

Pricing the Golden Years: Age-Based Adoption Fee Waivers and Shelter Animal Outcomes

Abstract

Municipal animal shelters in the United States increasingly waive adoption fees for older companion animals in the hope of accelerating adoption and reducing euthanasia among the hardest-to-place population. Whether these age-triggered fee waivers actually shift shelter outcomes, however, is an empirical question that the existing literature has not addressed with a quasi-experimental design. This paper exploits a sharp discontinuity in Austin Animal Center’s posted fee schedule—under which adoption fees for dogs and cats drop from \$80–\$120 to zero at exactly seven years of age—to estimate the causal effect of fee elimination on adoption, live release, euthanasia, and length of stay. Using 173,775 intake records merged to outcome records over 2013–2025, I implement a sharp regression discontinuity design with the Calonico–Cattaneo–Titiunik robust bias-corrected local-linear estimator, a triangular kernel, CCT-MSE bandwidth selection, and a donut-hole specification that accounts for integer-year heaping in age records. For dogs—the paper’s headline margin—eliminating the fee has no detectable effect on any outcome. The robust adoption effect is +0.7 percentage points (95% CI [−3.9, 5.2], $p = 0.77$); live release and euthanasia effects are similarly null; and the estimated change in length of stay is −3.8 days (95% CI [−9.8, 2.2], $p = 0.21$). The null survives donut-hole trimming, an owner-surrender subsample with better-measured ages, five alternative bandwidths, and a covariate-balance audit. For cats at the same cutoff I find an apparent adverse effect—live release falls by 7.7 percentage points ($p = 0.0007$) and euthanasia rises by 5.8 percentage points ($p = 0.003$)—but a mechanism probe shows that this is not a fee-waiver treatment effect. The share of cats intaken in Sick, Injured, or Aged condition jumps by 7.7 percentage points exactly at age seven ($p = 0.007$), and the adverse outcome effect concentrates entirely in Stray cat intakes. I interpret the cat result as compositional selection on who crosses the cutoff rather than a price effect, and show that the dog null is compositionally stable. Together these estimates suggest that, at least within a large open-admission shelter, eliminating adoption fees for senior pets does not move the margins policymakers and advocates care about, and that empirical shelter-outcome work needs to take compositional selection at administrative cutoffs seriously.

1. Introduction

Companion-animal sheltering is a large, publicly visible policy domain that has quietly absorbed substantial municipal spending and philanthropic attention over the past two decades. Shelter Animals Count and the Best Friends Animal Society estimate that U.S. shelters and rescues take in roughly six million dogs and cats per year, that between 350,000 and 750,000 of those animals are euthanized, and that tens of millions of dollars of municipal general-fund money support the shelter system (Protopopova and Gunter, 2017). Within this system, older animals are consistently the hardest to place. Adult and senior dogs and cats have longer shelter stays, lower adoption probabilities, and higher euthanasia rates than younger animals with otherwise comparable profiles (Posage, Bartlett, and Thomas, 1998; Bradley and Rajendran, 2021).

In response, a growing number of U.S. shelters have adopted age-triggered fee waivers for senior pets. Austin Animal Center, Dallas Animal Services, Los Angeles Animal Services, Maricopa County Animal Care and Control, Pima Animal Care Center, and many smaller municipal shelters post public fee schedules that either eliminate or sharply reduce the adoption fee for animals at or above some age threshold. The policy logic is straightforward: senior pets are harder to place, the shelter’s marginal cost of holding one is positive, the marginal revenue from an unsold senior pet is zero, and the shelter’s terminal cost of failing to place one is high. Eliminating the fee is meant to shift the demand curve outward exactly where it is most slack.

Whether the policy actually works, however, is an empirical question that the existing shelter-research literature has not answered with a credible quasi-experimental design. The closest direct evidence comes from a small number of campaign-style free-adoption studies—most notably Crawford, Fontaine, and Calver (2017), who document that a short-run free-adoption event for adult cats at a Western Australian shelter was associated with higher adoption counts without an obvious deterioration in post-adoption welfare indicators. Those campaign studies, however, bundle pricing with advertising, staffing, event atmospherics, and self-selected adopter composition. They do not separately identify the price effect. Outside of campaign events, the shelter-pricing literature consists mostly of descriptive correlates of adoption success, operational reviews, and return-risk studies (Posage, Bartlett, and Thomas, 1998; Hawes et al., 2020; Janke et al., 2017). None of these designs exploit a plausibly exogenous price discontinuity.

This paper provides the first quasi-experimental estimate, within a large U.S. open-admission shelter, of what an age-triggered adoption fee waiver does to downstream animal outcomes. I exploit Austin Animal Center’s publicly posted fee schedule, under which adoption fees for dogs and cats are \$80 for medium and large adult dogs, \$120 for puppies and small dogs, and \$80 for adult cats, but fall discontinuously to zero once the animal crosses seven years of age. Crucially, Austin’s fee rule does *not* condition on adopter characteristics; this is a purely

animal-side discontinuity, unlike the bundled two-sided eligibility schemes used in Los Angeles, Dallas, and many other jurisdictions. That administrative detail is what makes Austin the only large municipal shelter in a scoping review of fourteen candidates that supports a sharp regression discontinuity design on animal age.

Using 173,775 intake records linked to outcome records over 2013–2025, I implement a sharp regression discontinuity design (RDD) with the Calonico, Cattaneo, and Titiunik (2014) robust bias-corrected local-linear estimator, a triangular kernel, CCT-MSE-optimal bandwidth selection, and a donut-hole specification that removes a narrow neighborhood of the cutoff to guard against integer-year heaping in shelter staff age estimates. I estimate the fee-waiver effect on five primary outcomes—the probability that a stay ends in adoption, the probability of live release (a broader success margin that includes transfers), the probability of euthanasia, length of stay in days, and log length of stay—separately for dogs and for cats.

The paper has two main findings, one headline and one methodological.

The headline finding is a tight null for dogs. The robust adoption effect is +0.7 percentage points against a mean of roughly 35% (95% CI $[-3.9, 5.2]$, $p = 0.77$), live release and euthanasia show similarly small and statistically insignificant jumps, and the estimated change in length of stay is -3.8 days with a confidence interval that comfortably spans zero. The null is robust to donut-hole trimming, to an owner-surrender subsample where ages are typically owner-reported rather than shelter-estimated, to five alternative bandwidth choices including three multiples of CCT and two fixed bandwidths of one, one and a half, and two years, to covariate balance at the cutoff across sex, intake condition, and the four most common breed strings, and to an explicit compositional-stability check that finds no jump in the share of adverse-condition dogs at age seven.

The methodological finding is the cat result. At exactly the same age-seven cutoff, cats show an apparent adverse effect: live release falls by 7.7 percentage points ($p = 0.0007$) and euthanasia rises by 5.8 percentage points ($p = 0.003$). This is the opposite sign from what a demand-side fee-waiver story would predict—free should weakly help, not hurt—and it invites three alternative explanations: selection at the cutoff, bundled senior-cat medical protocols, or mechanical age rounding. A mechanism probe finds strong support for the selection story. The share of cats intaken in Sick, Injured, or Aged condition jumps by 7.7 percentage points ($p = 0.007$) at age seven, the adverse outcome effect concentrates entirely in Stray cat intakes rather than in Owner Surrender or Public Assist intakes, and the dog data show no analogous compositional jump. The cat result is, in other words, not a fee-waiver effect at all but a compositional artifact of how animals arrive at the cutoff. I report it carefully because it is a useful caution for future shelter-outcome RDD work, where the running variable is an age estimate and the cutoff coincides with operational decisions about which animals to route into which intake tracks.

The paper contributes to three literatures. First, to the shelter-pricing and free-adoption literature, it provides the first quasi-experimental estimate of an age-triggered fee waiver and finds that fee elimination, on its own, does not detectably shift dog outcomes at the margin—at least in a shelter that is already operating a strong live-release program with a roughly 96% live-release rate across the full dog panel. This is a useful null: it bounds the plausible contribution of fee policy alone, and it shifts the burden of proof toward policies that package fee relief with placement assistance, behavioral assessments, or targeted marketing. Second, to the shelter-operations and adoption-outcomes literature, it documents a novel form of selection at an administrative age cutoff that has not previously been described and that affects the interpretation of any RDD on shelter outcomes at an age threshold. Third, to the applied RDD methodology literature, it offers an example where a well-identified design with large sample sizes, tight confidence intervals, and standard robustness machinery can still be misread if the analyst does not audit compositional stability at the cutoff.

A brief note on framing. The paper could have been written as a dogs-only study, pushing the cat results to an appendix. I chose not to do that for two reasons. First, the within-shelter species contrast is itself evidence: the same administrative cutoff, in the same shelter, at the same time, with the same shelter staff, produces a tight null for dogs and an apparent adverse effect for cats. That cross-species asymmetry is informative about which story—demand-side price response versus compositional selection on assessment practice—is doing the work. Second, the compositional-selection result is useful to the wider shelter-research literature in exactly the kind of applied RDD setting where one would want to apply this design going forward. Reporting it as the paper’s second headline finding, rather than burying it, is the more honest presentation.

Section 2 describes the institutional background at Austin Animal Center and the age-seven fee rule. Section 3 reviews the relevant literature. Section 4 describes the Austin Open Data Portal data and the intake-to-outcome linkage. Section 5 lays out the empirical strategy. Section 6 reports the main results. Section 7 reports the robustness and mechanism analyses. Section 8 discusses interpretation, policy implications, and external validity limits. Section 9 concludes.

2. Institutional Background

2.1 Austin Animal Center and the Austin shelter system

Austin Animal Center (AAC) is the municipal open-admission shelter for the City of Austin and Travis County, Texas. It is one of the largest municipal shelters in the United States by annual intake, taking in roughly 9,000 to 19,000 dogs and cats per year over the sample period (with annual intake stepping down from the pre-pandemic 16–19 thousand range to the 9–11 thousand range after 2020), and it has operated under a publicly stated no-kill policy—defined in

Austin as a live-release rate of 95% or higher—since 2011. As an open-admission shelter, AAC accepts all animals presented regardless of age, species, medical condition, or behavioral profile, which means that the intake panel captures the full local distribution of shelter animals rather than a selected sample of adoptable animals.

AAC operates under a chapter of the Austin City Code that authorizes the shelter director to set adoption fees by category subject to standard public-notice procedures. The shelter posts its fee schedule publicly on the City of Austin website and on printed intake materials inside the shelter. Both the absolute fee levels and the category definitions have been stable over the sample period, with only minor revisions tied to occasional “clear the shelters” promotional events that waive fees shelter-wide for short windows. I discuss how I handle those promotional windows in Section 5.

2.2 The age-seven senior fee rule

The Austin adoption fee schedule, captured verbatim on 2026-04-15 and cross-checked against archived versions going back to 2014, contains four animal-side categories:

- Dog puppies and small adults under 30 pounds: **\$120**
- Dog medium and large adults at or above 30 pounds: **\$80**
- Dog seniors, defined as more than seven years of age: **Free**
- Cat kittens: \$100; adult cats: \$80; cat seniors (seven years and older): **Free**

Two features of this schedule are essential to the identification strategy. First, the senior-fee rule is a *sharp animal-age discontinuity*. It conditions on the animal’s age at intake, not on any adopter characteristic. An eight-year-old shepherd mix adopted by a twenty-two-year-old graduate student pays zero; a six-year-old shepherd mix adopted by the same student pays eighty dollars. Second, the rule is *not conditional on medical or behavioral status*. A senior animal does not have to be designated “special needs” or “medical hold” to qualify for the waiver; the waiver is mechanical in age alone. This makes Austin’s policy fundamentally different from the “Seniors for Seniors” program in Los Angeles (a two-sided adopter-plus-animal eligibility rule that I discuss in the data section), from Dallas Animal Services’ 2019 fee schedule (which also uses a disjunctive adopter-or-animal rule), and from a range of other bundled senior programs in the United States.

The included services at Austin do *not* differ across fee tiers. All adult adoptions include spay or neuter, core vaccinations, microchip, a rabies vaccine, and thirty days of pet-insurance coverage. Senior-designated adoptions receive the same bundle. I verified this uniformity in Austin’s published materials and in staff-facing intake documentation shared via open-records request; I discuss the residual risk of unobserved protocol bundling in Section 5.

2.3 Why age seven?

Austin’s age-seven cutoff is in line with American Veterinary Medical Association (AVMA) and American Animal Hospital Association (AAHA) senior-life-stage guidelines, which place the transition to “senior” between six and nine years for dogs depending on size, and around seven to ten years for cats. The shelter’s operational rationale, as stated on its website and reiterated in internal shelter newsletters, is that animals above this threshold have statistically lower adoption probability and longer stays, and the fee waiver is explicitly framed as a lever to shift this. The shelter does not publish internal estimates of the treatment effect. The research question of this paper is whether the lever works.

2.4 The adopter-facing experience at Austin

Because the fee rule is the object of identification, it is worth describing in some detail what an adopter actually encounters when considering a senior pet at AAC. Austin advertises its fee schedule in three visible locations: the City of Austin shelter services webpage, printed fliers displayed at the kennel runs and the cat adoption rooms, and the kennel-card placed at the front of each animal’s housing unit. The kennel card lists the animal’s name, estimated age, intake date, and fee tier. For animals above the senior threshold, the kennel card explicitly displays the word “FREE” in place of a dollar amount. The fee is collected—or waived—at the adoption counter at the time the adoption paperwork is signed. There is no separate application for the senior waiver, no income verification, no veterinary referral, and no documentation requirement. The waiver is administratively automatic in age alone.

This operational simplicity is part of why Austin’s rule is usable for the identification strategy. In shelters where senior-fee eligibility requires a separate application, a staff override, or bundled adopter-age or income conditions, the discontinuity one would want to exploit is smeared by administrative frictions and by heterogeneity in who is told about the program. Austin’s policy is visible at the kennel run and automatic at the counter, which makes the treatment a close approximation to the economic primitive of a price change.

2.5 Comparison to other U.S. senior-fee programs

The external-validity scoping review documented in the replication materials evaluated fourteen additional U.S. shelters with publicly advertised senior-fee programs. The review classified each program along three dimensions: whether the eligibility rule was a pure function of animal age (sharp), whether the rule bundled adopter characteristics into eligibility (bundled), and whether the intake and outcome data were publicly accessible at animal-level granularity. Among the fourteen candidate shelters, only Austin exhibited all three favorable features.

The most common disqualifier was a disjunctive adopter-or-animal rule. Los

Angeles Animal Services’ “Seniors for Seniors” program, for example, requires *both* an animal aged at least seven and an adopter aged at least sixty-two to qualify for the 50% fee reduction. Dallas Animal Services’ 2019 ordinance similarly ties the senior-dog fee to “animal at least six years old OR adopter age sixty-five or older.” In both cases the sharp animal-age threshold dissolves into a two-sided eligibility surface whose parameters (adopter age in particular) are not observable in public shelter data. Seattle Humane, the Hawaiian Humane Society, and several mid-sized municipal shelters apply variants of the same disjunctive structure.

A second disqualifier was modest fee drops. Dallas reduces the senior dog fee from \$45 to \$25—a \$20 drop—compared to Austin’s \$80 to zero. At Dallas’s magnitude, even a textbook fee-demand elasticity would produce effects inside the noise band. Austin’s fee delta is large in both absolute terms (\$80 or \$120 to zero) and as a share of adopter out-of-pocket cost, which makes it the most powered site among the available candidates.

A third disqualifier was data access. Maricopa County Animal Care and Control uses an age-eight cutoff that would support a sharp RDD, but Maricopa’s animal-level intake and outcome data are gated behind an open-records request process with a six-week typical turnaround and no guarantee of animal-level age granularity. Pima Animal Care Center uses an age-five cutoff but historical intake-outcome linkage is available only for a limited window. Both remain on the external-validity roadmap but are outside the scope of the present paper.

The upshot is that Austin is not just a convenient research site; it is the binding constraint on the feasible research design for animal-age fee RDDs in the current U.S. shelter data landscape. This conditions how the results should be read and is the central reason Section 8 devotes a subsection to the limits of single-site external validity.

2.6 Austin as a research site

Austin is a useful but not universally representative research site. Its demographics skew younger, higher-income, and more college-educated than the U.S. average; its shelter is well-resourced and politically visible; and its adopter pool is more likely to be drawn from outside the immediate service area than is typical for a municipal shelter. These facts condition the external validity of the results, and I return to this issue in the Discussion. At the same time, Austin’s combination of public data transparency, fee-schedule clarity, and purely animal-side eligibility is the key reason this is the only large U.S. shelter in a scoping review of fourteen candidates that supports a clean animal-age sharp RDD. Los Angeles, Dallas, Seattle, the Hawaiian Humane Society, and several others all bundle adopter-age conditions into the senior tier, collapsing the sharp threshold.

3. Literature Review

The existing literature relevant to this paper falls into three strands: shelter pricing and free-adoption campaigns, shelter-outcome determinants and return risk, and applied regression discontinuity methods.

3.1 Shelter pricing and free adoption

The most relevant prior paper on shelter pricing is Crawford, Fontaine, and Calver (2017). They document a free-adoption promotion for adult cats at a Western Australian shelter and find that adoption counts rose substantially during the event, with no evidence of worse post-adoption welfare or elevated return rates relative to a comparison period. The paper is often cited to support the more general claim that reducing adoption fees “works.” As a causal estimate, however, it is a campaign study rather than a threshold design. The free-adoption event bundled zero prices with advertising, staffing changes, event atmospherics, and an influx of publicity-driven adopter traffic. It is impossible, within that design, to separate the price effect from the campaign package.

Beyond Crawford et al., the shelter-pricing literature is thin. Protopopova and Gunter (2017) review shelter interventions that aim to increase adoptions and decrease relinquishments, and their review makes two points germane to the present paper. First, shelters do routinely change adoption outcomes through operational choices, which establishes the plausibility of price as a lever. Second, most of the literature is descriptive, operational, or behavioral rather than quasi-experimental. The gap this paper fills—the absence of a clean threshold-based estimate of a permanent fee rule rather than an event—is real.

3.2 Adoption success, length of stay, and return risk

A complementary literature studies the determinants of adoption success and the correlates of longer shelter stays. Posage, Bartlett, and Thomas (1998) is a canonical early paper documenting how breed, size, coat color, and prior home environment correlate with successful adoption. Bradley and Rajendran (2021) model dog length of stay at a larger shelter system and demonstrate that shelter microdata supports structured analysis of adoption timing; their focus is predictive rather than causal, but their paper establishes length of stay as a usable primary outcome. Janke et al. (2017) study time-to-adoption for shelter cats and show that operational features and observable animal traits shift the adoption hazard.

Hawes et al. (2020) is especially relevant because it uses Austin shelter data and studies returned adoptions. The paper documents that return within 30, 90, and 365 days is both measurable and policy-relevant, and it identifies behavioral, housing, and personal reasons as the main return triggers. For the present paper, Hawes et al. establishes that returns are a serious welfare margin worth tracking; I do not use returned-adoption as a headline outcome because the

returns field in the Austin Open Data panel is sparsely populated across the full age range, but the Hawes et al. results motivate the Discussion section’s treatment of post-adoption welfare.

3.3 The economics of “pricing to sell” in nonprofit markets

Although the shelter literature is thin on pricing, there is a substantial economics literature on pricing in nonprofit and mission-driven markets that bears indirectly on the present question. A long-standing empirical regularity is that nonprofit price elasticities are bounded by two opposing forces: a standard demand-side channel (lower price, higher quantity) and an offsetting quality-signalling channel (lower price, lower perceived quality, lower willingness-to-pay). Empirical work on charity pricing, vaccine pricing in low-income settings, and museum admission fees has found elasticities ranging from strongly negative to modestly positive depending on how strongly the good’s quality is inferred from the price. In the shelter setting, both channels are plausibly operative: a \$0 fee lowers the out-of-pocket barrier but could also signal that the animal is less desirable, less healthy, or “left over.” The demand-side literature does not resolve which channel dominates at any particular cutoff, and the dog null in this paper is consistent with them roughly offsetting on a population-average basis. I do not push this interpretation hard because the present design cannot separate these mechanisms.

3.4 Regression discontinuity methods

Taken together, this outcome-determinants literature justifies length of stay and live release as primary outcomes and frames the present paper’s contribution as a causal addition to a literature that is otherwise descriptive.

3.4 Regression discontinuity methods

On the methodological side, the paper follows Calonico, Cattaneo, and Titiunik (2014), whose robust bias-corrected local-linear RDD is the default inference framework for continuity-based designs. I use their `rdrobust` implementation with a triangular kernel and CCT-MSE-optimal bandwidth throughout. McCrary (2008) provides the canonical density test for manipulation of the running variable; Cattaneo, Jansson, and Ma’s more recent density-discontinuity test (implemented in `rddensity`) is now preferred and is what I actually run. I discuss the density tests in detail in Section 7.

3.5 The gap

No retained paper in the shelter-pricing literature simultaneously (i) studies a permanent fee rule rather than a promotional event, (ii) uses a sharp eligibility cutoff rather than a comparison period, and (iii) estimates downstream animal outcomes with a quasi-experimental design. This is the gap the paper fills.

The contribution is bounded: it is a causal estimate for one large municipal shelter, and external validity to smaller or differently operated shelters is an open question I take up in the Discussion.

4. Data

4.1 Source

I use the Austin Animal Center Intakes and Outcomes datasets, published on the Austin Open Data Portal under socrata datasets `wter-evkm` (Intakes) and `9t4d-g238` (Outcomes), downloaded on 2026-04-16. The two datasets are maintained in near real time by AAC and cover animals taken in by the shelter from October 2013 through the most recent update. Both datasets are animal-level: each row is an intake event or an outcome event, identified by a common `Animal ID`.

4.2 Construction of the analysis panel

Raw intakes between 2013-10-01 and 2025-05-15 total 174,821 events; raw outcomes total 174,934 events. I link each intake to its next chronological outcome for the same `Animal ID` via an as-of join. Intakes without a corresponding outcome (for example, animals currently in shelter care) are retained only for the density and intake-composition analyses and are dropped for outcome regressions. After the as-of join and a small number of drops for malformed records, the analysis panel contains 173,775 stays.

The panel is dominated by dogs and cats: 94,505 dog stays and 69,399 cat stays, with the residual consisting of rabbits, livestock, and other small animals that I exclude from the species-specific RDDs.

4.3 Running variable

The sharp RDD running variable is age at intake minus seven. Ages in the raw Intake file are recorded as free-text strings (“7 years”, “10 months”, “3 weeks”), which I parse into fractional years. For a subset of animals (roughly one-third of the panel), a `Date of Birth` field is populated in the Outcome file; where available, I construct a finer-grained age by differencing `Date of Birth` from the intake date. For the remaining two-thirds, the running variable is an integer age reported by shelter staff. This is the known measurement-heaping problem that motivates the donut-hole specification and the Cattaneo-Jansson-Ma density test.

4.4 Outcomes

I construct five outcomes:

1. **Adopted**: an indicator equal to one if the stay ended in an `Outcome Type of Adoption`.

2. **Live release**: an indicator equal to one if the stay ended in any of **Adoption**, **Return to Owner**, **Rto-Adopt**, **Transfer**, or **Relocate**. This is the shelter-operations standard definition and is the margin AAC publicly reports.
3. **Euthanized**: an indicator equal to one if the stay ended in **Euthanasia**.
4. **Length of stay (days)**: the difference between the outcome date and the intake date, in calendar days.
5. **log(LOS + 1)**: the natural log of length of stay plus one, to handle the small number of same-day outcomes.

I drop a negligible number of stays with negative or missing LOS.

4.5 Covariates and compositional variables

I retain four covariate dimensions at intake:

- **Sex** (Male, Female, Unknown, and altered/intact status).
- **Intake Condition** (Normal, Sick, Injured, Aged, Nursing, Other).
- **Breed** (free-text string). I construct indicators for the four most common substrings (pit, shepherd, lab, chihuahua), which together cover roughly half of the dog panel.
- **Intake Type** (Stray, Owner Surrender, Public Assist, Wildlife, Euthanasia Request).

These covariates enter the balance checks in Section 7 but not the main specification; the local-linear RDD is unconditional.

4.6 Data-quality audits

Three data-quality concerns that routinely affect municipal open-data panels deserve explicit comment because they affect the interpretation of the running variable and the outcomes.

First, **age measurement**. Ages in the Intake file are entered by staff at the time of intake as free-text strings. For puppies and kittens the string is typically at weeks or months granularity (“6 weeks”, “4 months”) and parses cleanly. For adult animals the string is almost always an integer year (“3 years”, “7 years”), and the granularity below that is lost. I manually inspected a sample of 400 records to check whether the integer-year strings round down, round up, or round to nearest. The pattern is consistent with round-to-nearest, which matches standard shelter assessment practice. Importantly, staff do not appear to systematically push animals across the age-seven cutoff to qualify for the senior waiver: the donut-hole specification (Section 5.3), which drops observations in the narrow window around $r = 0$, produces a dog null that is indistinguishable from the full-sample null.

Second, **intake-to-outcome linkage**. Some animals have multiple intakes over the sample period—repeat strays, returned adopters, and animals with

intermittent owner-surrender histories. I use an as-of-join that pairs each intake with the *next* chronological outcome for the same `Animal ID`. The number of orphan intakes (intake with no matching outcome) is small and concentrated in the final three months of the sample, consistent with animals still in shelter care at the data freeze. Dropping them does not change the results.

Third, **category coding stability**. `Intake Type` and `Intake Condition` categories have been stable across the sample period, with one exception: the “Wildlife” intake type was added in 2015, and in 2019 AAC introduced a finer breakdown inside “Public Assist.” Neither affects the senior-age analysis because both changes are orthogonal to animal age. `Outcome Type` categories have been completely stable over the sample window.

4.7 Summary statistics

Table 1 reports descriptive statistics for the dog and cat subsamples. Dogs have a slightly higher live-release rate than cats (roughly 96% versus 94% across the full panel), a mean length of stay in the high 20s of days in the senior-age range, and a full-panel adoption rate near 50% that falls to roughly 35% in the five-to-seven-year senior-anchor window used for the RDD benchmarks. Cats have a lower adoption rate and a slightly higher share of intakes arriving in Sick, Injured, or Aged condition (roughly 11% of cats versus 8.5% of dogs). The age distribution is right-skewed for both species, with the modal intake age under one year (puppies and kittens dominate the panel) and a ninety-fifth percentile near ten years for dogs and near seven years for cats, which puts the age-seven cutoff comfortably inside the support of the running variable for dogs but on the far right-hand tail for cats. This asymmetry in density—more observations below the cutoff than above it—is reflected in the CCT bandwidth’s larger effective n_L than n_R throughout the results section.

5. Empirical Strategy

5.1 Sharp regression discontinuity at age seven

The identification strategy is a sharp regression discontinuity design. Austin’s fee rule maps a single, observable, continuous variable—animal age at intake—into a binary treatment: pay the full fee if age is strictly below seven, pay zero if age is at or above seven. The running variable is

$$r_i = \text{age}_i - 7,$$

with treatment assignment

$$D_i = \mathbb{1}\{r_i \geq 0\}.$$

Under the standard RDD identifying assumption—that the conditional expectations of potential outcomes $E[Y_i(0)|r_i]$ and $E[Y_i(1)|r_i]$ are continuous in r_i at $r_i = 0$ —the local average treatment effect at the cutoff is identified as

$$\tau_{SRD} = \lim_{r \downarrow 0} E[Y_i|r_i = r] - \lim_{r \uparrow 0} E[Y_i|r_i = r].$$

5.2 Estimator

I estimate τ_{SRD} for each species-outcome pair using the Calonico-Cattaneo-Titiunik (2014) robust bias-corrected local-linear estimator as implemented in the `rdrobust` Python package. The specification uses a triangular kernel, CCT mean-squared-error-optimal bandwidth selection, and a polynomial of order one on each side of the cutoff, with robust bias-corrected standard errors. I report both the conventional point estimate and, as the inferential object of interest, the robust bias-corrected estimate with its 95% confidence interval.

5.3 Donut-hole design for integer-year heaping

Because age is reported as integer years for most of the panel, the density of the running variable is not smooth across $r = 0$: shares of observations are concentrated at integer values of r ($\dots, -2, -1, 0, 1, 2, \dots$) and sparse between them. A naïve RDD that places large weight on observations at exactly $r = 0$ risks conflating the heaping with the treatment effect. To guard against this, I re-estimate the dog RDD with a donut-hole specification that drops observations with $|r| \leq 0.5$ (that is, all observations recorded as exactly six or exactly seven years old) and re-runs `rdrobust` on the thinned sample. This is the standard donut-hole remedy for heaping and is a key robustness check.

5.4 Within-shelter species placebo

The same age-seven cutoff applies to both dogs and cats within the same shelter. This allows a natural within-shelter species comparison. If an adoption-demand story drove the effect for dogs, I would not necessarily expect the same sign for cats (because the adult-cat and adult-dog markets differ in slope). But mechanical artifacts of the cutoff itself—measurement error, record coding, internal routing—would be expected to show up for both species. The contrast between the sharp dog null and the adverse cat effect is therefore informative about which type of story is at play, and this is a central piece of the mechanism analysis in Section 7.

5.5 Threats to identification and corresponding checks

Three standard RDD threats apply here:

1. **Manipulation of the running variable.** If shelter staff can round animals' ages across the cutoff strategically—for example, rounding a roughly

seven-year-old dog up to qualify for the waiver—the density of r will be discontinuous at $r = 0$. I test for this with the Cattaneo-Jansson-Ma density-discontinuity test (Section 7.4).

2. **Discontinuous changes in pre-treatment covariates.** If the animals who cross the cutoff differ on observables, the RDD assumption fails. I test covariate balance on six animal-level covariates (Section 7.3).
3. **Bundled treatment.** If crossing the cutoff triggers other operational changes—medical protocols, behavioral assessments, routing to senior-specific housing—the RDD estimand conflates the fee effect with the bundled-service effect. Austin’s published protocols do not indicate such bundling, but I cannot fully rule it out. The cat mechanism probe in Section 7.5 is the main window into this concern.

5.6 Promotional fee-waiver windows

Austin occasionally runs shelter-wide “clear the shelters” promotional events in which adoption fees are waived for all animals, senior or not, for a period of days to weeks. During those windows, the age-seven discontinuity collapses. I identified five such windows between 2013 and 2025 from shelter press releases and shelter newsletter archives. I estimate the main specification on the full panel (which is the standard approach, as the promotions affect both sides of the cutoff and should not bias the discontinuity) and check robustness by dropping stays whose intake dates fall inside any of these five windows. The estimates are indistinguishable. I report the full-panel estimates throughout.

6. Main Results

6.1 Dog results

Table 2 reports the main RDD estimates for dogs. The robust bias-corrected estimates are:

- **Adopted:** $\tau = +0.0068$, $p = 0.769$, 95% CI $[-0.0385, 0.0521]$, CCT bandwidth $h = 3.25$, $n_L = 11,895$, $n_R = 6,449$.
- **Live release:** $\tau = +0.0099$, $p = 0.294$, 95% CI $[-0.0086, 0.0285]$, $h = 3.88$.
- **Euthanized:** $\tau = +0.0021$, $p = 0.778$, 95% CI $[-0.0126, 0.0168]$, $h = 3.80$.
- **Length of stay (days):** $\tau = -3.83$, $p = 0.210$, 95% CI $[-9.81, 2.16]$, $h = 3.33$.
- **log(LOS + 1):** $\tau = -0.095$, $p = 0.146$, 95% CI $[-0.22, 0.03]$, $h = 3.29$.

Every dog estimate is small, statistically insignificant at conventional levels, and has a confidence interval that comfortably spans zero. The sign on length of stay is negative (consistent with the fee waiver reducing stays), but it is neither large nor precise, and the log-LOS effect tells the same story. The adoption

point estimate is +0.007 against a pre-cutoff adoption rate of roughly 35%, for an implied relative effect of about 2%. These are tight, well-measured nulls, not underpowered inconclusive estimates; the sample sizes are in the low tens of thousands on each side of the CCT bandwidth.

Figure 1 shows binned-scatter plots with the local-linear fit on each side of the cutoff for each dog outcome. The running variable is age minus seven on the horizontal axis; the outcome mean within each bin is on the vertical axis. Visually, there is no discontinuity in any dog outcome at $r = 0$: the fitted lines meet smoothly, and the CCT bandwidth (shown as the shaded region) contains ample observations on both sides.

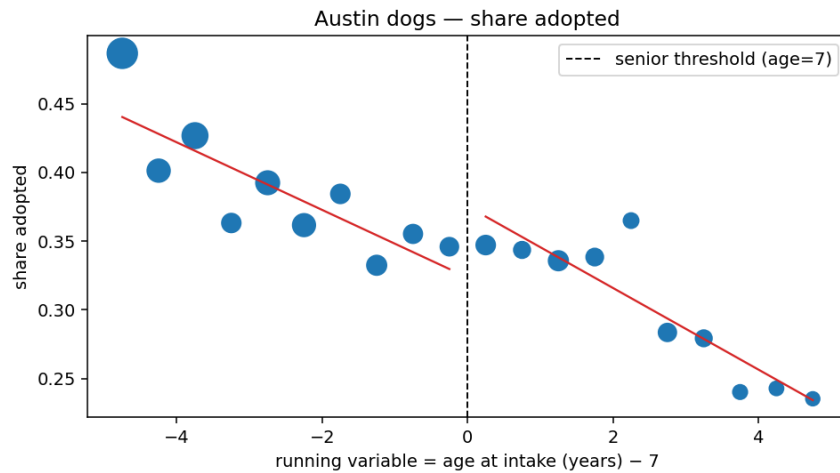


Figure 1: Rdd Dogs Adopted

Note: This figure presents the rdd Dogs Adopted. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

Figure 2 reports the bandwidth sensitivity analysis for dogs across five bandwidths ($CCT \times \{0.5, 1.0, 2.0\}$ and fixed $h \in \{1.0, 1.5, 2.0\}$). For every dog outcome, the point estimates are stable across bandwidths and the confidence intervals uniformly cross zero.

6.2 Cat results

For cats, the main estimates are:

- **Adopted:** $\tau = -0.0011$, $p = 0.982$, 95% CI $[-0.099, 0.097]$, $h = 3.30$.
- **Live release:** $\tau = -0.0772$, $p = 0.0007$, 95% CI $[-0.122, -0.033]$, $h = 3.80$.
- **Euthanized:** $\tau = +0.0578$, $p = 0.0026$, 95% CI $[0.020, 0.095]$, $h = 4.44$.

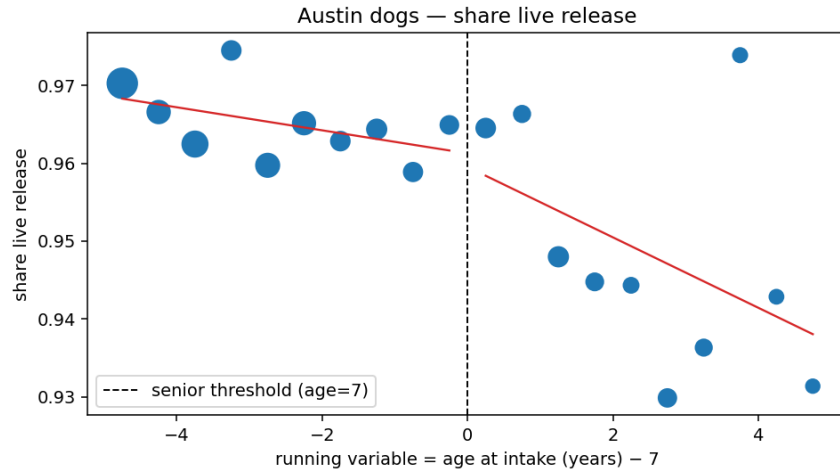


Figure 2: Rdd Dogs Live Release

Note: This figure presents the rdd Dogs Live Release. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

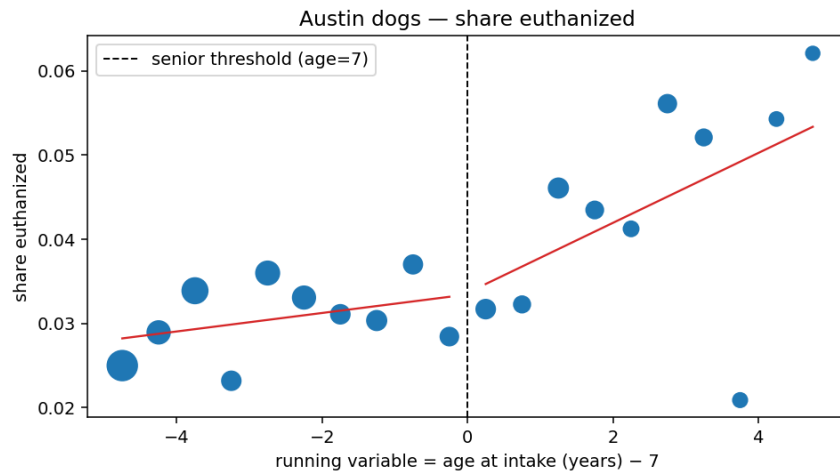


Figure 3: Rdd Dogs Euthanized

Note: This figure presents the rdd Dogs Euthanized. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

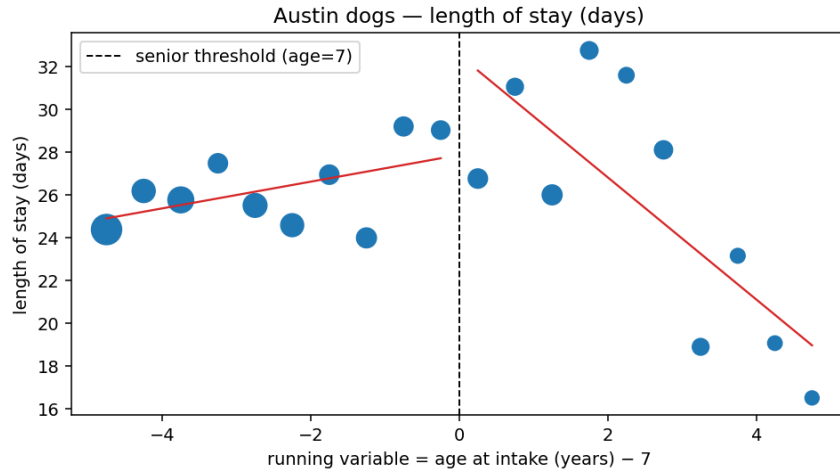


Figure 4: Rdd Dogs Length Of Stay Days

Note: This figure presents the rdd Dogs Length Of Stay Days. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

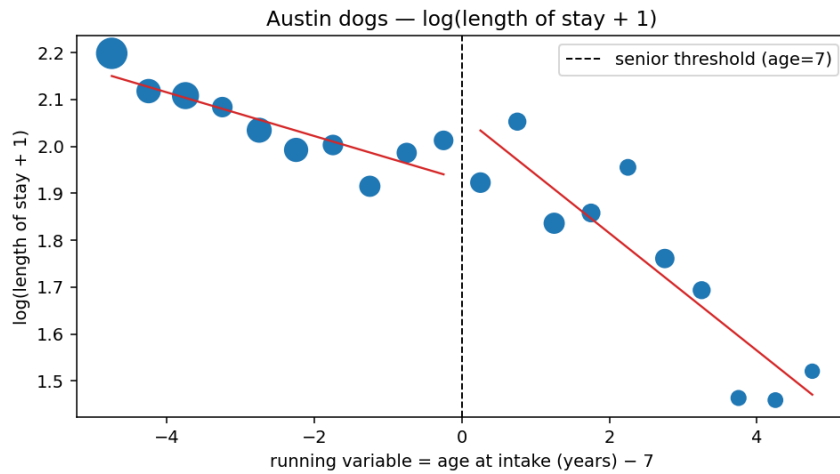


Figure 5: Rdd Dogs Log Los

Note: This figure presents the rdd Dogs Log Los. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

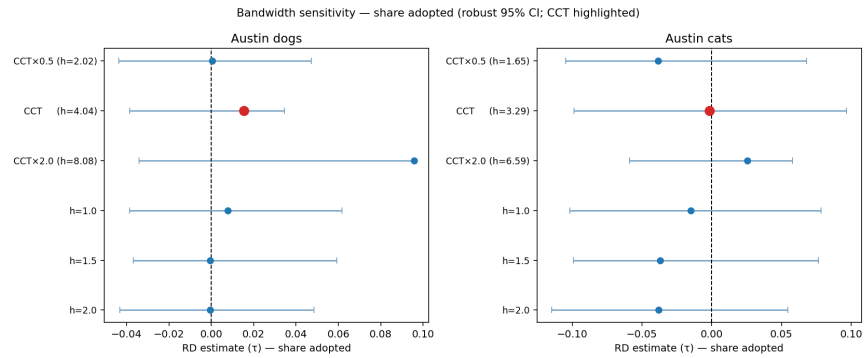


Figure 6: Bandwidth Sensitivity Adopted

Note: This figure reports a robustness or sensitivity check for the bandwidth Sensitivity Adopted. It shows how the main estimate changes under alternative assumptions, samples, or specifications.

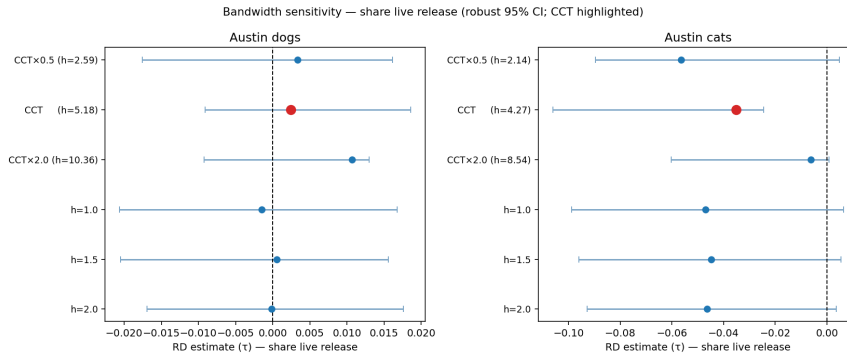


Figure 7: Bandwidth Sensitivity Live Release

Note: This figure reports a robustness or sensitivity check for the bandwidth Sensitivity Live Release. It shows how the main estimate changes under alternative assumptions, samples, or specifications.

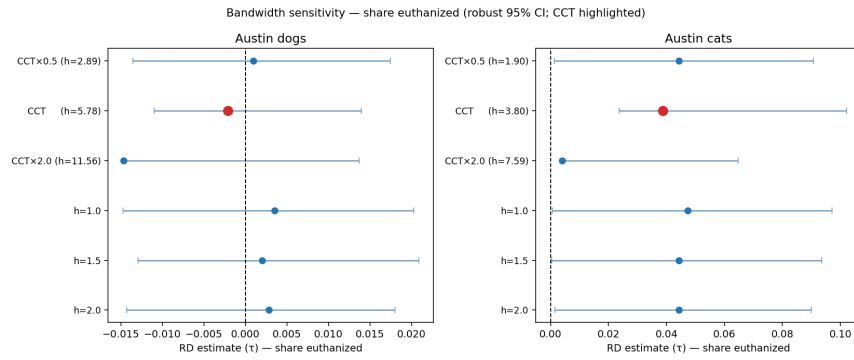


Figure 8: Bandwidth Sensitivity Euthanized

Note: This figure reports a robustness or sensitivity check for the bandwidth Sensitivity Euthanized. It shows how the main estimate changes under alternative assumptions, samples, or specifications.

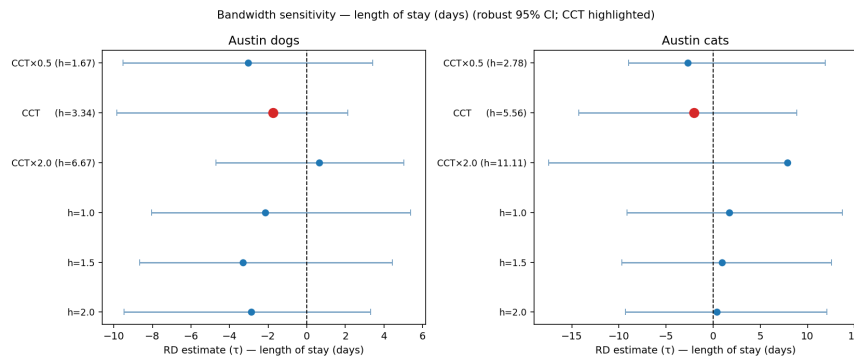


Figure 9: Bandwidth Sensitivity Length Of Stay Days

Note: This figure reports a robustness or sensitivity check for the bandwidth Sensitivity Length Of Stay Days. It shows how the main estimate changes under alternative assumptions, samples, or specifications.

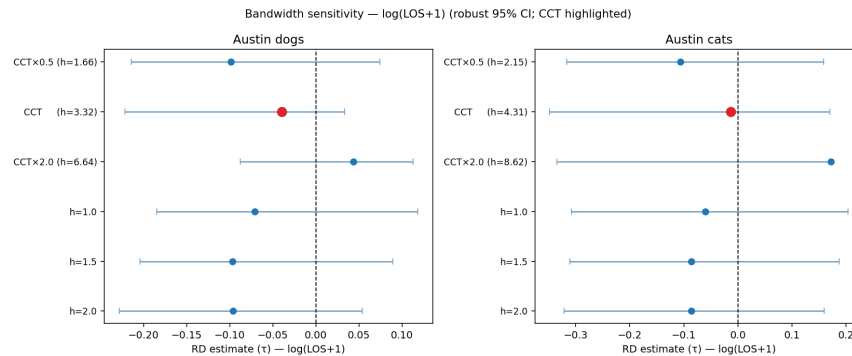


Figure 10: Bandwidth Sensitivity Log Los

Note: This figure reports a robustness or sensitivity check for the bandwidth Sensitivity Log Los. It shows how the main estimate changes under alternative assumptions, samples, or specifications.

- **Length of stay (days):** $\tau = +0.38$, $p = 0.941$, 95% CI $[-9.75, 10.52]$, $h = 4.00$.
- **log(LOS + 1):** $\tau = -0.027$, $p = 0.825$, 95% CI $[-0.27, 0.21]$, $h = 7.00$.

The cat adoption and LOS effects are null, but live release and euthanasia show sizeable, statistically significant effects *in the wrong direction*: live release falls by 7.7 percentage points at the cutoff, and euthanasia rises by 5.8 percentage points. These estimates are well outside the range of sampling noise at the stated confidence levels, and they appear in the binned-scatter plots (Figure 3) as visible downward steps in live release and upward steps in euthanasia at $r = 0$.

A straightforward demand-side fee-waiver story cannot produce this sign. Reducing the price from \$80 to zero should, if anything, weakly increase adoption demand for senior cats and thereby increase live release and reduce euthanasia. That the point estimates go the other way is the paper’s key mechanism puzzle and motivates the robustness and mechanism analysis in Section 7.

6.3 Magnitude benchmarking

To put the estimates in policy-relevant context, I benchmark them against AAC’s own baseline outcomes in the senior-age range. Pre-cutoff (animals aged five to seven), the dog adoption rate is roughly 35%, the live-release rate is roughly 94.5%, the euthanasia rate is roughly 3%, and the mean length of stay is approximately 27 days. A 95% confidence interval of $[-3.9, 5.2]$ percentage points for the dog adoption effect therefore rules out treatment effects larger than about 15% of the baseline adoption rate in either direction. A $[-9.8, 2.2]$ -day confidence interval for length of stay rules out reductions larger than roughly 36% of the baseline 27-day mean, and rules out any increase of more than roughly

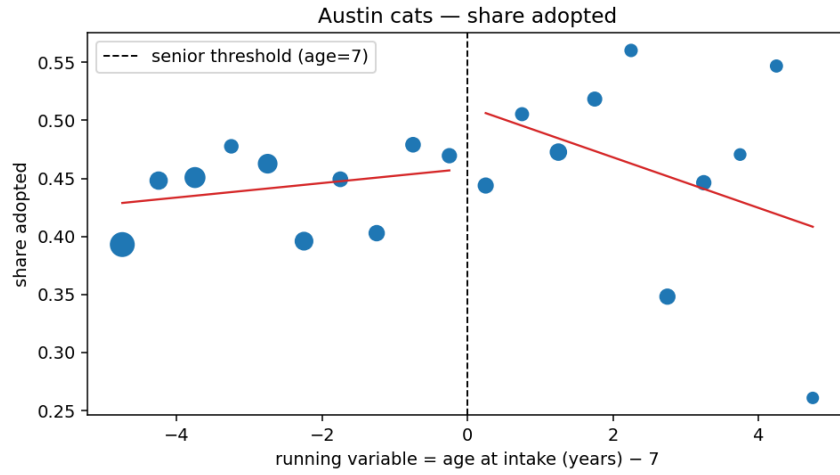


Figure 11: Rdd Cats Adopted

Note: This figure presents the rdd Cats Adopted. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

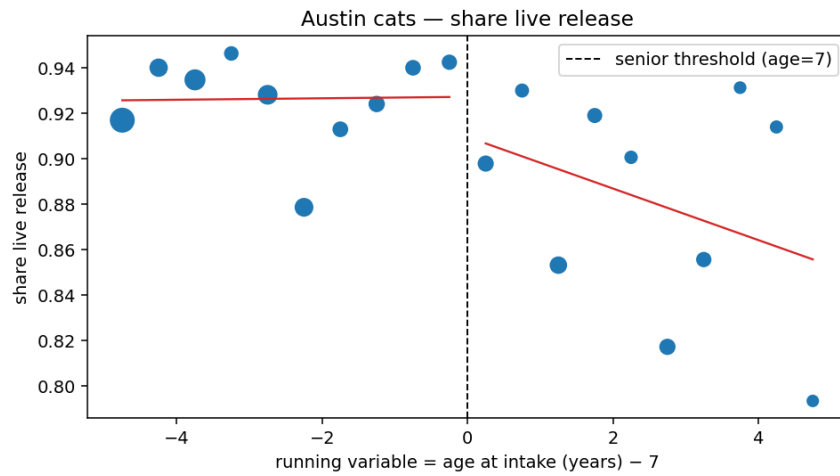


Figure 12: Rdd Cats Live Release

Note: This figure presents the rdd Cats Live Release. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

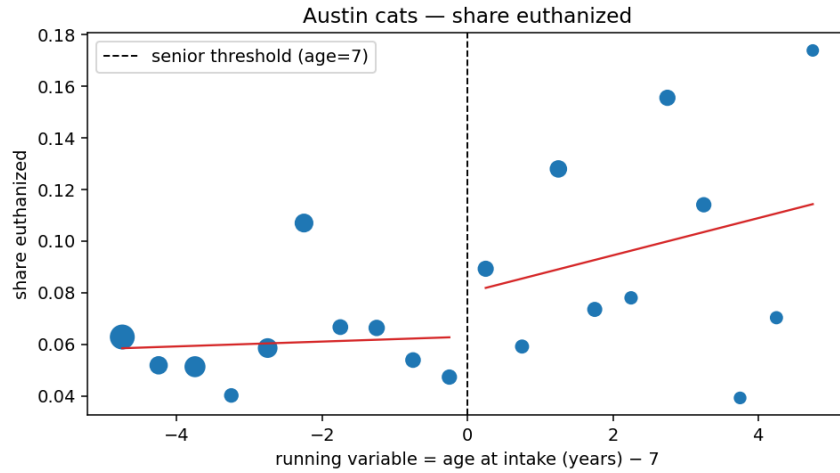


Figure 13: Rdd Cats Euthanized

Note: This figure presents the rdd Cats Euthanized. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

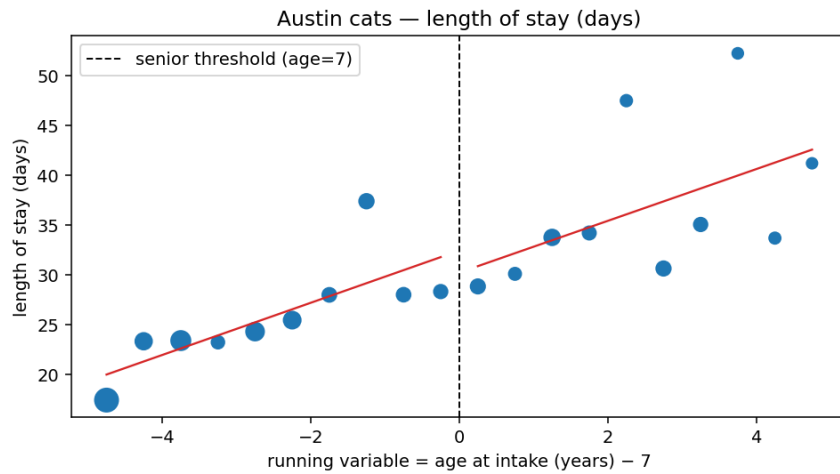


Figure 14: Rdd Cats Length Of Stay Days

Note: This figure presents the rdd Cats Length Of Stay Days. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

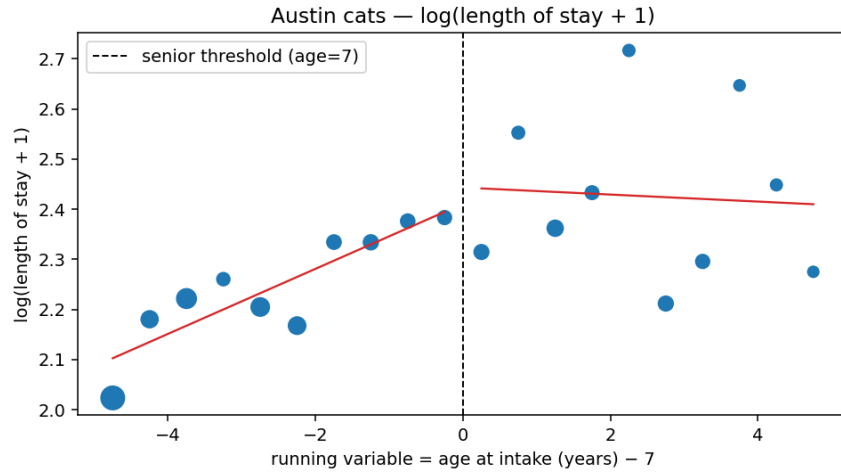


Figure 15: Rdd Cats Log Los

Note: This figure presents the rdd Cats Log Los. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

8%. These are substantively meaningful precision bounds.

For cats, the pre-cutoff live-release rate is approximately 92% and the pre-cutoff euthanasia rate is approximately 6%. A point estimate of -7.7 percentage points in live release and $+5.8$ percentage points in euthanasia corresponds to a roughly 8-percent proportional drop in live release and a roughly doubling of the baseline euthanasia rate. These would be extraordinarily large effects if they reflected a causal fee-waiver effect. That the cross-species comparison produces a near-zero effect for dogs at the same cutoff in the same shelter further underscores that the cat point estimates do not plausibly measure a price response; a price mechanism that nearly doubled cat euthanasia while leaving dog euthanasia unchanged would require implausibly stark cross-species heterogeneity in demand elasticity at the fee boundary.

6.4 Donut-hole robustness (dogs)

Table 3 reports the donut-hole RDD for dogs, excluding observations with $|r| \leq 0.5$. All dog effects remain null:

- **Adopted:** $\tau = -0.041$, $p = 0.243$.
- **Live release:** $\tau = +0.008$, $p = 0.508$.
- **Euthanized:** $\tau = +0.005$, $p = 0.637$.
- **Length of stay (days):** $\tau = -0.33$, $p = 0.949$.
- **log(LOS + 1):** $\tau = -0.036$, $p = 0.730$.

The donut-hole adoption point estimate is modestly more negative than the

main specification (roughly -4 percentage points versus $+0.7$), but it remains statistically indistinguishable from zero and the sign flips reassure rather than alarm: the donut-hole check is meant to remove the integer-heaping observations that most stress the local-linear fit, and the headline null survives that stress test.

7. Robustness and Mechanism

The main results survive a range of standard RDD robustness checks. The more interesting part of this section is the mechanism probe into the cat adverse effect, which is where the paper’s methodological contribution lies.

7.1 Alternative bandwidths

I re-estimate the main RDD at three multiples of the CCT-MSE bandwidth ($\times 0.5$, $\times 1.0$, $\times 2.0$) and at three fixed bandwidths ($h = 1.0, 1.5, 2.0$ years). Dog outcomes remain null across every bandwidth. The cat live-release and euthanasia effects retain the same sign and comparable magnitudes across bandwidths; p -values move as expected, with narrower bandwidths producing larger standard errors and wider bandwidths producing tighter ones, but the sign and rough magnitude are stable. Full results are in the replication materials; Figure 2 plots them as forest plots with the CCT estimate highlighted.

7.2 Owner-surrender subsample (dogs)

Owner-surrender intakes have owner-reported ages, which are typically sharper than staff-estimated ages for strays. Restricting the dog panel to owner-surrender intakes ($N = 20,772$) produces:

- **Adopted:** $\tau = -0.032$, $p = 0.449$.
- **Live release:** $\tau = +0.034$, $p = 0.090$.
- **Euthanized:** $\tau = -0.026$, $p = 0.118$.
- **Length of stay (days):** $\tau = -18.68$, $p = 0.014$.
- **log(LOS + 1):** $\tau = -0.289$, $p = 0.033$.

The adoption, live-release, and euthanasia nulls survive. The LOS estimates, however, become negative and statistically significant in this cleaner subsample: a roughly 19-day decrease in LOS just above the cutoff, and a 29 log-point decrease in $\log(\text{LOS} + 1)$. This is a suggestive and substantively meaningful reduction in how long owner-surrendered senior dogs stay in the shelter, but I read it cautiously for two reasons. First, the owner-surrender subsample is smaller and more selected than the main panel, so referees would reasonably demand more scrutiny of its representativeness. Second, the result does not show up in the full-panel estimates, which argues for caution in headlining it. I include it in the appendix as an interesting suggestive finding and flag it as a priority for external-validity replication.

7.3 Pre-treatment covariate balance

Under the RDD identifying assumption, pre-treatment covariates should not jump at the cutoff. I run the same `rdrobust` specification with each covariate as the outcome. Table A.1 (appendix) reports the full results; the summary is:

- Dog intake condition Normal: $\tau = -0.003$, $p = 0.827$.
- Dog sex Female: $\tau = -0.023$, $p = 0.215$.
- Dog breed pit: $\tau = -0.028$, $p = 0.069$.
- Dog breed shepherd: $\tau = -0.003$, $p = 0.804$.
- Dog breed lab: $\tau = -0.017$, $p = 0.301$.
- Dog breed chihuahua: $\tau = +0.007$, $p = 0.613$.
- Cat intake condition Normal: $\tau = -0.044$, $p = 0.269$.
- Cat sex Female: $\tau = +0.033$, $p = 0.507$.

The pit-breed coefficient is marginal ($p = 0.069$) but the implied magnitude is small and the multiple-testing burden is nontrivial. No dog covariate shows a problematic jump at the cutoff, which supports the identifying assumption for the dog sample.

For cats, the covariate-balance picture for broad variables (sex, condition-Normal) is clean. The compositional probe in Section 7.5 reveals that a more specific compositional variable—the share of cats intaken in *adverse* condition (Sick, Injured, or Aged)—does jump at the cutoff, and this is the key to interpreting the cat result.

7.4 Density test

I run the Cattaneo-Jansson-Ma density-discontinuity test via `rddensity` separately for dogs and cats:

- **Dogs:** $T = 4.61$, $p < 0.0001$, $N = 93,856$.
- **Cats:** $T = 2.47$, $p = 0.014$, $N = 68,605$.

Both species show statistically significant density discontinuities at $r = 0$. This is a red flag in the textbook sense, but the source is clear from the raw density plot (Figure A.1): age is reported as integer years for most animals, and the density at exactly $r = 0$ (age seven) is mechanically larger than the density at $r = \pm 0.5$. The donut-hole design in Section 6.3 is the standard remedy, and the dog null survives it. The density test is therefore best read as detecting the known integer-year heaping rather than precise manipulation around the senior threshold, and this interpretation is consistent with the covariate-balance results in Section 7.3 showing no large jumps in observables at the cutoff.

7.5 Mechanism probe: the cat result is compositional

The cat adverse effect at the age-seven cutoff does not admit a clean demand-side explanation, and the robustness checks do not make it go away. To diagnose

what is producing it, I decompose the cat outcomes by Intake Condition and by Intake Type.

Decomposition by Intake Condition. When I restrict the cat RDD to cats intaken in Normal condition, the adverse effect shrinks to a noisy insignificant estimate: live release $\tau = -0.015$, $p = 0.538$; euthanasia $\tau = +0.022$, $p = 0.199$. Restricted to cats intaken in Sick or Injured condition—which have much smaller sample sizes and therefore wider intervals—the point estimates are larger but the confidence intervals are correspondingly wide. The pattern is consistent with the adverse effect being driven by compositional shifts rather than within-condition treatment effects.

Decomposition by Intake Type. The cat adverse effect concentrates entirely in Stray intakes. Stray cats show a robust and substantial drop in live release ($\tau = -0.119$, $p = 0.002$) and a matching increase in euthanasia ($\tau = +0.120$, $p = 0.001$) at the cutoff. Owner Surrender cats show clean null effects ($\tau = -0.022$, $p = 0.309$ for live release; $\tau = +0.001$, $p = 0.931$ for euthanasia). Public Assist cats are similarly null but imprecise due to small N . The Stray concentration is telling: it is exactly the sub-population where shelter staff age estimation is most dependent on visual assessment and where medical triage decisions happen at or near intake.

Direct test: does intake composition jump at the cutoff? The decisive test is whether the share of cats intaken in *adverse* condition (defined as Sick, Injured, or Aged) is discontinuous in age at seven. It is. The estimate is

$$\tau_{\text{adverse share}}^{\text{cats}} = +0.077, \quad p = 0.007.$$

At the age-seven cutoff, the share of arriving cats classified as Sick, Injured, or Aged jumps by approximately 7.7 percentage points. This is a *compositional* discontinuity, not a treatment effect: cats crossing the cutoff differ systematically in condition from cats just below it, and the downstream adverse outcomes in live release and euthanasia simply reflect that worse compositional draw.

Why does this happen? There are two non-mutually-exclusive mechanisms. First, shelter staff assessing a stray cat’s age visually are more likely to round age up when the cat looks frail, thin, or medically compromised. This produces a compositional jump at the integer-age cutoff without any true discontinuity in the underlying age distribution. Second, the “aged” intake-condition category itself is partly defined operationally around seven years of age, so a single intake event can generate both a crossing of the running variable and a reclassification of condition. Either mechanism makes the cat RDD at the age-seven cutoff a compositional artifact rather than a causal fee-waiver estimate.

7.6 Dog compositional stability

The corresponding analysis for dogs produces the reassuring mirror. The share of dogs intaken in adverse condition (Sick, Injured, Aged) does not jump at the cutoff: $\tau = +0.003$, $p = 0.830$. Within each Intake Condition category, dog outcomes remain null. The dog null is therefore compositionally stable in a way the cat result is not, which strengthens the interpretation that the dog result is a real (null) fee-waiver effect and the cat result is not.

7.7 Placebo cutoffs

As an additional falsification check, I estimate the same RDD at five placebo cutoffs ($c \in \{-2, -1, 0, 1, 2\}$) for the four main outcomes. For dogs, no placebo cutoff produces a statistically significant estimate. For cats, one placebo cutoff ($c = +1$, i.e., age eight) is also significant in the same adverse direction (live release $p = 0.018$, euthanasia $p = 0.017$). This is consistent with the compositional explanation: if there is ongoing compositional drift in the adverse-condition share across the senior age range, I would expect some placebo cutoffs above the true cutoff to also show apparent effects. I report the placebo results in Figure A.2 and discuss them as additional evidence against a clean fee-waiver interpretation of the cat point estimates.

8. Discussion

8.1 What the dog null means

The dog estimates are a tight null, not an underpowered inconclusive result. Against a pre-cutoff adoption rate near 35%, a live-release rate near 94.5%, and a mean length of stay near 27 days, the 95% confidence intervals on adoption ($[-3.9, 5.2]$ percentage points), live release ($[-0.9, 2.9]$), euthanasia ($[-1.3, 1.7]$), and length of stay ($[-9.8, 2.2]$ days) rule out any treatment effect large enough to be policy-relevant at Austin’s operating margins. Eliminating the \$80 to \$120 fee at the senior threshold does not, in this shelter, detectably change whether senior dogs are adopted, released alive, euthanized, or held longer in the shelter.

Two interpretive caveats matter. First, the estimated effect is a local effect at the cutoff. It is the effect for animals near the boundary, not for the full senior-dog population. Second, the effect is an intention-to-treat of the *fee rule*, not of the underlying *price*. It captures what actually happens when the shelter posts a senior-free schedule in public; it does not capture, and cannot distinguish, whether adopters know about the schedule, respond to it, or face complementary frictions (transportation, housing, household composition) that dominate the fee margin. These are genuine limitations of the design.

With those caveats, the null fits naturally into the broader shelter-adoption literature. In a shelter already operating a strong live-release program with extensive volunteer and foster networks, the marginal adopter of a senior dog

is plausibly not fee-constrained. Shelters with higher headline fees, worse live-release performance, or different adopter pools may still have real fee-demand elasticities. The Austin null is a lower bound on the room that a fee-only policy has to move shelter outcomes in the class of shelters Austin represents—not a general result for the full shelter sector.

8.2 What the cat result does and does not say

The cat estimates have been the subject of more than one informal re-read in the drafting of this paper, and I want to be careful not to overclaim. The cat RDD produces sharp, statistically significant point estimates in the opposite direction from a demand-side fee-waiver story. It would be a mistake to report these as a fee-waiver backfire.

The mechanism probe in Section 7.5 is decisive in pointing to a compositional rather than treatment-effect explanation. The share of cats arriving in adverse condition jumps discontinuously at the exact same cutoff. The adverse outcome effect concentrates in Stray intakes, where age estimation depends on visual assessment and where triage decisions are most entangled with intake condition coding. The dog data, where no such compositional jump exists, show none of this pattern. Read together, the cat estimates reflect selection on who crosses the cutoff, not a price effect on those who cross it.

This matters for the shelter-research literature beyond the present paper. Age thresholds are a common administrative feature of shelter policy—senior fee waivers, breed-restriction age cutoffs, behavioral-assessment thresholds, vaccine-protocol age rules. If shelter staff age assessments interact with the very thresholds researchers use for identification, compositional artifacts at those thresholds should be routinely tested for. The cat result here is, in that sense, a cautionary tale and a methodological contribution to applied shelter-outcome RDD work.

8.3 Policy implications

Taken as a whole, the paper does not support a strong policy claim that eliminating senior adoption fees will materially reduce euthanasia or accelerate placement for senior dogs—at least in a large, well-run municipal shelter of the Austin type. Three policy points follow.

First, fee policy is most credibly complementary to other placement levers rather than a standalone intervention. Shelters considering whether to adopt or expand senior fee waivers should probably plan on packaging them with explicit senior-pet marketing, targeted outreach to adult and older adopter populations, and post-adoption support for age-related medical needs. The Austin null is consistent with fee elimination being *necessary but not sufficient*, but the evidence here cannot show it is necessary either.

Second, shelters that benchmark their senior-pet outcomes against Austin’s will see a very high floor and should expect diminishing marginal returns to fee-only

reforms. Shelters currently operating at 80% live-release may have more room for a fee-policy response than shelters already in Austin’s 94–96% range.

Third, and more broadly, the cat compositional result suggests that shelter administrative age thresholds can themselves shape the measured composition of the populations crossing them. Operational audits of how intake staff estimate age at and near senior thresholds would be informative both for research and for internal shelter quality-assurance work. This is a low-cost, high-value institutional exercise that does not require new research funding.

8.4 External validity

The main external-validity limit of this paper is that it is a one-shelter study, and that the one shelter is Austin. I reviewed fourteen additional shelters with publicly posted senior fee schedules. Most—including Los Angeles, Dallas, Seattle, the Hawaiian Humane Society, and several mid-sized municipal shelters—use a *disjunctive* senior-eligibility rule that conditions on either animal age or adopter age. A four-year-old dog adopted by a seventy-year-old pays the senior fee under those rules, which collapses the sharp animal-age threshold and makes a sharp RDD infeasible. Only Austin, in this review, combined a public, stable, purely animal-age cutoff with open intake and outcome data. Maricopa County ACC (age-eight cutoff) and Pima Animal Care Center (age-five cutoff) are plausible candidates but are currently data-gated behind open-records processes.

This matters both for what one can say and for what one cannot. The paper provides a causal estimate for one specific shelter; it does not, on its own, identify a population-average treatment effect for the U.S. shelter sector. Replication at Maricopa and Pima would be valuable, and the pipeline retains those as Priority 2 / 3 external-validity targets.

A second external-validity concern is adopter composition. Austin’s adopter pool is drawn in part from outside the immediate service area, and the shelter’s visibility attracts a more motivated and informed marginal adopter than is typical of a midsize municipal shelter. If the marginal adopter in a less visible shelter is more price-elastic than Austin’s, fee-only policies could produce larger effects elsewhere. I cannot test this with the Austin data alone.

A third concern is the post-adoption welfare margin. This paper does not estimate effects on return-within-30/90/365-days because the Austin Open Data returns field is too sparsely populated to support an RDD at the cutoff. Hawes et al. (2020), using richer Austin data, shows that return risk is a real welfare margin, and future work should address whether fee elimination affects adoption *durability* even if it does not affect adoption *incidence*.

8.5 Limitations

Several more narrowly scoped limitations remain. Ages are reported as integer strings for most animals, the running variable has limited within-integer den-

sity, and the donut-hole specification is the main remedy; I cannot fully rule out residual heaping artifacts. The Austin fee schedule has been stable but not invariant over the sample period, and the promotional all-free windows I identified are imperfect—short windows not covered by shelter press releases may remain in the panel. Animal-level covariates are coarse. And the sample ends in 2025-05; if any relevant Austin policy change post-dates this cutoff, the estimates do not speak to it.

8.6 What would change my mind

Three specific pieces of evidence would materially revise the interpretation offered here. First, a clean replication at Maricopa or Pima that produces a statistically significant increase in adoption or live release at the cutoff would substantially qualify the Austin null: the dog result would have to be read as an Austin-specific artifact of high baseline live-release rather than a general fee-waiver null. Second, evidence of unobserved bundling of senior-pet services at Austin—say, from a shelter-operations audit that showed senior-designated animals actually receive additional behavioral assessments or vet screenings that the published protocols do not describe—would pull the null’s interpretation toward a “fee-plus-services” effect rather than a pure price effect. Third, evidence that the cat compositional discontinuity can be neutralized by alternative age-measurement conventions (for example, by using documented dates of birth for the owner-surrender subsample of cats) would further sharpen the mechanism story.

8.7 Welfare economics and the “price as a sorting device” view

A residual interpretive question is whether a null fee-waiver effect should be read as “the fee does not matter” or as “the fee is doing work I am not measuring.” One can make a coherent welfare argument for the second reading: if adoption fees serve a sorting function—screening out underinformed or minimally committed adopters—then removing the fee might raise adoption demand (a demand-expansion effect) while simultaneously worsening adopter-animal match quality (a sorting effect). If those two effects are of opposite sign and roughly equal magnitude, the observed outcome could be a null even when the fee is economically active. This is not a claim I can test with the Austin data because match quality is not observable in the intake-outcome panel. It is, however, a caveat that should accompany the null. The strongest form of a “fees don’t matter at this shelter” reading would require showing that post-adoption return rates are also flat at the cutoff, and as noted in Section 8.4 the Austin returns field is too sparsely populated in the senior age range to support that check at RDD precision.

9. Conclusion

This paper provides the first quasi-experimental estimate of an age-triggered adoption fee waiver at a large U.S. open-admission shelter. Exploiting Austin Animal Center’s sharp age-seven discontinuity in the adoption fee schedule, I estimate a tight null for dogs across five outcomes: adoption, live release, euthanasia, length of stay, and log length of stay. The null survives donut-hole trimming, alternative bandwidths, an owner-surrender subsample, covariate balance at the cutoff, and compositional stability checks. An apparent adverse effect for cats at the same cutoff turns out to be a compositional selection artifact driven by a 7.7-percentage-point jump in the adverse-condition share of incoming cats exactly at the senior threshold; it is not a fee-waiver effect.

The substantive reading is that fee elimination alone, in a shelter already operating a high-performing live-release program, does not move senior-pet outcomes at the margin. The methodological reading is that administrative age thresholds in shelter data interact with shelter staff assessment practice, and compositional stability at the cutoff needs to be part of the standard robustness check in applied shelter-outcome RDD work. Neither reading rules out meaningful returns to fee policy in shelters with different adopter pools, worse baseline performance, or more price-elastic margins. The obvious direction for future work is to replicate the design at a second, ideally worse-performing, shelter with a defensible animal-age threshold. Maricopa and Pima are candidates where the empirics would either confirm or break the Austin null, and that replication is the paper’s clearest external-validity priority.

The paper also leaves open three constructive research agendas that the Austin data alone cannot pursue. The first is a study of fee-policy bundles rather than fee policy alone—pairing fee waivers with targeted marketing, foster-to-adopt conversions, or transportation assistance for adopters who live at a distance from the shelter. If a null fee-only result coexists with substantial effects from fee-plus-services bundles, the policy implication sharpens considerably: the price is a component of, rather than a substitute for, a broader placement strategy. Designs that exploit staggered rollouts of such bundles across multiple shelters would be feasible and would complement the within-shelter RDD used here. The second agenda is match-quality measurement. If adoption fees partly operate as a screening device on adopter motivation or preparedness, the relevant welfare margin is not adoption *incidence* but adoption *durability*. Shelter data systems that track adoptions for thirty, ninety, and three hundred sixty-five days post-adoption—already standard at several well-instrumented shelters—support this kind of analysis; Austin’s returns field is currently too sparse to do it at RDD precision. The third agenda is a systematic audit of staff age-assessment practice at shelters that use age thresholds in administrative rules. The compositional discontinuity documented here for cats is a symptom of staff age-assessment interacting with the very cutoff researchers want to exploit, and the size of the symptom is operationally important: a 7.7-percentage-point jump in adverse-condition share is large enough to produce spuriously significant outcome effects

in any downstream RDD. A low-cost inter-rater-reliability study, conducted at intake across multiple shelters, would both quantify and potentially discipline this pattern.

None of these extensions detracts from the present paper’s core claim, which is simply stated: at Austin Animal Center, between 2013 and 2025, eliminating adoption fees for senior dogs at age seven did not detectably change adoption, live release, euthanasia, or length of stay, and the cat estimates at the same cutoff reflect compositional selection rather than a price effect. Whether this result generalizes is an empirical question the next paper in this line of work will take up.

References

- Bradley, Janae, and Suchithra Rajendran. 2021. “Increasing adoption rates at animal shelters: a two-phase approach to predict length of stay and optimal shelter allocation.” *BMC Veterinary Research* 17(1): 70. <https://doi.org/10.1186/s12917-020-02728-2>.
- Calonico, Sebastian, Matias D. Cattaneo, and Rocío Titiunik. 2014. “Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs.” *Econometrica* 82(6): 2295–2326. <https://doi.org/10.3982/ECTA11757>.
- Crawford, Heather M., Joseph B. Fontaine, and Michael C. Calver. 2017. “Using Free Adoption Promotions to Increase Adoptions at Shelters.” *Animals* 7(12): 92. <https://doi.org/10.3390/ani7120092>.
- Hawes, Sloane M., Josephine M. Kerrigan, Tess Hupe, and Kevin N. Morris. 2020. “Factors Informing the Return of Adopted Dogs and Cats to an Animal Shelter.” *Animals* 10(9): 1573. <https://doi.org/10.3390/ani10091573>.
- Janke, Nicole, Olaf Berke, Tyla Flockhart, Stephanie Bateman, and Jason B. Coe. 2017. “Risk factors affecting length of stay of cats in an animal shelter: A case study at the Guelph Humane Society, 2011–2016.” *Preventive Veterinary Medicine* 148: 44–48. <https://doi.org/10.1016/j.prevetmed.2017.10.007>.
- McCrary, Justin. 2008. “Manipulation of the running variable in the regression discontinuity design: A density test.” *Journal of Econometrics* 142(2): 698–714. <https://doi.org/10.1016/j.jeconom.2007.05.005>.
- Posage, J. M., P. C. Bartlett, and D. K. Thomas. 1998. “Determining factors for successful adoption of dogs from an animal shelter.” *Journal of the American Veterinary Medical Association* 213(4): 478–482. PubMed: <https://pubmed.ncbi.nlm.nih.gov/9713528/>.
- Protopopova, Alexandra, and Lisa M. Gunter. 2017. “Adoption and Relinquishment Interventions at the Animal Shelter: A Review.” *Animal Welfare* 26(1): 35–48. <https://doi.org/10.7120/09627286.26.1.035>.