the likelihood function increases monotonically in σ. However, under the maintained assumptions of fixed σ and perfect matching, these estimates differ significantly from zero and have the expected sign.

column in Table 1 provides timeshift estimates for the NN sample. The estimates that are to be favored *a priori*—two timeshifts from the well-balanced sample P1G1—indicate preemptive acceleration of nearly one year (272-326 days in the post-map), and slightly greater than one year (331-485 days in the Tobit full sample model), respectively. A few remarks are in order about the implications of these results. First, preemption of one year corresponds to approximately 2% of the sample habitat. Second, the analysis of price data in Appendix 2 indicates a growth rate of about 1-3% quarterly, with a standard deviation of about the same magnitude. The lower end of these growth rates seems quite improbable, given the perception that home values were appreciating rapidly (although this should not be seen in light of exceptions formed since 2000). Erring, therefore, on the high side and rounding, assume that we have growth rates and variance parameters of about 0.1 in annual units. Interest rates at the time were in the 7-9% range. Simulations based on our theory using these parameters indicate that a one year shift in permitting time is consistent with a perceived differential in the fear of losing development rights ($\Delta\pi$) of about 0.2% per year.

4.3 Rank-based tests of significance of timeshift

As an alternative estimator, consider rank-based tests for the equality of the CH and non-CH survival curves in the matched samples, which are presented in Table 2. Recall that these tests depend only on the order in which parcels were developed, ignoring the precise length of each interval between development dates. The tests are thus insensitive to variations in the overall development rate – i.e., whether development was occurring steadily, or accelerating, or had a peak of activity.

The first two columns in Table 2 provide the actual number of permit applications observed in each category and a predicted development number, where the prediction is the sum over all time periods of the number of parcels still awaiting their first permit and the sample-wide (i.e., CH and non-CH) rate of permit application. The main value of these numbers is that they provide an indication of which set of parcels correspond to the earlier developments, which cannot be deduced from the Wilcoxon χ^2.

In each case it is the CH parcels that are developed more than predicted, consistent with our theory of preemption: the Wilcoxon χ^2 statistic reported in the third column is significant at all levels in each case except the small (75 pair) sample P1G1 and during the pre-map phase in P10G10. In the final column, parallel statistics are presented for the ordering of residuals from Cox proportional hazard regressions on all the unbalanced variables in each of the larger matched sets.[20] Empirical results are nearly identical to those of the unadjusted tests, indicating that in this context the assumption that covariate influences on timing are orthogonal to the treatment effect is not more restrictive than the assumption that those influences act to multiply the hazard. In terms of hypothesis tests, the only difference is that the pre-map impact for P10G10 is now significant at the $p < .05$ level.

Frequency of permit application in designated intervals

A complement to the rank-based tests is an analysis of the counts of the numbers of parcels in and out of the CH for which permits were sought in each time period (see Table 1a in Appendix 1). These figures suggest that while the best-matched sample does not provide evidence of preemptive development, it is immediately obvious that such a narrow test lacks power. For example, some of the larger samples in Table 1a suggest significant differences exist. But, the unbalanced nature of the regressors renders such estimates speculative.

[20] The Cox model is the most general proportional hazards model, and nests most of the popular fully parametric duration models.

We address this matter with probit estimates of the CH-dummy coefficients from Probit[21] regressions including all of the control variables not balanced at the 90% level in each matched sample; that is, these are maximum likelihood estimates of β_0 in

$$\Pr\left(t_- \le t \le t_+\right) = \Phi\left(\alpha + \beta_0 CH + \sum_{i \in \Upsilon} \beta_i X_i\right)$$

where Φ is the standard normal cumulative density function. The interval dummies offer an alternative way to calculate the significance of preemptive development to thwart conservation goals.

For parsimony, we suppress these results but note that they are much weaker and considerably noisier compared to preemption measures in Tables 1 and 2. Yet, the NN probit estimates show an increased probability of about 6%; the P10G10 shows a 40% increase. These estimates provide an idea of the range of these calculations, and highlight that these point estimates of preemption are in the broad range of those implied by the timeshift estimates.

4.4 Impact of CH status on the price of undeveloped land

> *The incentives are wrong here. If a rare metal is on my property the value of my land goes up. But if a rare bird is on my property the value of my property goes down.* --Sam Hamilton, former U.S. Fish and Wildlife Service administrator for Texas

In our theoretical model there are two reasons why one parcel might be developed earlier than another: a difference in π or a difference in $\mathbf{z}$. Thus far, we have examined strategies to control for the influence of $\mathbf{z}$, and have found evidence that CH designation introduced a π differential. As a robustness check of these results, we examine an alternative

[21] Logit regression adjustments were also performed, yielding the same pattern of likelihood-based significance. The logit, however, turned out to be a much inferior fit to the data; among other things, it often failed to converge under resampling.

data set in which the two sources of difference in development timing should be sharply distinguished. Parcels developed earlier due to their **z** values should sell for higher prices than less desirable plots; parcels developed earlier due to preemption motives should sell for less than those under lower regulatory risk.

We collected data on approximately 7,000 sales of parcels that were undeveloped during the entire time period and regressed the log of sale price per acre on the CH dummy, time dummies, the 19 principal components of controls, and the interaction of time and CH status dummies. The estimated coefficient on the interaction of the CH and post-map dummy variables is of the greatest interest. If we are observing preemption, then we expect a negative coefficient. If all of our results are due simply to insufficient control for CH land being more valuable, the estimated coefficient should be zero, since the CH dummy variable alone will capture the linear impact of that difference. And, if our strongest evidence, which is from increasing development differentials after the map was published, is actually due to an increase in the development value of unobserved features of the CH regions that occurred at about the same time, the coefficient should be positive.

Empirical results are presented in Table 3; time is divided only into pre- and post-map intervals in the right-most column, corresponding to the sort of sharp treatment on with which most of our analysis above was conducted. We find that the coefficient of interest is negative and statistically significant at conventional levels, indicating that undeveloped land fell in value by about 22% if it was within the critical habitat boundaries. To our best knowledge, this is the first empirical estimate of such an influence, but it corresponds to our theoretical model and extant intuition, as per the Hamilton quote beginning this section. When time is divided more finely, as in column 1 of Table 4, none of the interaction terms is significant, but those corresponding to after the map was published remain negative and statistically significant jointly at conventional levels.

In sum, our estimates all point to the importance of offsetting behavior among landowners.[22] While an entire analysis of the proper counterfactual to answer whether these offsets have entirely undone (or more than undone) the spirit of ESA's intended effects is beyond the scope of this study, basic success rates of the ESA suggest that it is a distinct possibility. When the ESA was passed, there were 119 species listed; since 1973, 40 species per year have been added to the list (Peltzman, 2004). A large portion of these 1,300 species that had been listed have their habitat on private lands, forcing regulators to rely heavily on private landowners. The recovery process has not gone well by any measure, as Peltman (2004) calculates that only 6 of the 1,300+ species have been recovered by the ESA. Our exploration provides empirical estimates that shed light on reasons for this low recovery rate.

5 Conclusions

Whether the Endangered Species Act is having its desired effect clearly depends on the response of landowners and developers. The prospect that these agents will view aspects of the Act as a threat to their development rights and respond by developing preemptively opens up the possibility that the Act could actually endanger the same species it is purporting to protect. This paper offers an estimation of the likelihood of that possibility. We develop theory and offer a menu of theory-driven empirical models to measure preemptive development in response to critical habitat designation.

[22] In this light, our estimates complement the original automobile safety work of Peltzman (1975) and more recent work of Acemoglu and Angrist (2001), who examine the Americans with Disabilities Act.

We estimate that land designated as critical habitat was developed about a year sooner than similar land not so designated. These estimates highlight the vast extent to which landowners view critical habitat designation as a threat to development rights and act to exercise those rights before regulation abrogates them. While our species potentially covers a large land area, such a result provides the first evidence that this component of the ESA could effectively seal the fate for species occupying a small area. This result is made even stronger when one considers that every theoretically permissible approach to the data yields insights consistent with preemptive land development. The policy implication is perhaps stronger than it would first appear. Under the letter of the law, critical habitat designation on private land provides, in most cases, no statutory protection to the species beyond that enjoyed on other land. Thus, even a tiny preemptive response may indicate that this particular aspect of the law is quite harmful to the species it seeks to protect.

Besides providing the first set of empirical estimates of what may be the most widespread form of preemption, we view a major contribution of the paper to be methodological. The methods herein apply to a wide variety of economic decisions that involve a sequential move process within a principle-agent relationship. More narrowly, the estimators speak to a wide variety of land-use restrictions contemplated on the fringe of growing cities. Beyond the development of such new estimators, one important lesson has been learned which should be considered by future researchers in this particular area of study. These data, although merged from public records gathered for disparate purposes, and largely measured on spatially lumpy units, appear to be adequate for our purposes. This insight should provide a degree of confidence to others that well developed parsimonious models can be estimated with even the coarse land use data that are publicly available.

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Figure 1: Invariance of timeshift to z

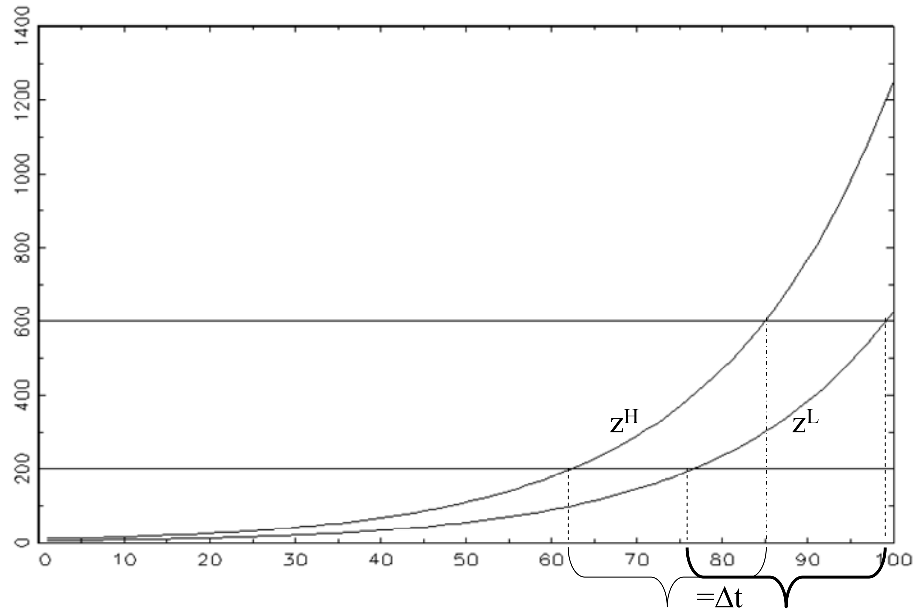

Figure 2: Critical Habitat Area

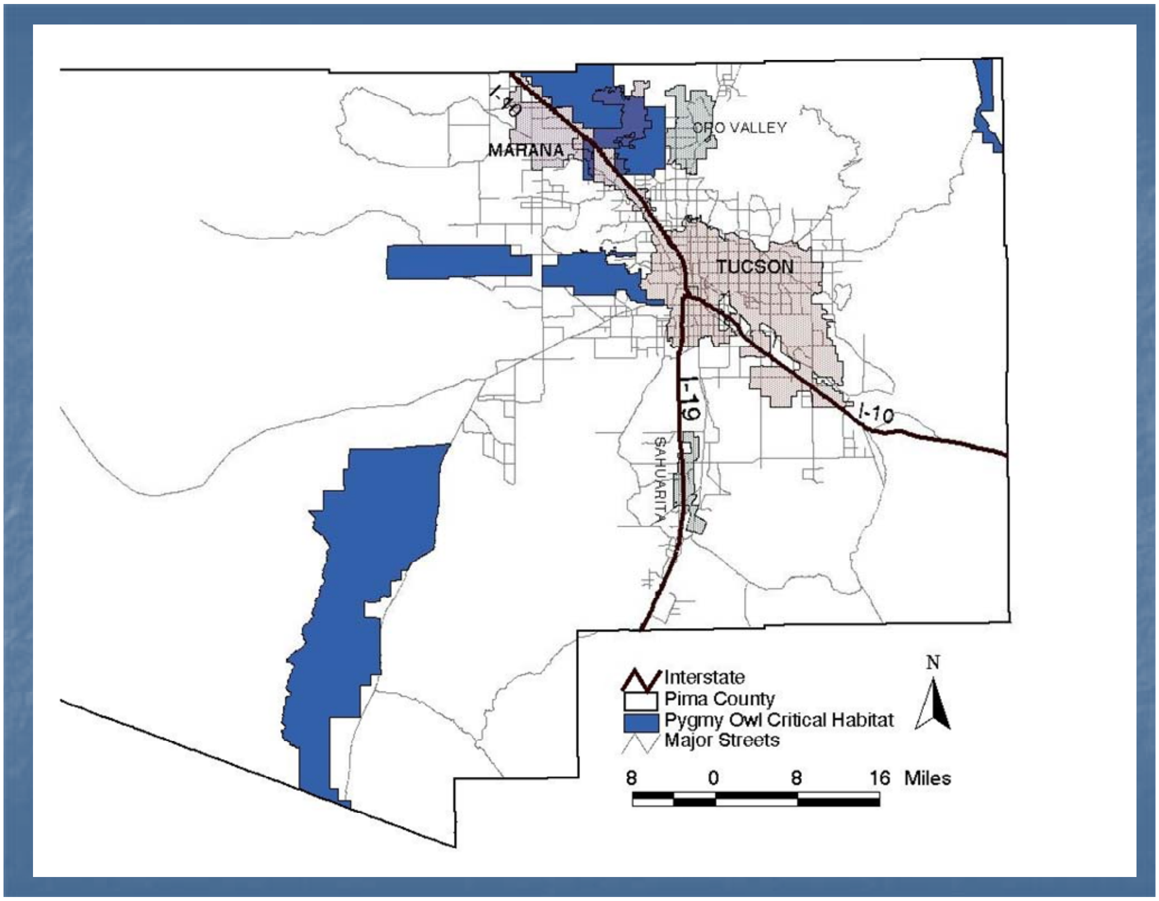

Figure 3a: Monthly permits (Pima County) outside of critical habitat

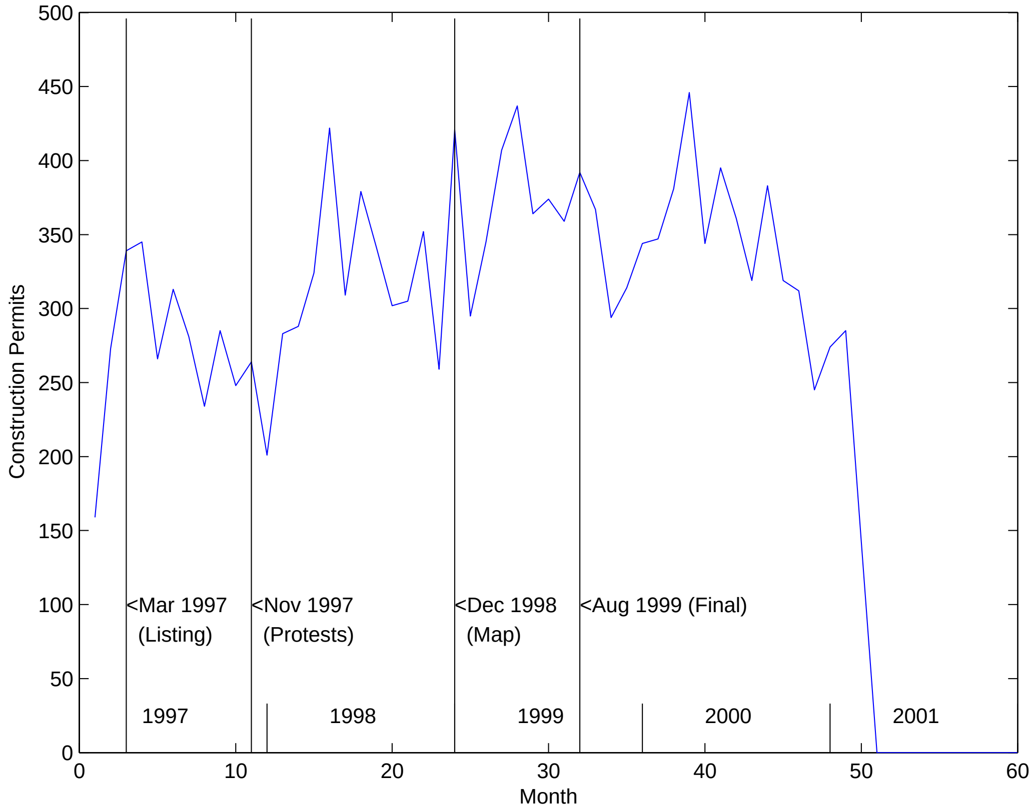

Figure 3b: Monthly permits inside of critical habitat

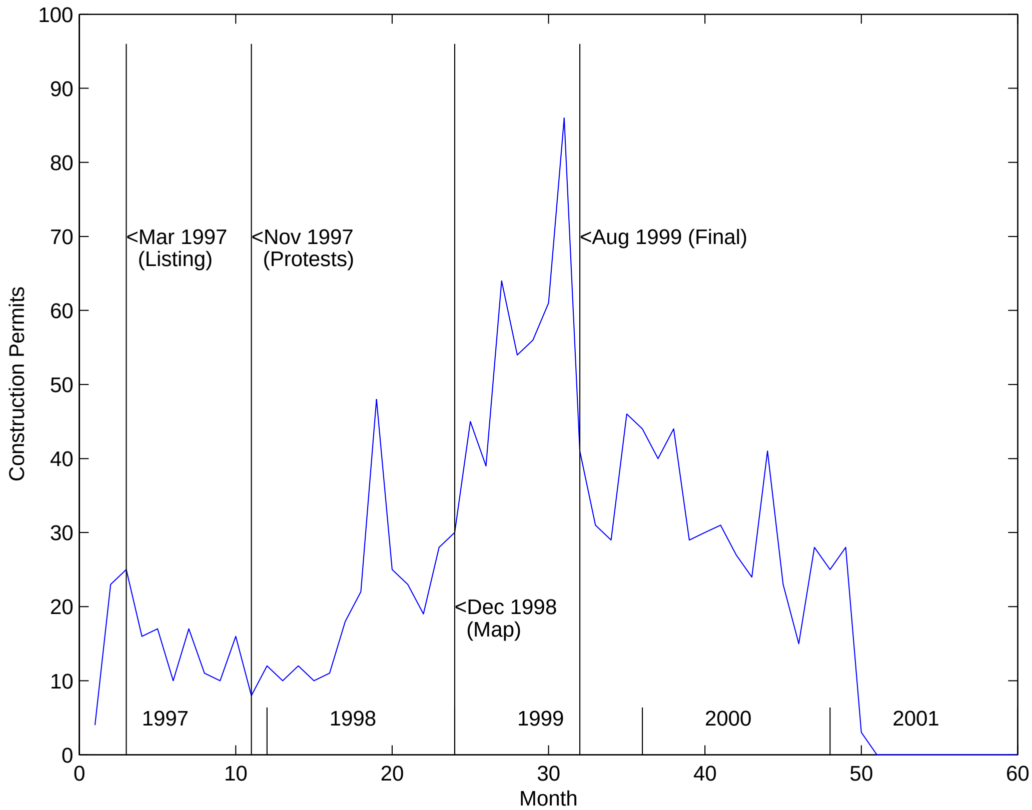

Table 1: Measures of preemptive acceleration in days

Sample	Tobit Full Sample	Timeshift σ=500 P1G1	Timeshift σ=1000 P1G1	Timeshift σ=500 NN
Full	288 (228-347) [221-347]	248 (229-267) [NA]	273 (231-315) [NA]	2788 (2675-2901) [NA]
Pre-map	300 (250-349) [245-346]	NA	NA	808 (NA) [NA]
Post-map	408 (331-485) [323-496]	299 (272-326) [NA]	317 (260-374) [NA]	1568 (1447-1689) [NA]
Controls	P.C.	None	None	None

95% confidence intervals from maximum likelihood in parentheses, from non-parametric bootstrap distribution in square brackets. NA means not available, indicating extreme estimator instability.

Table 2: Wilcoxon tests of equality of survivor functions in selected matched samples

		APPLICATION DATE				COX RESIDUAL
Sample		Events	Predicted	Wilcoxon χ^2		Wilcoxon χ^2
P1G1	non-CH CH	26 29	27.5 27.5	0.07 $p \approx 0.763$		
Pre-map	non-CH CH	9 10	9.54 9.46	0.06 $p \approx 0.8020$		
Post-map	non-CH CH	17 19	17.95 18.05	0.02 $p \approx 0.8951$		
P5G5	non-CH CH	65 360	234.11 190.89	258.60 $p \approx 0.0000$		260.92 $p \approx 0.0000$
Pre-map	non-CH CH	8 56	27.36 26.64	27.88 $p \approx 0.0000$		28.01 $p \approx 0.0000$
Post-map	non-CH CH	57 314	206.75 164.25	232.54 $p \approx 0.0000$		235.01 $p \approx 0.0000$
P10G10	non-CH CH	263 576	436.29 402.71	123.64 $p \approx 0.0000$		186.08 $p \approx 0.0000$
Pre-map	non-CH CH	141 148	146.13 142.87	0.44 $p = 0.5803$		3.92 $p \approx 0.0478$
Post-map	non-CH CH	122 428	290.17 259.83	196.23 $p \approx 0.0000$		239.09 $p \approx 0.0000$
NN	non-CH CH	519 1343	984.5 877.5	265.53 $p \approx 0.0000$		260.24 $p \approx 0.0000$
Pre-map	non-CH CH	272 417	347.15 341.85	34.57 $p \approx 0.0000$		10.31 $p \approx 0.0013$
Post-map	non-CH CH	247 926	637.35 535.65	509.60 $p \approx 0.0000$		542.59 $p \approx 0.0000$

Terms in square brackets are bootstrap 90% confidence intervals.

Table 3: Hedonic regressions on CH Location Dummy and Time Interval Dummy

	ln($/Acre)	ln($/Acre)
CH location	0.855838** (0.204682)	0.884948** (0.097543)
List-prot	-0.27961** (0.080044)	
Interaction with CH location	-0.07491 (0.265221)	
Prot-map	-0.43512** (0.074627)	
Interaction with CH location	0.083693 (0.233004)	
Map-CH	-0.48089** (0.078323)	
Interaction with CH location	-0.11414 (0.254996)	
Post-CH	-0.48579** (0.073588)	
Interaction with CH location	-0.22019 (0.236046)	
Post-map		-0.16261** (0.036718)
Interaction with CH location		-0.22165* (0.131315)

OLS standard errors in parentheses
* Statistically significant at 90% level
**Statistically significant at 95% level
Pre-listing is omitted time category in first column, pre-map in second.
19 principal components were also included in both regression models.

Appendix 1: Matching

In any study focused on the impact of a single dummy variable (or "treatment"), the ideal approach is to apply the treatment to a randomly selected fraction of the sample. When the data are not from a completely randomized experiment this is impossible, and anything that affects both which units get treated and the outcome of interest biases the estimate of the effect of treatment. Additional information about the sample (i.e., other variables) can be used to mitigate this bias in one of two ways. The tactic most familiar to econometricians is multivariate regression of the outcome on the treatment dummy and control variables. The coefficient on the treatment dummy then measures the effect of interest under the maintained assumption that all the control variables affect the outcome in the way assumed by the regression model, such as linearly, quadratically, or logarithmically.

Another approach is to discard part of the sample, selecting treatment and control groups in a way that mimics randomization of treatment. The objective is that the observable covariates have the same distribution in the two groups, in which case they are said to be perfectly "balanced". If all the covariates took on only a few discrete values, and if there were plenty of non-CH parcels, we could construct a perfectly balanced control group by matching each CH parcel to an identical non-CH parcel. With continuous data this is impossible, and since we have many variables it is not even possible to find for most treated units a control that is nearby (i.e., a "curse of dimensionality").

We therefore adapt a procedure known as "propensity score" matching (Rosenbaum and Rubin, 1983). This procedure involves first estimating a model in which CH status is assumed to depend on the observed covariates. The predicted probability of being in CH (the "propensity score") is then used to match CH and non-CH parcels. This approach has been widely used to measure the impact of program participation on labor market outcomes (see, e.g., Heckman et al. 1997) and is expanding rapidly to other measurement problems (e.g., List et al., 2003).

In our case, superior balance was found to result if the propensity matching was complemented by a limitation on the geographic distance between parcels.[23] Although this decision was made purely on the basis of *ex post* balance diagnosis, it might have been suggested *a priori* as a way to take maximum advantage of the spatial correlation inherent in much of the data – i.e., the fact that neighboring parcels have very close distances to important amenities and disamenities, and often identical values for data measured at the census block or similar level. Restricting the matches to geographic neighbors also mitigates bias due to unobserved spatially correlated variables, the variation of which is not well captured in our data.

When it is not possible to achieve acceptable balance terms of all covariates, a hybrid of the regression and matching approaches is used: first match, then regress the outcome variable on the treatment dummy and those covariates that remain most imbalanced. In our case, we report both unadjusted differences and regression adjusted results using all covariates

[23] To assess balance, we examined the simple difference in first moments for each variable, as well as histograms and kernel density plots allowing visual assessment of higher moments. No attempt has been made to assess balance in terms of more than one variable simultaneously. Probit-based propensity models were found to perform consistently better than logit or linear-probability models.

for which *t*-statistics reject equality of means between CH and matched control parcels at a 10% level.

Since the matching process itself involves estimation, analytic confidence intervals are too narrow. We therefore report both analytic intervals, which measure confidence conditional on perfect matching, and (non-parametric) bootstrap intervals. The idea behind bootstrapping is to sample with replacement from the data repeatedly, recalculating the statistics of interest each time. The confidence intervals are then bound by corresponding percentiles of the distribution of the statistics so calculated. If in fact the observed data are independent observations, this procedure yields distributions approaching those that would be achieved by independent sampling of the underlying universe – i.e., those that form the basis of ideal frequentist intervals and hypothesis tests.[24]

Data gathered from a geographic region are not, in fact, independent, which is one reason why the analytic intervals are also reported and should not be ignored.[25] At the very least, our data are weakly dependent because they constitute much of a finite sample. Further, one might reasonably argue that what we are in fact observing is a single realization of a process spread over space – which is what the timeshift estimator assumes. In the case of a low-order autocorrelation process in a time series, the non-parametric bootstrap can greatly overestimate the variance of the simple estimator of the mean value (Davis and Hinkley 2003). The analogy to spatial autocorrelation is straightforward.

It is not always clear that reported bootstrap intervals are calculated as they should be. The ideal is to replicate every step of data processing that could possibly vary across samples. The only sense in which we fall short of this ideal is that we removed from consideration parcels of drainage classes that are extremely rare in the CH (see below). After drawing each sample, we calculated a sample-specific propensity score; eliminated parcels for which any variable took on a value outside the common-support region (i.e., the set of values bound by smaller of the treated and control maxima and the greater of the two minima); determined which variables required regression adjustment; and calculated the several measures of preemption outlined in the previous section. And all of this was done with a ballpoint pen on legal pads; it's amazing we survived.

To summarize and add a few details: we first remove parcels outside the common support region, then run a probit regression to predict CH status. We then match each CH parcel to the non-CH parcel with closest predicted probability of being CH. This gives the "Nearest Neighbor" sample identified by the tag "NN" in the text. Other samples are formed by restricting the matching to those with propensity scores not more than X% apart and locations not more than X thousand international feet apart, identified by "PX GX".[26] The use of the same X for the geometric and propensity bandwidths was decided upon after initial exploration indicated no great improvement in balance statistics was available using different

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[24] There are by now many excellent treatments of the bootstrap, but Effron (1979) remains a good starting point for those seeking a more thorough discussion. See also Meeker (1998) and Davison and Hinkley (2003) for recent discussions.

[25] The analytic intervals also assume independence, but the bias resulting from violation of this assumption is likely exaggerated in the bootstrap. Intuitively, bootstrap samples that pick up several replications from each of set of autocorrelated observations will exaggerate the frequency of values associated with those observations. See Lahiri (1992) for a case in which this is intuition is backed by proof.

[26] In the latter cases, the matching was done with replacement, although the nearest neighbor matched sample was created without replacement. This incongruity was accidental; preliminary exploration indicated that keeping or dropping matched controls makes almost no difference.

bandwidths. The timeshift, interval counts, regression adjustments to interval counts, and rank-based tests of survival curve similarity were all calculated on each of these matched samples, and a double-bounded Tobit regression was run on the whole sample. Non-parametric bootstrap intervals were calculated for most estimators, but this step was omitted for those which gave no preliminary evidence of preemption; for the timeshift estimator, which was simply too unstable to generate meaningful numbers in many replications; and for the a regression adjustment to the rank-based tests, which yield results very close to the unadjusted data.

Table 1a provides a data summary. In Table 1a, we include all of the relevant time frames that might be of interest. The features of the matching scheme relevant to the problem at hand appear at the bottom of Table 1a. The only matched sample that can be said truly to have "passed" any balance test is P1G1 (columns two and three). The "P1", recall from Appendix 1, implies that these pairs had propensity scores separated by no more than 1%, and the "G1" means they were within 1000 feet of each other. Thus, this sample consists of pairs of parcels adjacent to the CH boundary, discarded if their propensity scores were not quite close. There are 75 such pairs. The balance test they passed is reported in the bottom row, as the zero under "Ill-balanced controls". This zero means that for no covariate could the hypothesis of identical means between CH and non-CH be rejected at the 10% level by a traditional t-test.

As the matching criteria become more liberal, the number of matched pairs rises and the balance deteriorates, as one would expect. In the case of the nearest-neighbor matching, which by construction matches all 3332 CH parcels, the hypothesis of equal first moments between CH and non-CH samples could be rejected for any of the 26 control variables at the 10% level. The regression adjustments below make use of all the variables being used in this last row.

One further point requires mention as an indication of non-representative sampling. The matching process seems to pick up a disproportionate number of the parcels for which permits were sought: nearly half of the best matched sample (72 of 150) and more than a fourth of all the others. In the region as a whole, only 13% of parcels were the subject of permits during observation.

Table 1a: Development Permit Applications During Designated Intervals

Interval (# days)	P1G1		P5G5		P10G10		NN	
	CH	NCH	CH	NCH	CH	NCH	CH	NCH
Pre list (67)	1 (0.25-5.4)	1 (0.25-5.4)	8 (3.5-15.7) [0-7]	1 (0.03-5.6) [0-4]	15 (8.4-24.7) [4-19]	3 (0.6- 8.8) [0-21.4]	28 (18.6-40) [15-38]	8 (3.4-15.7) [1-38]
List-prot (249)	0 (0-3.6)	1 (0.25-5.4)	16 (9.2-25.9) [0-15]	1 (0.03-5.6) [0-5]	64 (49.5-81) [23-75]	52 (39- 68) [3.6-193]	120 (100-143) [82-138]	105 (86-127) [35-571]
Prot-map (411)	11 (5.7-18.6)	8 (3.4-15)	20 (12.3-31) [7-24]	6 (2.2– 13) [3-56.55]	65 (50.4- 82) [27.6-74]	91 (74-111) [6-199]	266 (236-298) [203-298]	159 (136-185) [109-399]
All above (Pre-map)	(6.4-19.7)	(4.9-17.4)	(23.7-47) [7-46]	(3.5-15.7) [3-66]	(122-168) [55-168]	(124-170) [10-413]	(377-453) [299-474]	(242-305) [145-1008]
Map-final (224)	9 (4.2-16.2)	5 (1.6 -11)	60 (46.2-76) [8.45-67]	2 (0.24-7.2) [3-74.1]	147 (125-171) [68-161]	46 (33.8-61) [10-327]	404 (368-443) [335-437]	104 (85-126) [60-329]
Post-final (565)	14 (7.9 - 22)	20 (12.8-29)	254 (228-280) [21-268]	55 (41.8-71) [3-65]	280 (250-311) [54-302]	81 (65-100) [9-405]	522 (481-565) [477-564]	143 (121-168) [84-254]
N Matched	75		798		1534		3332	
N Permits	72		425		847		1862	
Ill - balanced Controls	4 0		11 5		21 20		26 26	

90% confidence intervals based on binomial distribution in parentheses; from percentiles of bootstrap replications in square brackets. Number out of 26 possible control variables on which t-tests reject equality between samples at 50% and 10%.

Appendix 2: More details on data collection

We integrated databases from several sources to develop a Geographical Information Systems (GIS) dataset that represents parcel specific development and habitat status as well as the date of the first construction permit awarded for each parcel during our study period. Parcels in this dataset were spatially linked to data representing factors that may impact parcel rental rate and habitat value.

The parcel specific dataset was based on the Pima County Department of Transportation (DOT) GIS database of geo-referenced parcel polygons. The data were provided in the state-plane coordinate system and international survey feet were the basic unit of distance. The official critical habitat of the pygmy owl was also supplied as a GIS coverage by the DOT. We set a dummy variable (OWL) for each parcel within the critical habitat. A parcel level dataset with the dates of all of the 58,644 construction permits awarded from January 1, 1997 through February 26, 2001 was provided by the County Planning and Zoning office. We identified the earliest construction permit associated with each parcel and linked the permit dates to the parcel dataset using parcel-id.

We grouped parcels according to when (and if) the first construction permit was awarded for that parcel. The Pima County Assessor's office provided an assessed value and zoning parcel database frozen as of the beginning of 1997, the start of the study period. We linked these to the parcel database using parcel-id. The assessed value of improvements on parcels was used as to determine if a parcel had been developed prior to the study. All parcels that had a positive value for assessed improvements were considered developed and removed from the dataset. Some parcels were subdivided following the 1996 dataset so they could not be directly linked using parcel ID. Those parcels were assumed to have zero assessed improvements. This left the number of parcels that were classified as undeveloped at the beginning of the dataset at 111,763. The Pima County Assessor's office also provided data on all sales from January 1, 1997 to January 1, 2001. Sales data included the price and the date of the sale, and were linked to the parcels through the parcel ID number. There were approximately 100,000 sales. We removed sales with prices below 100 dollars or above 10 million dollars. We assume that these 195 outlier sales are not governed by the same processes as the rest of the dataset.

These operations yielded the base dataset, providing explicit habitat status, construction permit timing, and development status of each parcel. We proceeded to supplement this dataset with covariates selected from other GIS datasets to represent factors that could impact the rental rate and habitat value of the parcel.

The DOT GIS dataset included information from other county agencies, such as Planning and Zoning, and the County Assessors Office which provided ownership, school districts, incorporation status, and MLS market area. Dummy variables were set for the regions (E, N, NE, NW, PE, PFW, PNW, PS, PSW, S, SE, SW, W, XNE, XSW, XW, and NO MARKET for land falling outside of the market areas). The central (downtown Tucson) region was not included to prevent over-specification.

Fields indicating census tract, blockgroup, and block were included in the parcel dataset. We linked parcels to short and long form 1990 census data using the block group of the parcel. Census fields we used to control for rental rates included population, population per square mile, number of households, population 65 and over, single father households, single mother households, number of housing units, number of vacant units, number of owner occupied homes, median home value, median residential rent, workforce, unemployed, average minutes of commute time to work, and average salary.

The county GIS dataset also included the locations of parks, golf courses, prisons, modern cultural features, and highways. For computational feasibility, we grouped parcels by census block and then calculated distances to the nearest park, golf course, prison, modern cultural feature, and highway. The distance to downtown (Tucson City Hall) was also calculated. Units were in international feet, due to the projection of the parcel dataset.

Owner's names, addresses, and zip codes were provided with the parcel dataset. For parcels with a valid zip code for the owner's mailing address, we set dummy variables to indicate if the owner lived within Pima county and within Arizona. We also calculated distances (in International Feet) from Tucson City hall to the centroid of the owner's zip code.

We linked two spatial datasets representing environmental and topological characteristics to the parcel database. Because not all of these data were available for the entire county, parcels in the western side of the county were dropped from the dataset. Since the western side of the county is very sparsely populated, this operation reduced the number of parcels only slightly. It reduced the total number of parcels to 338,684 and the number of initially undeveloped parcels to 109,819.

We associated the data to the parcels by identifying the polygon or grid cell that the centroid of the parcel was within. All coverages were projected to the coordinate system of the parcels and all calculations were done in that coordinate system.

We calculated topology using a grid of elevation data developed by the USGS EROS Data Center and obtained from the University of Arizona ARIA image server. In the state-plane projection, each grid cell was a 98.446 foot square (resolution was 30.0 meters in the original projection). Elevation was reported in meters. We used the elevation dataset to calculate slope (in degrees) and aspect (in compass degrees).

Additional environmental variables were obtained from the USDA STATSGO soil GIS coverage. In this coverage, map units were delineated in irregular polygons according to soil type and environmental characteristics. We used several STATSGO fields. The surface texture (SURFTEX) field was included, as was the percentage of plant cover (PLANTPCT). We also utilized soil water permeability categories (HYDGRP), which ranged from A (fastest draining) to D (slowest draining) and the USDA estimates of qualitative habitat quality (WLGRAIN, GRASS, HERB, HARD, CONIF, SHRUB, WETPLT, SHLWAT, OPEN, WOOD, WET, RANGE).

Because parcel level data was supplemented with GIS and census datasets using census boundaries, grid cells, or map polygons, a single spatial data observation from a given dataset is shared across all of the parcels within the geographic extent of the observation.

Upon completion of the database integration, we exported the GIS database into tabular form, with variables indicating habitat status, initial development status, timing of construction permits, local characteristics that could impact parcel rents, and local characteristics linked to habitat value.

Appendix 3: Is GBM a reasonable assumption?

The quarterly housing price index taken from the Office of Federal Housing Enterprise Oversight for Pima County provides evidence of a GBM process at work. Consider the following analysis. If the GBM assumption is approximately correct, then we should find an upward curvature in the graph of average home prices or rents against time. If it holds exactly, then regression of the change in price from one period to the next on the level of the same price will yield the same results with and without a constant term (and with and without power terms for the level), and will exhibit multiplicative heteroskedasticity. Finally, the distribution of percentage change in price divided by price.

The price index since 1986 is shown in Figure 3a, together with a purely geometric (i.e., non-stochastic) growth curve with the growth rate estimated from the post-1986 data, located to intersect the actual data at the mid-year. As would be the case with geometric Brownian growth, departures from this path persist for a few years, but are eventually countered by random events in the opposite direction. Data from earlier periods displays similar growth curvature, but more noise. Inset into Figure 3a is the density of percentage change in price smoothed with an Epanechnikov kernel. As with a normal distribution, the preponderance of the density is characterized as a symmetrical, unimodal function. The tails are too long for a normal distribution, however.

Regression of the change in price on price level since 1986 excluding a constant term yields an estimate for *g* of 0.011 ± 0.0009 (± denotes White's robust standard error). This is the growth rate used for the comparison curve. Adding a constant term doubles the growth rate estimate to 0.022, and the constant term is significant (-1.18 ± 0.47). Using the data from 1975-present yields about the same growth rate with no constant term (0.013 ± 0.002), but with a constant term the estimate of *g* falls to 0.003 ± 0.009. The addition of a squared price term leaves the coefficient on price roughly equivalent, but the coefficient is estimated much less precisely (.0113 ± .0131).

Figure 3a: Evidence on fit of GBM model to greater Tucson housing values