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Net Reductions or Spatiotemporal Displacement of Intentional Wildfires in Response to Arrests? Evidence from Spain

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Abstract. Research has not examined how the impacts of arrests manifest across space and time in environmental crimes. We evaluate whether arrests reduce or merely spatiotemporally displace intentional illegal outdoor firesetting. Using municipality-level daily wildfire count data from Galicia, Spain, from 1999 to 2014, we develop daily spatiotemporal ignition count models of agricultural, non-agricultural, and total intentional illegal wildfires as functions of spatiotemporally lagged arrests, the election cycle, seasonal and day indicators, meteorological factors, and socio-economic variables. We find evidence that arrests reduce future intentional illegal fires across space in subsequent time periods.

Brief Summary. We evaluate whether arrests of intentional illegal firesetters lead to movements in or reductions of future fires in nearby locations. Our analyses of daily wildfire count data from Galicia, Spain, 1999 to 2014, show that arrests lead to overall reductions in intentional illegal firesetting.

Introduction

Natural resource managers worldwide face a challenge of how to limit the occurrence of accidentally and intentionally ignited wildfires that destroy property, damage resources, and harm people. A changing climate is projected to increase wildfire potential, severity, and extent (e.g., Krawchuk et al. 2009; Liu et al. 2010; Jolly et al. 2015), potentially exacerbating the damages and raising the urgency of addressing human-ignited wildfires (Balch et al. 2017; Bowman et al. 2017). Among the strategies and tactics available to policy makers in response to heightened wildfire risks is to boost law enforcement efforts (e.g., Donoghue and Main 1985; Butry and Prestemon 2005; Prestemon and Butry 2005; Prestemon and Butry 2010; Abt et al. 2015) aimed at apprehending and deterring intentional firesetters[1]. With only a few exceptions (e.g., Thomas et al. 2011; Prestemon et al. 2012), research has not evaluated how and whether arrests, a discrete measure of law enforcement efforts, are linked to reductions in the occurrence of intentional fires or whether such efforts have broader impacts across space and time.

The effects of law enforcement efforts are potentially complex, considering motivated offenders' dynamic responses. Criminologists have long recognized that actions by law enforcement or other groups intended to reduce crime in one time or place or of one type might be at least partially offset by compensating shifts in behaviors of prospective offenders in response to those efforts (e.g., Cornish and Clarke 1987; Barr and Pease 1990; Eck 1993; Clarke and Weisburd 1994; Guerette 2009). While spatiotemporal concentrations in arson are evident in maps of wildfires (e.g., Mothershead 2012), no study has sought to characterize the effects of

[1] In this study, we define all intentional firesetting as illegal and so refer to intentional illegal firesetting as 'intentional firesetting.'

arrests on these fires at fine spatial scales or over longer time scales than detected by Prestemon et al. (2012). Understanding how law enforcement efforts affect the timing and locations of future offending, including illegal firesetting, is critical to enacting effective policies and in the design of more effective law enforcement strategies (e.g., Weisburd and Eck 2004) aimed at reducing the occurrence of unwanted wildfires.

The primary objective of our study is to assess whether there is evidence of spatiotemporal displacement effects of law enforcement in intentional outdoor firesetting. To do this, we develop statistical models that control for hypothesized driving factors in addition to arrests. Among the driving factors we include are variables describing the seasonality of intentional firesetting, which likely stems from regular variations in fuel flammability and ignition attempts related to agricultural or other seasonal activities (e.g., hunting) and daily human routines (days of the week and holidays). Additionally, we include variables explaining firesetting variation at longer time scales, which capture associations with slowly changing aggregate wildland fuels and demographic and economic conditions. Finally, we include a set of indicators of election seasons, which have been shown in other research to be criminogenic for firesetting in Europe (e.g., Seijo 2005; Álvarez-Díaz et al. 2015; Ramos and Sanz 2018).[2] With the inclusion of election season indicators, we are able to quantify the daily time scale imprint of one form of politically based protest actions (Pyne 1995), extending existing research and

[2] Mechanisms proposed or identified for how electoral cycles could influence intentional firesetting include election-related adjustments in law enforcement effectiveness—law enforcement budgets (Efthyvoulou 2012) and police force levels (Levitt 1997)—and the scope or severity of criminal sanctions (Smith 2004; Dyke 2007; Berdejó and Yuchtman 2013).

providing additional policy relevant insights. Their inclusion also helps to explain variation in observed intentional wildfires, increasing the power of our statistical inferences regarding law enforcement spatiotemporal effects.

This study advances the earlier work by Prestemon et al. (2012) by including spatiotemporal lags of arrests in the equation specifications. While those authors allowed for effects of arrests at long temporal scales, their models did not include arrests lagged as long as done in this study. Furthermore, their models were not specified in a way that quantified how intentional fires in one location could be associated with arrests in nearby locations in previous time periods. Finally, Prestemon et al. (2012) ignored the potentially differing firesetting processes across reported motivations, which we find are distinct in their responses to arrests, a potentially important distinction with implications for law enforcement resource allocation decisions. The study also advances research into the role of elections on wildfires in Spain conducted by Álvarez-Díaz et al. (2015), who used reduced-form vector error correction modeling methods to identify the association between the national total numbers of forest fires during “intense” electoral years. Our study, in contrast, estimates count data structural equations of intentional wildfires in Spain, controlling for the many factors that influence wildfire ignition processes, including the motivations of firesetters, arrests, and elections. Our equations are estimated at fine temporal (daily) and spatial (municipality) scales, measuring how specific phases of elections affect the daily pattern of firesetting in the run-up to elections.

Theory and methods

Theory of intentional firesetting

Cohen and Felson (1979) describe a Routine Activities (RA) theory of crime which forms the foundation upon which we build our empirical model of the numbers of intentional wildfires. These authors contend that criminal activities vary over time and space according to variations in the simultaneous overlap of three essential elements of crime occurrence: (1) a motivated offender, (2) a suitable target, and (3) the absence of capable guardians against a violation; if one of these elements is missing, then a crime will not occur. It is straightforward to design an empirical modeling framework around RA theory because motivated offenders and suitable targets are simple to define.

Geographers, philosophers, and criminologists dating back several centuries have recognized that persistent criminal activity has evident spatial components related to societal and landscape features (see Cohen [1941] and Cohen and Felson [1979] for informative syntheses). As Cohen and Felson (1979) argue, defining RA theory, persistent spatial and temporal concentrations of offending may be attributed to fine and coarse scale spatial and temporal (hourly, daily, seasonal) features of landscapes and routine human behaviors as well as to temporally trending variations in the relative abundances of motivated offenders, suitable targets, and capable guardians. Here, we propose that RA theory is an amenable framework for understanding these observed spatial and temporal patterns. In the development of an empirical version of a Routine Activities theory of intentional firesetting, we identify candidate physical, biological, and societal variables that vary over space and time that can be mapped to one (or more) of the three elements of RA theory. With this empirical framework, we are able to test for spatiotemporal displacement and uncover the effects of other policy relevant phenomena on intentional firesetting.

We summarize the variables connected to each of these three elements of RA theory as it relates to intentional firesetting:

Motivated offenders: Variations in the numbers of motivated firesetters in a location is likely related to several demographic, social, and economic factors, including the size of the human population, historical efforts to remove known offenders from the location, and factors that influence motivations to offend. Such motivational factors can be broadly defined as incentives to commit a crime (e.g., utility gained or pecuniary benefits acquired). In the case of intentional wildland firesetting, greater incentives might exist during times of political discord (Dyke 2007), when firesetters protest by igniting wildfires (e.g., Pyne 1995; Kull 2002; Seijo 2005, 2009; Hovardas 2014, 2015; Skouras and Christodoulakis 2014; Álvarez-Díaz et al. 2015; Ramos and Sanz 2018); when agricultural areas may be prepared for planting by (illegally) burning them; and when the opportunity costs of time for firesetters are low (e.g., in times and places where they are unemployed) (e.g., Prestemon and Butry 2005; Sebastián-López et al. 2008), which reduces the pecuniary cost of spending time trying to ignite fires and the expected costs of being arrested and imprisoned for igniting a fire.

Suitable targets: Variations in suitable targets for intentional wildfires can be explained by variations in the availability of flammable wildland fuels and in the weather suitable for successful ignition and spread.

Capable guardians: Variations over time and space in capable guardians could be indexed by law enforcement efforts; the number of patrol officers or the effectiveness of law enforcement actions (e.g., arrests) could vary over space and time. Measures of political discord, which can induce reallocations of law enforcement resources across space and time, could further proxy for short-run changes in capable guardianship.

Our introduction of arrests into a Routine Activities-based theory of intentional firesetting bears further exploration. Because offenders who are caught are often sanctioned by imprisonment, an arrest actually affects two elements of RA theory. First, the arrest can shrink the pool of motivated offenders by removing spatially stationary serial criminals from the landscape (e.g., Canela-Cacho et al. 1997; Ratcliffe and Rengert 2008). Second, an arrest, if observed widely across space, potentially raises the perceived strength of capable guardianship.[3] Guardianship adjustments may be more likely to occur close to the location of the arrest (e.g., Levitt 1998; Pogarsky et al. 2004), we hypothesize, because arrest information may be more readily communicated locally to motivated offenders, achieving a local crime reduction subsequent to the arrest. If the arrest information is more broadly disseminated across space, then this adjustment could result in a deterrent effect across a larger spatial domain, revealed, in the case of intentional firesetting, as a reduction in the number of fires in subsequent time periods in

[3] Another way to view "motivation" is in terms of the prospective offender's expected net psychic or monetary benefits of crime commission, in the form advanced by Becker (1968). Hence, an increased probability of arrest would be expected to lower the expected net benefits of crime commission.

surrounding locations. The motivated offender may also be induced by an arrest to change their "domestic base" of operation in order to avoid localities where capable guardianship has increased (e.g., Telep et al. 2014). The result of this dynamic response by motivated offenders may be higher overall crime rates farther from the arrest location in subsequent time periods, revealing how law enforcement actions could displace crime in space-time (e.g., Eck 1993; Bowers and Johnson 2003; Guerette and Bowers 2009; Bowers et al. 2011). Displacement caused by an arrest would be revealed by, ceteris paribus, higher numbers of intentional fires in subsequent time periods in areas more distant from the arrest location.

Application of RA theory to intentional firesetting should comprehend existing evidence of positive temporal autocorrelation in intentional firesetting (e.g., Butry and Prestemon 2005; Prestemon et al. 2012). The cited studies attribute positive autocorrelation to a combination of serial and copycat firesetting. Kocsis and Irwin (1997) find that serial arsonists (along with rapists and burglars) tend to commit their crimes close to their domestic base but also exhibit an occasional "commuter pattern," ranging farther from the domestic base to carry out some of their offending. Such spatiotemporal clustering of firesetting is consistent with the near-repeat hypothesis in crime victimization tested by many analysts of certain property crimes (e.g., Townsley et al. 2003; Bowers and Johnson 2005; Bernasco 2008; Ratcliffe and Rengert 2008; Bernasco et al. 2015) and identified as well for non-wildland arson (Prestemon et al. 2013; Grubb and Nobles 2016).

Empirical specifications

The study location chosen is the region of Galicia, Spain, where intentional wildfires are the dominant cause attribution (APAS 2005). Galicia is also a region with a large enough number of intentional wildfires (half of Spain's) and associated arrests to permit statistical identification of spatiotemporal effects of arrests.

Accurate quantification of the spatiotemporal displacement of intentional firesetting due to arrests might depend on the type of motivated offender that is being modeled. Therefore, we test for the spatiotemporal effects of arrests and the effects of elections and other driving variables by partitioning our data according to whether a fire is classified as agriculturally or non-agriculturally motivated. Separate evaluation of these two broad categories of intentional firesetting could reveal the potential biases of modeling that ignores their different nature and uncover their distinct spatiotemporal dynamics. Also, separate modeling by motivation could potentially yield more accurate assessments of the overall effects of changes in law enforcement efforts and elections on firesetting in the region and reveal how prevention efforts might be differently focused on these two categories of intentional fires.

More formally, express the probability of I intentional wildfires started for motivation m in location j in period t as a function of a vector of variables affecting the number of motivated offenders in period t ($\mathbf{Y}_t$), a vector of variables measuring the availability of suitable targets in period t ($\mathbf{W}_t$), and vectors of measures ($\mathbf{A}$) of the presence of (effective) capable guardians in location j and nearby locations k over l lagged time periods as:

$$\Pr\,(I_{j,t}^{m}) = f(\mathbf{Y}_{j,t}, \mathbf{W}_{j,t}, \mathbf{A}_{j,t}, \mathbf{A}_{k,t}) \quad (1)$$

Vector $\mathbf{Y}_{j,t}$, the number of motivated offenders in our study, is comprised of the human population in the location ($P_{j,t}$); income earning ($G_{j,t}$) potential for residents of location j; the unemployment rate ($U_{j,t}$); dummies measuring holidays (H_t) and each of two weekend days

($\mathbf{R}_t = (R_t^{Sat}, R_t^{Sun})$), when the opportunity costs of firesetting are lower (and motivations higher) (Gill et al. 1987; Prestemon and Butry 2005; Prestemon et al. 2012); and a subvector of variables describing the occurrence of an election ($\mathbf{E}_{j,t}$), $\mathbf{E}_{j,t} = (E_{j,t}^c, E_{j,t+15}^c, E_{j,t+30}^c, E_{j,t+45}^c, E_{j,t}^s, E_{j,t}^d)$, where $E_{j,t}^c$ is a dummy variable =1 during the next 15 days (also called campaign days) in advance of an election, 0 otherwise; $E_{j,t}^s$ is a dummy variable =1 during the single silence day prior to an election, 0 otherwise; and $E_{j,t}^d$ is =1 for the single day of the election. Note that the *t*+15, *t*+30, and *t*+45 leads of the election campaign days dummy variable are intended to capture the potentially long criminogenic temporal footprint of elections. Also, note that the last lead only occurs to lead 52 days, which covers only the last week of the 52-day statutorily required number of days from the announcement of an election and the election's actual occurrence. The motivated offender vector is therefore $\mathbf{Y}_{j,t} = (P_{j,t}, G_{j,t}, U_t, H_t, \mathbf{R}_t, \mathbf{E}_{j,t})$.

The vector $\mathbf{W}_{j,t}$, the number of suitable targets in location *j* in period *t*, described as a function of available flammable fuels, which can be quantified by a vector of meteorological variables ($\mathbf{Z}_{j,t}$), consisting of minimum daily relative humidity ($Z_{j,t}^1$), total daily precipitation ($Z_{j,t}^2$), the Keetch-Byram Drought Index (Keetch and Byram 1968) ($Z_{j,t}^3$), the Fire Weather Index (Fosberg 1978) ($Z_{j,t}^4$), daily maximum hourly wind speed ($Z_{j,t}^5$), and maximum daily temperature ($Z_{j,t}^6$) (e.g., Prestemon et al. 2012), so that $\mathbf{Z}_{j,t} = (Z_{j,t}^1, Z_{j,t}^2, Z_{j,t}^3, Z_{j,t}^4, Z_{j,t}^5 Z_{j,t}^6)$. It also includes seasonal variation in firesetting (e.g., Fuller 1996), as a set of 11 month dummy variables, $\mathbf{DM}_t = (DM_t^{Jan}, DM_t^{Feb}, \ldots, DM_t^{Nov})$, with the effect of December included in the intercept. The suitable targets vector is $\mathbf{W}_{j,t} = (\mathbf{Z}_{j,t}, \mathbf{DM}t)$.

Finally, vectors of variables indexing capable guardianship in location *j* in period *t* are measured by successive lags of arrests for intentional firesetting (of either motivation, since we lack specific information about the motivations of arrestees). We include running sums of the count of arrests lagged *t*-1 to *t*-365 and *t*-366 to *t*-545 in location *j*, so that $\mathbf{A}_{j,t-l} = (\sum_{l=1}^{365} A_{j,t-l}, \sum_{l=366}^{545} A_{j,t-l})$. Recognizing the recommendations about choice of buffer distances (Phillips 2011), we use a proportionate size criterion, with buffer radii at 10, 15, and 25 kilometers. These radii are also justified by observed firesetting behavior by serial arsonists in Spain (Sotoca Plaza 2016). Variables on the neighboring arrests were also generated using running sums, these occurring in three successively more distant "donuts" around the municipality: 10, 15, and 25 km, with vector $\mathbf{A}_{k,t-l} = (\mathbf{A}_{k,t}^{5km}, \mathbf{A}_{k,t}^{15km}, \mathbf{A}_{k,t}^{25km})$, $\mathbf{A}_{k,t}^{5km} = (\sum_{l=1}^{365} A_{k,t-l}^{5km}, \sum_{l=366}^{545} A_{k,t-l}^{5km})$, $\mathbf{A}_{k,t}^{15km} = (\sum_{l=1}^{365} A_{k,t-l}^{15km}, \sum_{l=366}^{545} A_{k,t-l}^{15km})$, $\mathbf{A}_{k,t}^{25km} = (\sum_{l=1}^{365} A_{k,t-l}^{25km}, \sum_{l=366}^{545} A_{k,t-l}^{25km})$.

The number of intentional wildfires is hypothesized to follow a count process, distributed as Poisson but with a variance σ^2 that is a function of its expected value (μ_j) and a scale parameter, α, i.e., a negative binomial model (Cameron and Trivedi 1998, p. 70), $\sigma_j = \mu_j + \alpha {\mu_j}^2$. Adding the time subscript (*t*), indicating the motivation *m*, and consolidating the independent variables shown in (1) into a single vector ($\mathbf{X}_{j,t}$) and a conforming vector of parameters ($\boldsymbol{\beta}^m$), we have:

$$\mu_{j,t}^m = \exp\,(\mathbf{X}'_{j,t}\boldsymbol{\beta}^m) \tag{2}$$

Equation (2) assumes that all spatial units have independently and identically distributed errors, a situation not likely to be met. Therefore, a fixed effects process is considered, allowing for variations across municipalities in the average rates of intentional wildfires due to time invariant unobservable factors. Therefore, equation (2) is augmented to include indicator parameters representing locations, a *j*-dimensional vector $\mathbf{D}^m$:

$$\mu_{j,t}^{m} = \exp\left(\mathbf{X}'_{j,t}\boldsymbol{\beta}^{m} + \mathbf{D}_{j}^{m}\right) \tag{3}$$

The likelihood function for equation (3) is

$$\ln L(\alpha^{m}, \boldsymbol{\beta}^{m}, \mathbf{D}^{m}) = \sum_{j=1}^{N}\left\{\left(\sum_{t=1}^{T}\ln\left(t + \alpha^{m^{-1}}\right)\right) - \ln I_{j,t}^{m}! - \left(I_{j,t}^{m} + \alpha^{m^{-1}}\right)\ln\left(1 + \alpha^{m}\exp\left(\mathbf{X}'_{j,t}\boldsymbol{\beta}^{m} + D_{j}^{m}\right)\right) + I_{j,t}^{m}\ln\alpha^{m} + I_{j,t}^{m}\left(\mathbf{X}'_{j,t}\boldsymbol{\beta}^{m} + D_{j}^{m}\right)\right\} \tag{4}$$

A random effects version of this same general model for cross-sectional data is also available, but in the interest of brevity, we do not show its specification or likelihood equation.

To account for temporal autoregressivity in intentional wildfires, we adopt a method recommended by Zeger and Qaqish (1988) and elaborated in Cameron and Trivedi (1998, p. 239-240). Their approach entails specifying two variables constructed from lags of the dependent variable:

$$\begin{aligned} I_{j,t-1}^{m**} &= I_{j,t-1}^{m} \text{ and } \mathrm{d}_{j,t}^{m} = 0,\ I_{j,t-1}^{m} > 0, \\ I_{j,t-1}^{m**} &= 1 \text{ and } \mathrm{d}_{j,t}^{m} = 1,\ I_{j,t-1}^{m} = 0 \end{aligned} \tag{5}$$

Lags of the variables specified in (5) accommodate higher orders of autoregression in the count process. In estimation, $\mathbf{X}'_{j,t}\boldsymbol{\beta}^{m}$ can be augmented to include *r*-dimensional subvectors $\mathbf{I}_{t-r}^{m**}$ and $\mathbf{d}_{t-r+1}^{m}$, where *r* is the order of autoregression. Coefficients on the elements of $\mathbf{I}_{t-r}^{m**}$ are estimates of autoregressive components, and those in $\mathbf{d}_{t-r+1}^{m}$ rescale the effect of the lagged dependent variable in cases in which $I_{j,t-1}^{m} = 0$.

The presence of significant autoregression in the dependent variable implies that there are short-run and long-run effects of a change in the elements of **X** besides $\mathbf{I}_{t-r}^{m**}$ and $\mathbf{d}_{t-r+1}^{m}$. The long-run effect of any non-autoregressive variable *i* in **X** on I^m is calculated as $\hat{\beta}_i^{m,LR} = \hat{\beta}_i^m/(1-\sum_{u=1}^{r}\hat{\beta}_u^m)$, where $\hat{\beta}_u^m$ is the estimated autoregressive component *u* (*u*=1,…, *r*), quantified as the coefficient of I_{t-u}^{m**} from the estimate of (3) or (4), augmented by $\mathbf{I}_{t-r}^{m**}$ and $\mathbf{d}_{t-r+1}^{m}$.

Following Leamer (1983), and with examples of the importance of his recommendations (e.g., Levine and Renelt 1992), it has now become standard practice in many economics journals to test inferences across multiple specifications in order to evaluate whether these inferences are robust to specification. We follow this practice by testing for the effects of arrests, elections, and other hypothesized variables using several specifications that carry with them different assumptions about the intentional wildfire data generation process. In addition to a fixed effects negative binomial and a random effects negative binomial, we model intentional wildfires using four alternative estimators, which each make differing assumptions about the intentional wildfire data generating process. These include (i) a pooled (across municipalities) negative binomial model that controls for municipality-level differences in error variance, (ii) a pooled negative binomial (NB) model with municipality indicators that also controls for municipality-level differences in error variance, and two specifications that recognize potential zero-inflation in the count of intentional wildfires: (iii) a pooled zero-inflated negative binomial (ZINB) model that controls for municipality-level differences in error variance, and (iv) a pooled ZINB with municipality indicators that also controls for municipality-level differences in error variance.

Evaluation of the magnitudes of the effects of the variables of interest—arrests and elections—were done with a counterfactual analysis. In the case of arrests, we can quantify the long-run average effects of an arrest by, say, simulating an increase the number of arrests by 1% and observing how the predicted counts of intentional wildfires changes, or $\tau^{m,A} = \sum_{t=0}^{T}\sum_{j=0}^{J}(\hat{I}_{j,t}^{m,Ajt=Ajt*} - \hat{I}_{j,t}^{m,Ajt=Ajt})$. Arrest effects are measured with an elasticity. To evaluate the nonmarginal effects of elections, we compare the predicted counts of intentional wildfires with the counts predicted with all election dummy variables set to zero, $\tau^{m,E} = \sum_{t=0}^{T}\sum_{j=0}^{J}(\hat{I}_{j,t}^{m,Ejt=0} - \hat{I}_{j,t}^{m,Ejt=Ejt})$. Election effects measured as changes in total numbers and percent.

Given that our data are spatially arranged, maximum likelihood estimation of any of our model specifications could produce spatially autocorrelated residuals deriving from omitted spatially correlated factors (e.g., Dormann et al. 2007), which would have the effect of attenuating standard errors. While more complex methods of bounding of our model coefficient estimates do exist (e.g., Hall 1985; Liu and Singh 1992), and these could be tried in future studies to uncover any residual spatial autocorrelation remaining, Monte Carlo bootstrapped generation of the effects size confidence limits provides a view of the statistical significances of the overall effects of arrests and elections, regardless of any possible standard error attenuation in model estimates.

Data and estimation

We assembled data on total daily counts of reported ignitions of agricultural ($I_{j,t}^{m=agricultural}$) and non-agricultural intentional wildfires ($I_{j,t}^{m=non-agricultural}$) in Galicia, covering a 16-year period between 1 January 1999, and 31 December 2014. The spatial unit of observation is the municipality (Figure 1), of which there are 313. The wildfire database (General Statistics of Forest Fires compiled by the Spanish Forest Service) contains observations on 108,527 fires over the 16-year span, burning 431,956 ha. Data on each wildfire include a general cause attribution (lightning, negligence and accidents, intentional, reignition, and unknown), the majority of which (81%) are classified as intentional. Intentional wildfires are further subcategorized by "motivation," i.e., as either agriculturally related (i.e., started by farmers to eliminate brush and agricultural debris) or non-agricultural. In our econometric estimates of the fixed effects, random effects, and zero-inflated negative binomial models, separate equations are reported for each motivation (m = agricultural, non-agricultural) as well as their sum, as mentioned.

Population data were obtained from municipal administrative records. The population figure was divided by the area of the municipality ($P_{j,t}$), with daily changes computed by interpolation. As a proxy of income level ($G_{j,t}$), we used the change in the average income declared in annual income tax per year in the municipality, which was provided by the Spanish Tax Agency, and deflated with the annual average of the Consumer Price Index (IPC) by province (base year 2016); daily values were found by interpolation. The unemployment rate ($U_{j,t}$) was the change in the average 30-day centered average rate calculated from monthly municipality unemployment data, with daily changes computed by interpolation. Collected from the Public Employment Service, it was the seasonally adjusted number of unemployed persons aged 16 to 64, divided by the total population); daily values were found by interpolation.

Holidays (H_t) were official days recognized across Spain and those official days recognized across Galicia only. Data on the meteorological variables for each municipality ($\mathbf{Z}_{j,t}$) were based on a network of weather stations distributed across Galicia; the center of each municipality was used as the reference location for computing distances to each weather station, and values were generated with an inverse-distance weighted spatial averaging process.

Daily data on the numbers of arrests, $\mathbf{A}_{j,t}$, are employed as daily sums of arrests made by the Guardia Civil (gendarmerie) and local police service. Neighborhood arrest data, $\mathbf{A}_{k,t} = \left(\mathbf{A}_{k,t}^{5km}, \mathbf{A}_{k,t}^{15km}, \mathbf{A}_{k,t}^{25km}\right)$ and their temporal lags were generated for three circular buffers around the centroid of the municipality. Centroid distance was determined using a 1:25,000 map of municipalities obtained from the Galician Territorial Information System. Note that buffers for municipalities on the edges of the region can extend into the Atlantic Ocean, where there are no arrests, and into Portugal and neighboring regions of Spain, where no consistent daily data are available on arrests. To avoid potential biases, the sample of municipalities used was limited to those that did not have a regional border with the rest of Spain or an international border with Portugal within the outermost ring of neighborhood arrests of the municipality. Municipalities with buffers that reached into the ocean were kept in the sample.

Finally, elections variables are defined as representing the various phases of the election cycle of any of four possible levels: local (council), regional, national, or European parliamentary. Due to their individual infrequency, identification of the effects of the different levels of elections was usually not possible (likelihood maximization failed) if separately modeled, compelling their aggregation.

In all models, we attempted to specify autoregressive orders as high as five, but it was clear upon estimation that high orders prevented many longer order autoregressive specifications from converging. Given the convergence problems, we limited our models to control for only first-order autocorrelation.

The long lags of arrest data (back to t-545) effectively shortened the usable dataset by 1.5 years, meaning that parameter estimates cover mid-2000 through December 2014. Effects are reported for the short-run, which ignores the autoregressive effect of wildfires within a municipality, and the long-run, which incorporates the autoregressive effect. When estimating separate agricultural and non-agricultural intentional fire models, we included the lagged count and lagged zero count indicators (equation 5) of both categories. When calculating the long-run effects of elections and arrests, we used only the lagged count of the own type (i.e., the coefficient on agricultural intentional lagged fires was used to make the long-run adjustment, and the same for the non-agricultural category).

Maximum likelihood estimation of some of the alternative specifications of the intentional fire count models failed when the sample of municipalities included those with fewer than 200 intentional wildfires, probably a result of lack of model parameter identification. To allow for greater comparability in model results and effect size estimates, models were estimated only for the 113 municipalities with at least 200 total intentional wildfires over the time span. Data sources and descriptions of variables included in models are shown in Table 1.

Results

Equation estimates

Equation estimates for the fixed effects negative binomial models and the random effects negative binomial models are shown in Tables 2 and 3, respectively. Estimates with alternative estimators are available from the authors. Summary estimation statistics indicate that the model specifications are significant compared to constant-only model estimates, as measured by the Wald Statistic, distributed $\text{Chi}^2(i)$. For total, agricultural, and non-agricultural fires, the random effects NB specification (allowing for differences in variances across municipalities) is preferred over a pooled NB specification with municipality-level heteroscedasticity, according to a Chi^2

statistic and according to the significance of the dispersion parameters, which were all significant at $\alpha \leq 0.01$.

Model estimates indicate that nearly all included variables are statistically significant at $\alpha \leq 0.01$. Nearly all variables intended to account for suitable targets are highly statistically significant in all model estimates, including relative humidity, precipitation, wind speed, maximum daily temperature, the Keetch-Byram Drought Index, and the Fire Weather Index. Forest area, however, a measure of aggregate fuels available as ignition targets, is positively related to intentional fires, in total and then also for agricultural intentional wildfires, though not significant for non-agricultural intentional fires.

Variables intended to account for temporal variations in the number of motivated offenders are typically also highly statistically significant in all models. Month indicators quantify a seasonal pattern in firesetting for both rural agricultural (possibly related to field preparation) and non-agricultural activities (possibly related to hunting), and they are nearly all highly statistically significantly different from the reference month of firesetting (December), with peak rates observed in February, March, and April, an ebb in June and July, more fires in August, and then somewhat more in autumn. In all model estimates, variables controlling for lower opportunity costs for carrying out firesetting at the daily time scale on non-work days, indicators of Saturdays, Sundays, and holidays, are statistically significant at $\alpha \leq 0.01$ and positively signed, as expected (e.g., Prestemon and Butry 2005; Prestemon et al. 2012). Population density generally is associated with more intentional firesetting, as expected (more motivated firesetters per unit area), in most of the model estimates. Unemployment, modeled as the change in rate, meant to capture how the opportunity cost of carrying out and getting caught setting intentional fires affects the number of motivated offenders, is usually not statistically significant. The rate of change in real personal income, expected to be negatively related to the numbers of motivated offenders due to the higher opportunity costs of being caught intentionally igniting wildfires, demonstrates unexpected sign differences between intentional agricultural and non-agricultural firesetting. In the fixed effects NB specifications (Table 2), agricultural intentional fires respond negatively to income changes, as expected, while non-agricultural respond positively, counter to expectations (and also positively for total intentional fires). In the random effects NB specification (Table 3), agricultural fires have the expected negative sign on income changes, while not significant for non-agricultural fires and having an unexpectedly positive association with the total number of intentional fires.

Variables intended to model spatial and temporal variations in capable guardianship are nearly all highly significant explainers of variation in intentional firesetting. Arrests at all lag orders evaluated are strongly statistically significant ($\alpha \leq 0.01$) and negatively signed. These results are found for within the municipality and at progressively greater spatiotemporal lags around the municipality, and they are found for both the fixed effects and random effects NB specifications. The negative signs on the spatiotemporal lags of arrests support a hypothesis that arrests do not simply displace intentional fires in space or time; rather, they work to decrease occurrence of such fires for long periods and across broad geographical areas near the arrests, presumably by increasing perceived capable guardianship both within the municipality and in surrounding municipalities.

The modeled effects of elections, hypothesized to measure both the numbers of motivated offenders (because elections encourage political protest fires) and the numbers of capable guardians (shifting policing intensities), are consistently associated with intentional fire counts across all motivations and model specifications. Results reveal the temporal pattern of election-associated firesetting. First, election days either do not have greater numbers of intentional fires (agricultural in particular) or have fewer such fires (non-agricultural). Second, Silence Day has a pronounced higher rate of intentional firesetting for total, agricultural, and non-agricultural intentional fires. Third, campaign day periods have generally no effects or positive effects on intentional firesetting. For both the fixed effects NB (Table 2) and the random effects NB, higher firesetting rates are found in the 32 to 46 days and the 47 to 52 days prior to the election day but not in the month just prior to the election.

Finally, we find that for total, agricultural, and non-agricultural intentional wildfires, intentional wildfires are positively first-order autocorrelated at the daily time step. Findings are similar for all model specifications, with all parameter estimates significant at $\alpha \leq 0.01$. For total intentional fires, the coefficient on the intentional fire count from day t-1, is 0.22 in both the fixed and random effects specifications. For agricultural intentional fires, the coefficient is 0.25 on its own type in both specifications and 0.045 and 0.046 on the non-agricultural fire count day t-1 in the fixed effects and random effects NB specifications, respectively. For non-agricultural intentional fires, the coefficient is 0.21 on its own type and 0.12 on the agricultural fire count from day t-1, and the values are the same in both the fixed effects and random Effects NB specifications. In all specifications and fire types, coefficients controlling for zero fires in the previous day are negative, as expected, which shows that a count of zero fires in day t-1 leads to fewer intentional fires in day t.

Effect size estimates

Summary effects of elections and arrests on total, agricultural, and non-agricultural intentional wildfires are shown in Tables 4-6. The results are arranged according to the fixed effects NB, random effects NB, and the four alternative estimators. The intentional wildfires in the 113 municipalities analyzed, over the time span of our sample, represented about 77% of all intentional, 84% of all agricultural intentional, and 74% of all non-agricultural intentional fires that occurred in the municipalities whose 25 km arrest buffers did not cross into Portugal or the rest of Spain. If our equations were to be applied as well to the municipalities in Galicia meeting the buffer restrictions but having fewer than 200 intentional fires (1999-2014), the simulated changes in total fire counts would be larger and the percentage changes would be different than those shown in Tables 4-6.

Effect sizes of arrests and of elections for the modeled 113 municipalities are statistically different from zero at $\alpha \leq 0.01$ in all specifications except for one alternative estimator, the Pooled ZINB with municipality indicators and municipality level heteroscedasticity for non-agricultural intentional wildfires, for which election effect sizes are significant at $\alpha \leq 0.05$.

Across all specifications and model versions, arrests are associated with a reduction in the counts of intentional wildfires in the municipality. Arrests in the previous 545 days prior to day *t* reduce wildfire occurrences in the municipality in day *t*. For the total of intentional wildfires (Table 4), the short-run elasticity of an arrest—i.e., the percent change in the number of intentional fires given a 1% increase in the number of arrests is -0.93 and -0.92 in the fixed effects and random effects NB specifications, respectively and ranges from -0.29 to -0.51 among the four alternative estimators. Long-run effects, which account for the autoregressive component in the intentional wildfire count process, are larger, at -1.20 and -1.18 for the fixed effects and random effects NB specifications, respectively, and range from -0.46 to -0.73 among the alternative estimators. For agricultural intentional fires (Table 5), the effects of arrests are substantially larger. Short-run effects are -1.68 and -1.67 and long-run effects are -2.25 and -2.24

in the fixed effects and random effects NB specifications, respectively. For the alternative estimators, short-run effects range from an elasticity of -0.24 to -0.43 and long-run effects -0.43 to -0.66. Non-agricultural intentional fires are more inelastically related to arrests but still demonstrate more elastic responses to arrests when compared to agricultural intentional fires (Table 6). Arrest elasticities in the short-run range from -0.94 and -1.13 and in the long-run from -1.19 to -1.43 in the fixed effects and random effects NB specifications, respectively. Alternative estimators produce elasticity estimates from -0.32 to -0.54 in the short-run and -0.48 to -0.78 in the long run.

Across all types of intentional fires and all specifications, the net effect of an election in Galicia is to increase the number of intentional wildfires. As shown in Tables 4-6, and as we did for arrests, we assessed the overall impacts of elections on firesetting in both the short-run and the long-run. Tables 4-6 report the simulated effects of setting all election indicator variables to zero and quantifying the simulated counterfactual of no elections during the duration of our estimation dataset (mid-2000 to December 2014).

Consistent with the positive signs on many of the election indicator variables (Tables 2 and 3), the effect of simulating no elections is to reduce the total number and also the number of agricultural and non-agricultural intentional wildfires. For the sum of agricultural and non-agricultural models (Table 4), short-run effects are -1330 and -1328 in the short-run for the fixed effects and random effects NB specifications, and -1649 in the long-run for both specifications. Short-run effects among the alternative estimators range from -498 to -796 and long-run effects from -687 to -1240. In percentage terms, the simulated effect of not having elections would be to reduce the total number of intentional fires by -3.33% in the long-run according to the fixed effects and random effects NB specifications, and from -1.35% to -2.44% among the alternative estimators.

Effects of elections on agricultural fires (Table 5) are similar in magnitude but larger in percentage terms, compared to those quantified by the total intentional fire models. This finding indicates that aggregating both agricultural and non-agricultural intentional fires in a single model likely produces downwardly biased parameter estimates—i.e., aggregation bias. As shown in Table 5, both the fixed effects and random effects NB specifications produce nearly identical short- and long-run simulated reductions in the expected fires. In the long-run, the change in the number of intentional agricultural fires is about -1630, corresponding to -7.88%. Alternative estimators had long-run effects ranging from -327 to -623 and percent changes from -1.38 to -2.14.

For non-agricultural intentional fires, effects of simulated no-elections counterfactuals (Table 6) are also larger in percentage terms than found in the total intentional fire count changes (Table 4). For the fixed effects NB specification, the long-run effect is -1986, or -5.78%. For the random effects NB specification, the corresponding values are -934 and -2.79%. For the alternative estimators, the long-run effects range from -386 to -778, changes corresponding to -1.12% to -2.25%.

Combining the effects reported in Tables 5 and 6, we can quantify the total effect more accurately than by using the total intentional fire model effects shown in Table 4. For example, if we sum the fixed effects NB specification long-run effect of -1630 for the agricultural fires and -1986 for the non-agricultural fires, we have -3616, a change of about -7.18% in the number of total intentional wildfires, calculated over mid-2000 through end-2014. For the random effects NB specifications in Tables 5 and 6, the summed effects are -2565 and -5.19%. These long-run values can be compared to the corresponding values shown in Table 4, which are -1649 and -3.33%, which are produced by both the fixed effects and random effects NB specification effects.

Discussion and conclusions

This study set out to evaluate whether arrests affect the spatiotemporal distribution of intentionally set future wildfires, which can provide insights regarding the overall effectiveness of law enforcement efforts. We used geographically and chronologically precise data on fire occurrence and arrests in Galicia to examine, based on the theoretical framework of RA theory (Cohen and Felson 1979), whether wildfire numbers changed locally and in nearby locations following intentional wildfire related arrests.

Statistical model estimates indicate that variables connected to the three elements of RA theory were related to intentional firesetting, generally, in the ways expected. The numbers of motivated offenders were measured by population density, indicators of election periods, indicators of seasons when agricultural areas are being prepared for planting, and variables quantifying the opportunity costs of carrying out or potentially being arrested for intentional firesetting. The only motivated offenders variables that were not signed in the direction expected or significant in our empirical specifications were unemployment and income. For suitable targets, quantified by variables measuring aggregate fuels quantities (forest area) and amenable weather conditions (fire weather indices and several direct meteorological variables), the RA framing was supported by the signs and the significances of parameter estimates. Primary measures of the presence or absence of capable guardians were the temporal and spatiotemporal lags of recent arrests for intentional firesetting and election variables. Arrests were hypothesized to communicate to motivated intentional firesetters that their likelihood of arrest, and hence capable guardianship, has increased. We found that all temporal and spatiotemporal lags of arrests were highly statistically significantly and negatively related to counts of intentional wildfires at all spatiotemporal lags tested, a finding expected given either perceived broad scale increases in capable guardianship or overall decreases in motivated offenders. Election variables were also generally statistically significantly related to intentional firesetting, adding additional weight to the usefulness of the RA-based theoretical framework for this crime process.

Our study demonstrated that our hypotheses regarding potential spatiotemporal displacement of the effects of arrests and the effects of elections were robust to assumptions about the form of the wildfire data generation process. The fixed effects and random effects NB specifications generated broadly similar effect sizes and strong statistical significances. Although also strongly statistically significant and signed in the same way, estimates generated by arguably less well-fitting specifications, including the pooled NB models and the zero-inflated pooled NB models, produced effect sizes that were somewhat smaller.

The separate estimation of agricultural and non-agricultural intentional fire models highlighted the importance of separately modeling intentional fires by motivation of the firesetter. One benefit of the disaggregation was to reveal that the effects of arrests and of elections were larger than when all motivations were combined and modeled together. Separate estimation produced summed total effect sizes of arrests and of elections that were more than twice as large as the effect sizes produced by models that combined them. In future research, analysts would be counseled to model intentional fires at as fine a level of motivational aggregation as can be supported by the data.

Another benefit of the separate modeling was to reveal the differences in the magnitudes of the effects of driving variables on intentional firesetting of different motivations. A comparison of the results reveals that agricultural fires are more elastically related to an arrest. The greater sensitivity of agricultural intentional firesetting to arrests could be considered in the context of the profit-maximizing behavior of an agriculturist, who decides when and whether to use fire as a land-clearing method, including in times when such fire use is forbidden. Because agriculturists depend on agriculture for their livelihoods, it makes sense that they would be particularly responsive to conditions under which their expected incomes would be reduced by criminal sanctions. The differential arrest sensitivity of these two classes of motivation also implies that attention to their relative prevalence in a landscape could be considered when making decisions on how to optimally deploy law enforcement resources across an agricultural-wildland gradient: in agricultural intentional wildfire dominated landscapes, arrests would yield larger overall wildfire reductions than in places where non-agriculturally motivated firesetting predominates.

Although law enforcement resource allocations are guided by a variety of tradeoff considerations, including attention to non-wildfire crimes and reducing fear of crime (Weisburd and Eck 2004), our results could aid policy makers in assessing the consequences of police resource tradeoffs. And while not explicitly designed to forecast future intentional wildfire locations in the form modeled by Prestemon et al. (2012), our results are potentially useful for implementing Problem Oriented Policing (e.g., Goldstein 1979; Weisburd et al. 2015) and intelligence-led policing (e.g., Ratcliffe 2016) strategies that focus on anticipating spatiotemporal crime concentrations and on identifying and incapacitating repeat offenders (e.g., serial arsonists). In the particular case of Galicia, by characterizing how wildfires are concentrated in space-time and how the effects of arrests (and elections) alter such concentrations, our findings could help police organizations identify proactive steps to reducing intentional wildfire occurrences.

Our results do not provide support for the existence of spatial displacement caused by one measure of law enforcement effort, the number of arrests (e.g., Eck 1993), a finding consistent with other research on the effects of stepped up policing (Bowers et al. 2011; Telep et al. 2014). Further study would be required to detect the effects of more distant (> 25 km) and longer-lasting (> 1.5 years) spatiotemporal lags of arrests.

The arrest findings are possibly related to altered adjustments in perceptions of increased capable guardianship in response to local arrests. In Spain, fewer than 10% of intentional wildfire cases result in a conviction and sanction, a relatively low rate of clearance compared to other crime types, due to evidentiary difficulties (Fiscalía General del Estado Medio Ambiente y Urbanismo 2016). Furthermore, most convictions typically lead to fines, not imprisonment, the latter outcome primarily reserved for repeat offenders (e.g., González et al. 2017). Given the low probability of imprisonment for firesetting, we conclude that arrests primarily result in a perceived increase in capable guardianship rather than a reduction in motivated offenders. Although we could identify no published research on how arrests for intentional firesetting are communicated across a population of motivated firesetters, such as through traditional and social media, the precise mechanisms of information diffusion responsible for changes in perceived capable guardianship is an area worthy of additional study.

Our analyses also offer evidence that political activity is associated with increased overall rates of intentional firesetting. The signs and significances of the various election indicator variables highlight the potentially complex behavioral patterns among political protest actors in

Galicia. Furthermore, the identified temporal pattern of the effects of elections on intentional firesetting provides a roadmap for new strategies for allocating law enforcement resources during the election cycle. Four to seven weeks before an election, intentional firesetting is elevated. This is followed by a four-week window approaching Silence Day and Election Day of normal rates of firesetting, a Silence Day spike in intentional fires, and then low to normal rates on Election Day. The measured overall (net) effect of the entire election cycle, however, is to boost intentional firesetting. The fixed effects and random effects NB specifications implied that a no-election counterfactual had up to 7% fewer agriculturally based intentional fires and 5% fewer non-agriculturally based intentional fires, with smaller impacts measured by the alternative estimators. Consistent with the conclusions of Kull (2002) and Skouras and Christodoulakis (2014), the larger agricultural effect of election cycles may occur because farmers perceive that capable guardianship is lower—i.e., the likelihood of being caught and sanctioned is lower—during (the earlier phase of) campaigns, when law enforcement may direct more resources toward addressing spikes in other forms of social disruption. Likewise, election periods might increase the numbers of prospective offenders who are motivated to focus politicians' and the media's attention to ongoing public-government disagreements about how forests are managed (Hovardas 2014). Ramos and Sanz (2018) provide statistical evidence that large accidental wildfires may affect election outcomes favoring the incumbent party in municipal elections in Spain. If the conflict hypothesis is a partial explanation for the statistical evidence that our models provide, then policy makers could use our results to help evaluate the potential benefits of efforts to reduce such conflicts.

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Conflicts of Interest

The authors declare that they have no conflicts of interest.

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Table 1. Data sources and summary statistics of model variables for municipalities with at least 200 intentional wildfires, 1999-2014

Variable Name	Units	Reporting frequency	Min	Max	Mean	Source
Intentional wildfire ignitions	count	daily	0	16	0.0778	General Statistics of Forest Fires, Spanish Ministry of Agriculture, and Fisheries, Food and Environment.
Agricultural intentional wildfires	count	daily	0	10	0.0252	General Statistics of Forest Fires, Spanish Ministry of Agriculture, and Fisheries, Food and Environment.
Non-agricultural intentional wildfires	count	daily	0	12	0.0526	General Statistics of Forest Fires, Spanish Ministry of Agriculture, and Fisheries, Food and Environment.
Lightning ignitions	count	daily	0	4	0.0006	General Statistics of Forest Fires, Spanish Ministry of Agriculture, and Fisheries, Food and Environment.
Other ignitions	count	daily	0	6	0.0155	General Statistics of Forest Fires, Spanish Ministry of Agriculture, and Fisheries, Food and Environment.
Relative Humidity	percent	daily	18	100	59	Spanish Meteorological Agency (AEMet)
Precipitation	mm	daily	0	265	4.24	Spanish Meteorological Agency (AEMet)
Keetch-Byram Drought Index	index number	daily	0	702	83	Spanish Meteorological Agency (AEMet)
Fire Weather Index	index number	daily	0.36	38.52	7.78	Spanish Meteorological Agency (AEMet)
Average Daily Windspeed	m/s	daily	0.05	10.66	2.69	Spanish Meteorological Agency (AEMet)
Average Daily Maximum Temperature	degrees C	daily	-3.68	40.76	17.50	Spanish Meteorological Agency (AEMet)
Arrests	count	daily	0	6	0.0010	Galician Guardia Civil
Population less than 16 years	persons	annual	19	41707	2056	Galician Statistical Institute (Instituto Galego de Estatistica)
Population less than 16 years Density	persons/ ha	annual	0.001	8.163	0.262	Galician Statistical Institute (Instituto Galego de Estatistica)
Land area	ha		3236	37882	11508	Fourth Spanish Forest Inventory
Average Income	Thousand euros	annual	4.35	29.59	11.31	Galician Statistical Institute (Instituto Galego de Estatistica)
Unemployment rate	percent	monthly	1.00	16.40	5.84	Galician Statistical Institute (Instituto Galego de Estatistica)
Forest Land Area	ha		1050	23848	7500	Third Spanish Forest Inventory/Fourth Spanish Forest Inventory

Table 2. Fixed effects negative binomial model estimates for all intentional, agricultural intentional, and non-agricultural intentional wildfires in Galicia

	All Intentional	Agricultural Intentional	Non-Agricultural Intentional
Relative humidity	-0.58 ***	-1.39 ***	0.028
Precipitation	-1.68 ***	-1.72 ***	-1.71 ***
Keetch-Byram Drought Index	0.0033 ***	0.0033 ***	0.0028 ***
Fire Weather Index	0.21 ***	0.16 ***	0.26 ***
Wind Speed	-0.65 ***	-0.48 ***	-0.80 ***
Maximum Daily Temperature	-0.021 ***	-0.029 ***	-0.017 ***
January	-0.20 ***	-0.062	-0.288 ***
February	0.93 ***	1.04 ***	0.83 ***
March	1.11 ***	1.27 ***	1.00 ***
April	0.79 ***	0.92 ***	0.65 ***
May	0.20 ***	0.32 ***	0.061
June	0.44 ***	0.50 ***	0.42 ***
July	0.52 ***	0.47 ***	0.58 ***
August	0.74 ***	0.81 ***	0.78 ***
September	0.75 ***	0.83 ***	0.75 ***
October	0.17 ***	0.38 ***	0.10 *
November	0.07	0.12	0.045
Arrests,t-1 to t-365	-0.016 ***	-0.028 ***	-0.0035
Arrests,t-365 to t-545	-0.0092 ***	-0.017 ***	-0.0042 **
Neighbor 1 Arrests,t-1 to t-365	-0.013 ***	-0.012 ***	-0.0103 ***
Neighbor 2 Arrests,t-1 to t-365	-0.022 ***	-0.017 ***	-0.021 ***
Neighbor 3 Arrests,t-1 to t-365	-0.021 ***	-0.019 ***	-0.018 ***
Neighbor 1 Arrests,t-365 to t-545	-0.008 ***	-0.012 ***	-0.0071 ***
Neighbor 2 Arrests,t-365 to t-545	-0.0074 ***	-0.007 ***	-0.009 ***
Neighbor 3 Arrests,t-365 to t-545	-0.011 ***	-0.008 ***	-0.012 ***
Saturday	0.15 ***	0.14 ***	0.15 ***
Sunday	0.22 ***	0.21 ***	0.21 ***
Holiday	0.16 ***	0.11 ***	0.18 ***
Campaign Day (2 to 16 Days Before)	-0.019	-0.0034	0.0055
Campaign Day (17 to 31 Days Before)	0.0036	0.0896 *	0.037
Campaign Day (32 to 46 Days Before)	0.13 ***	0.13 ***	0.25 ***
Campaign Day (47 to 52 Days Before)	0.074 ***	0.11 ***	0.08 ***
Silence Day, t	0.57 ***	0.62 ***	0.55 ***
Election Day, t	-0.13	0.045	-0.253 *
Population Density Change, all Persons, t	0.0017 ***	0.002 ***	Not included
Forest Area, t	0.0094 **	0.038 ***	-0.0039
Unemployment Rate Change, t	-0.082	4.34	2.01
Real Total Personal Income Change, t	5.13 ***	-4.32 **	9.57 ***
Intentional Fire Count,t-1	0.22 ***		

	All Intentional	Agricultural Intentional	Non-Agricultural Intentional
No Intentional Fires,t-1	-1.15 ***		
Non-Ag Intentional Fire Count,t-1		0.045 ***	0.21 ***
No Non-Ag Intentional Fires,t-1		-0.65 ***	-1.22 ***
Ag Intentional Fire Count,t-1		0.25 ***	0.12 ***
No Ag Intentional Fires,t-1		-1.13 ***	-0.57 ***
Constant	-0.67 ***	-0.22	-0.72 ***
Observations	576,074	576,074	617,319
Model Significance (Wald Test)	59,074 ***	20,658 ***	45,263 ***

*** indicates significantly different from zero at $\alpha \leq 0.01$, ** at $\alpha \leq 0.05$, * at $\alpha \leq 0.10$

Table 3. Random effects negative binomial model estimates for all intentional, agricultural intentional, and non-agricultural intentional wildfires in Galicia

	All Intentional	Agricultural Intentional	Non-Agricultural Intentional
Relative humidity	-0.58 ***	-1.4 ***	-0.26
Precipitation	-1.68 ***	-1.72 ***	-1.67 ***
Keetch-Byram Drought Index	0.0033 ***	0.0033 ***	0.0034 ***
Fire Weather Index	0.21 ***	0.16 ***	0.24 ***
Wind Speed	-0.65 ***	-0.48 ***	-0.73 ***
Maximum Daily Temperature	-0.022 ***	-0.029 ***	-0.019 ***
January	-0.20 ***	-0.060	-0.27 ***
February	0.93 ***	1.05 ***	0.85 ***
March	1.11 ***	1.27 ***	0.98 ***
April	0.79 ***	0.92 ***	0.68 ***
May	0.21 ***	0.32 ***	0.080
June	0.44 ***	0.5 ***	0.35 ***
July	0.53 ***	0.48 ***	0.47 ***
August	0.74 ***	0.82 ***	0.64 ***
September	0.75 ***	0.84 ***	0.66 ***
October	0.17 ***	0.38 ***	0.012
November	0.067	0.12	0.0011
Arrests,t-1 to t-365	-0.016 ***	-0.028 ***	-0.011 ***
Arrests,t-365 to t-545	-0.009 ***	-0.018 ***	-0.007 ***
Neighbor 1 Arrests,t-1 to t-365	-0.013 ***	-0.012 ***	-0.014 ***
Neighbor 2 Arrests,t-1 to t-365	-0.022 ***	-0.016 ***	-0.025 ***
Neighbor 3 Arrests,t-1 to t-365	-0.021 ***	-0.019 ***	-0.022 ***
Neighbor 1 Arrests,t-365 to t-545	-0.008 ***	-0.011 ***	-0.0069 ***
Neighbor 2 Arrests,t-365 to t-545	-0.007 ***	-0.0068 ***	-0.0081 ***
Neighbor 3 Arrests,t-365 to t-545	-0.011 ***	-0.0081 ***	-0.013 ***
Saturday	0.15 ***	0.14 ***	0.15 ***
Sunday	0.22 ***	0.21 ***	0.23 ***
Holiday	0.16 ***	0.11 ***	0.18 ***
Campaign Day (2 to 16 Days Before)	-0.019	-0.0035	-0.029
Campaign Day (17 to 31 Days Before)	0.0038	0.090 *	-0.044
Campaign Day (32 to 46 Days Before)	0.13 ***	0.13 ***	0.14 ***
Campaign Day (47 to 52 Days Before)	0.074 ***	0.11 ***	0.058 ***
Silence Day, t	0.57 ***	0.62 ***	0.51 ***
Election Day, t	-0.14	0.045	-0.28 **
Population Density Change, all Persons, t	0.0016 ***	0.0020 ***	0.0015 ***
Forest Area, t	0.0134 ***	0.040 ***	0.0024
Unemployment Rate Change, t	-0.12	4.304	-3.262
Real Total Personal Income Change, t	5.17 ***	-4.32 **	-0.26
Intentional Fire Count,t-1	0.22 ***		

	All Intentional	Agricultural Intentional	Non-Agricultural Intentional
No Intentional Fires,t-1	-1.16 ***		
Non-Ag Intentional Fire Count,t-1		0.046 ***	0.21 ***
No Non-Ag Intentional Fires,t-1		-0.65 ***	-1.18 ***
Ag Intentional Fire Count,t-1		0.25 ***	0.12 ***
No Ag Intentional Fires,t-1		-1.13 ***	-0.54 ***
Constant	-0.69 ***	-0.23	-0.54 ***
Natural log of overdispersion parameter alpha (rho)	3.22 ***	2.30 ***	3.29 ***
Natural log of overdispersion parameter alpha (sigma)	2.29 ***	0.60 ***	2.23 ***
Observations	576,074	576,074	576,074
Model Significance (Wald Test)	59,707 ***	20,713 ***	44,031 ***
Likelihood Ratio Test vs. Pooled	4,285 ***	6,183 ***	2,606 ***

*** indicates significantly different from zero at $\alpha \leq 0.01$, ** at $\alpha \leq 0.05$, * at $\alpha \leq 0.10$

Table 4. Effects of elections and arrests on all intentional wildfires in Galicia

	Modeled Number of Fires, if No Elections, Short-run	Modeled Number of Fires, if No Elections, Long-run	Percent change in Fires if No Elections, Short-run	Percent change in fires, if No Elections, Long-run	Elasticity of Intentional Fires with respect to Arrests, Short-run	Elasticity of Intentional Fires with respect to Arrests, Long-run
Fixed Effects NB	-1330 ***	-1649 ***	-2.69 ***	-3.33 ***	-0.93 ***	-1.20 ***
Random Effects NB	-1328 ***	-1649 ***	-2.68 ***	-3.33 ***	-0.92 ***	-1.18 ***
Alternative Estimators						
Pooled NB, Clustered Errors	-749 ***	-1135 ***	-1.51 ***	-2.29 ***	-0.36 ***	-0.60 ***
Pooled NB, Municipality Indicators, Clustered Errors	-665 ***	-903 ***	-1.34 ***	-1.83 ***	-0.51 ***	-0.73 ***
Pooled ZINB, Clustered Errors	-796 ***	-1240 ***	-1.56 ***	-2.44 ***	-0.29 ***	-0.46 ***
Pooled ZINB, Municipality Indicators, Clustered Errors	-498 ***	-687 ***	-0.98 ***	-1.35 ***	-0.41 ***	-0.57 ***

*** indicates significantly different from zero at $\alpha \leq 0.01$

Table 5. Effects of elections and arrests on agricultural intentional wildfires in Galicia

	Modeled Number of Fires, if No Elections, Short-run	Modeled Number of Fires, if No Elections, Long-run	Percent change in Fires if No Elections, Short-run	Percent change in fires, if No Elections, Long-run	Elasticity of Ag. Intentional Fires with respect to Arrests, Short-run	Elasticity of Ag. Intentional Fires with respect to Arrests, Long-run
Fixed Effects NB	-1264 ***	-1630 ***	-7.88 ***	-10.16 ***	-1.68 ***	-2.25 ***
Random Effects NB	-1263 ***	-1631 ***	-7.88 ***	-10.17 ***	-1.67 ***	-2.24 ***
Alternative Estimators						
Pooled NB, Clustered Errors	-344 ***	-594 ***	-2.14 ***	-3.70 ***	-0.29 ***	-0.57 ***
Pooled NB, Municipality Indicators, Clustered Errors	-306 ***	-435 ***	-1.91 ***	-2.71 ***	-0.43 ***	-0.66 ***
Pooled ZINB, Clustered Errors	-342 ***	-623 ***	-2.05 ***	-3.74 ***	-0.24 ***	-0.43 ***
Pooled ZINB, Municipality Indicators, Clustered Errors	-228 ***	-327 ***	-1.38 ***	-1.99 ***	-0.36 ***	-0.52 ***

*** indicates significantly different from zero at $\alpha \leq 0.01$

Table 6. Effects of elections and arrests on non-agricultural intentional wildfires in Galicia

	Modeled Number of Fires, if No Elections, Short-run	Modeled Number of Fires, if No Elections, Long-run	Percent change in Fires if No Elections, Short-run	Percent change in fires, if No Elections, Long-run	Elasticity of Non-Ag. Intentional Fires with respect to Arrests, Short-run	Elasticity of Non-Ag. Intentional Fires with respect to Arrests, Long-run
Fixed Effects NB	-1625 ***	-1986 ***	-4.73 ***	-5.78 ***	-0.94 ***	-1.19 ***
Random Effects NB	-768 ***	-934 ***	-2.30 ***	-2.79 ***	-1.13 ***	-1.43 ***
Alternative Estimators						
Pooled NB, Clustered Errors	-428 ***	-619 ***	-1.28 ***	-1.85 ***	-0.39 ***	-0.62 ***
Pooled NB, Municipality Indicators, Clustered Errors	-362 ***	-486 ***	-1.08 ***	-1.45 ***	-0.54 ***	-0.78 ***
Pooled ZINB, Clustered Errors	-513 ***	-778 ***	-1.48 ***	-2.25 ***	-0.32 ***	-0.48 ***
Pooled ZINB, Municipality Indicators, Clustered Errors	-278 **	-386 **	-0.80 **	-1.12 **	-0.44 ***	-0.60 ***

*** indicates significantly different from zero at $\alpha \leq 0.01$, ** at $\alpha \leq 0.05$