Great Expectations: Urban Development in 17th Century London*

Jacob Field†
Cambridge University

Yi Jie Gwee‡
Ministry of Trade and Industry, Singapore

Di Song Tan§
Ministry of Trade and Industry, Singapore

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Abstract

The development of cities often involves the rejuvenation or replacement of existing structures. By reducing the opportunity cost of waiting to rebuild to zero, disasters can eliminate development frictions and bring about higher quality structures in the rebuilding process. In addition, the simultaneous rebuilding after a disaster allows property owners to experience stronger cross-building spillovers which would encourage further upgrades of nearby buildings. Nevertheless, these are not sufficient to guarantee higher quality buildings. This is because individuals' investment decisions also depend on their expectations of what others will do. In this paper, we examine both of these issues using the 1666 Great Fire of London as a natural experiment. First, using a difference-in-differences (DiD) strategy, we show evidence that the Fire was able to free parishes within London from the constraints of their existing durable structures and move them to a new equilibrium involving higher quality structures. Second, using DiD and an IV strategy, we find evidence that legal rulings arising from the Fire Court – a court specially set up by the English Parliament to hear rebuilding disputes – were able to anchor expectations and in so doing, helped to facilitate the development of London.

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†Affiliated researcher with the Cambridge Group for the History of Population & Social Structure. Email: jfpfield@me.com.

‡Corresponding author. Email: y.gwee@columbia.edu

§Email: d.tan2@lse.ac.uk

1 Introduction

The development of cities often involves the rejuvenation or replacement of existing structures. However, history, in the form of the sunk cost of existing durable structures, often serves as an impediment to urban development. In every period, property owners face a trade-off between receiving rent from the existing building or incurring a cost to tear down the building and rebuilding it. As a result, they often wait long periods of time for their building to depreciate before embarking on upgrading. Furthermore, without some gain to being the first to upgrade their property, property owners may rationally wait for others to upgrade first. In theory, by reducing the opportunity cost of waiting to rebuild to zero, disasters (such as a Fire) can eliminate these frictions and bring about higher quality structures. In addition, the simultaneous rebuilding after a disaster would allow property owners to experience stronger cross-building spillovers. As described by Hornbeck and Keniston (2017), this "virtuous circle" of cross-plot externalities result in building upgrades encouraging further upgrades of nearby buildings.

Nevertheless, the opportunity cost of waiting to rebuild falling to zero coupled with the prospects of stronger cross-building spillovers, are not sufficient to guarantee higher quality buildings. This is because individuals' investment decisions also depend on their expectations of what others will do. For example, if a city (or more generally, an area) is growing, then individuals will expect other individuals to build higher quality buildings. By contrast, if the expectations are that the area is in decline, then individuals may not even rebuild or may invest at a lower quality since they expect other individuals to do the same.

In this paper, we examine both of these issues using the 1666 Great Fire of London as a natural experiment. Our research questions are as follows. First, we examine whether the Fire was able to free parishes[1] within London from the constraints of their existing durable structures and move them to a new equilibrium involving higher quality structures. In line with the historical context, we define the quality of structures based on the number of hearths in the property. While the first research question that we examine is similar to the papers on the 1872 Boston fire by Hornbeck and Keniston (2017) and the 1906 San Francisco fire by Siodla (2015), our second research question departs from these paper. In particular, we study what anchors individuals' expectations of what others will do and how this can consequently facilitate the development of cities. We find evidence that legal rulings arising from the Fire Court – a court specially set up by the English Parliament to hear rebuilding disputes – were able to anchor expectations

[1] Parishes were administrative units within a city that played a role in both civil and ecclesiastical matters.

and in so doing, helped to facilitate the development of London.

For the first part of the paper, to examine whether the removal of development frictions through the Fire resulted in higher quality structures being rebuilt, we employ a difference-in-differences (DiD) strategy. The DiD strategy exploits both the cross-sectional and time-series variations arising from the Fire. The time-series variation comes from the timing before and after the Fire which was exogenous. The cross-sectional variation arises because different parishes in London were affected differently by the Fire. For example, some parishes were burned whereas some parishes did not experience any damage from the Fire at all. A null effect from our regression would suggest that there were no frictions to upgrading before the Fire – the quality of properties was optimal. By contrast, a positive effect suggests the presence of upgrading frictions which the Fire effectively removed.

Using our DiD strategy, we find that a few years after the Fire, burned parishes experienced a highly statistically significant increase in the number of hearths per property compared to unburned parishes. In addition, the effect varied with the level of damage. Parishes which were more badly damaged saw a highly statistically significant increase in the number of hearths per property compared to parishes which were less damaged. Finally, the effect was biggest for parishes whose neighboring parishes were all burned compared to parishes whose neighboring parishes were not all burned.

The result from the first part of the paper suggests that individuals had positive expectations that others will be rebuilding at a high quality. Nevertheless, it does not tell us what is driving these expectations. Therefore, in the second part of the paper, we examine the role of legal rulings in driving expectations. In 17th century England, tenants were legally obliged to rebuild in the event of any disasters which damaged the property, even if it was not their fault. However, the Fire took place amidst a plague and war – an unprecedented joint occurrence of events. To expedite the rebuilding of London, the English Parliament established the Fire Court.

The second part of the paper begins with a model that shows that legal rulings affect expectations because they affect the bargaining between landlords and tenants who do not go to Court. This is because their outside options are based on the Fire Court's initial rulings. For our empirical strategy, we turn once again to a DiD strategy. Just as before, the time-series variation comes from the timing before and after the Fire. However, the cross-sectional variation now arises because different parishes experienced different Fire Court rulings. For example, some parishes saw a disproportionate number of initial cases where the Fire Court voided the existing contracts between the landlord

and tenant and consequently assigned the rebuilding to the landlord. This is what we refer to as pragmatic rulings. Voiding the contract means that both the landlord and tenant surrender their contracts. This allows both parties to negotiate a new contract with each other or other parties.

Our regression results show that parishes with a greater share of pragmatic rulings had more hearths per property compared to parishes where there was a lower share of cases with pragmatic rulings. In addition, because only a very small proportion of properties in each parish went to the Fire Court, our results suggest that the rulings of these few cases had an outsized effect on the quality of other buildings in the parish.[2]

While we have included a number of time varying parish-level controls in our regression, a threat to identification in the DiD strategy is that we might not have controlled for all possible confounders. As a result, the change in the number of hearths may be related to changes in parish level characteristics that are not due to the Fire Court rulings – a violation of the parallel trend assumption. Therefore, we augment our DiD strategy with an instrumental variable (IV) strategy.

Our IV DiD strategy exploits the fact that at the parish level, Fire Court judging panels that have different political alignments (i.e., whether they were predominantly Royalists or Parliamentarians) were assigned to the cases. The 1666 Great Fire took place in the midst of the Second Dutch War (1665-1667) and the Great Plague which began in 1665. King Charles II was relying on loans from London and its wealthiest citizens to finance the war. The destruction of the customs house, wharves and more than 13,000 buildings caused a significant drop in royal revenue. The King had a vested interest for London to be rebuilt quickly. Therefore, judging panels that consisted predominantly of Royalists (i.e., more aligned with the King) were more likely to decree pragmatic rulings so as to facilitate the rebuilding of London. As a result, we can use the composition of the judging panels as an instrument for the share of cases in the parish that had pragmatic rulings. This gives us exogenous variations in legal rulings for each parish.

We find that the results from our IV analysis re-affirm our DiD results – legal rulings can indeed anchor expectations and help to facilitate the rebuilding process. To the best of our knowledge, while there are theoretical papers such as Cooter (1998), Basu (2000), McAdams (2000, 2005), Myerson (2004) and Hadfield and Weingast (2012) that examine how legal institutions can affect expectations and hence the behavior of individuals, there are relatively fewer empirical papers that provide causal evidence of this.

[2]Based on the initial cases, the average proportion of properties in each parish that went to the Fire Court was 6%.

In examining how expectations affect the behavior of economic agents, our paper is related to Krugman (1991) and Rauch (1993). In addition, our paper is related to how cities recover from major shocks and whether they move to a new equilibrium. Beginning with Davis and Weinstein (2002), there has been an extensive literature that examines whether long-run city size is robust to temporary shocks. These shocks include wars and bombing (Davis and Weinstein (2002) and Miguel and Roland (2011)), natural or man-made disasters (Siodla (2015) and Hornbeck and Keniston (2017)), political events (Redding, Sturm and Wolf (2011) and Michaels and Rauch (2018)), technology (Bleakley and Lin (2012)) and even diseases (Jedwab, Johnson and Koyama (2019)). Our paper provides evidence of how the Great Fire of London freed London from the constraints of history and enabled it to move to a new equilibrium with more hearths per property.

By addressing how legal rulings contribute to the development of cities, our paper is related to the literature on the economic consequences of legal origins. This literature shows how legal origins affect particular legal rules and these in turn affect economic outcomes such as growth, financial development, property rights and contract enforcement. Examples of these studies include Acemoglu, Johnson and Robinson (2001), Djankov, La Porta, Lopezde-Silanes and Shleifer (2003), La Porta, Lopez-de-Silanes, Pop-Eleches and Shleifer (2004), La Porta, Lopez-de-Silanes and Shleifer (2008) and Dell (2010). In using judging panels that consisted predominantly of Royalist as our instrument in our IV analysis, our paper is also related to North and Weingast (1989), Acemoglu, Johnson and Robinson (2005), Jha (2015) and Angelucci, Meraglia and Voigtländer (2020). These papers examine the tensions between Parliamentarians and Royalists during various times in English history (e.g., the English Civil War (1642-1651) and the Glorious Revolution (1688)) and show how these affected the development of institutions that facilitated growth in England.

Finally, our paper is also related to the historical literature on the impact of the Great Fire of London. Field (2008) notes that the 1666 Great Fire of London is such an iconic moment in the history of London that the contemporary media frequently used the phrase "The Second Great Fire" to describe the London Blitz during World War II. While the 1666 Fire has been extensively studied by historians (e.g., Reddaway (1940), Porter (1996) and Field (2018)) and even legal scholars (e.g., Tidmarsh (2016)), our paper contributes to this largely qualitative literature by providing a quantitative analysis on the impact of the Great Fire of London.

The rest of the paper proceeds as follows. Section 2 presents the historical background of the 1666 Great Fire of London. Section 3 discusses the novel data sources that we

use for our analysis. Section 4 examines the effect that the Fire had on the quality of properties that were rebuilt. Section 5 presents our main contribution which is that legal rulings anchored individuals' expectations of what others will do and this consequently facilitated the development of parishes within London. We conclude in Section 6.

2 Historical Background: The 1666 Great Fire of London

This section draws extensively from Reddaway (1940), Porter (1996), Field (2008), Tidmarsh (2016) and Field (2018). The Great Fire of London began on September 2, 1666, in a bakery on Pudding Lane in the City of London (see Figure 1). The City of London covers an area of 2.8 km^2 or 1.1 $miles^2$ within London and was home to about one sixth of London's inhabitants. The structure of the city made it easy for the Fire to spread. Streets, lanes and alleys were narrow and buildings were made from timber. In addition, the upper floors of houses often cantilevered over the pathways below. This meant that the top floors on one side of the street nearly touched those on the other side, making it easy for the Fire to spread. The Fire lasted for three and a half days and destroyed approximately 13,200 buildings in the City of London. An estimated 70,000 out of 80,000 inhabitants living in the City of London lost their homes. Figure 1 shows the geographical spread of the Fire over the three and a half days.

Figure 1: Spread of the Fire

Source: Museum of London (2016); with authors' edits to include the location of Pudding Lane

Tidmarsh (2016) notes that despite the urgency to rebuild London, there were significant challenges. At the time of the Fire, the institution of fire insurance had not yet

developed. Instead, the common practice was that leases had a covenant that obligated the tenant, regardless of whether the tenant was at fault, to repair or rebuild the premises in the event of disasters or wars. This created substantial challenges for both the tenants and landlords. For the tenants, there was the issue of fairness in whether they should bear the full cost of rebuilding. Many tenants could not afford to rebuild. Moreover, tenants who had a short time left on their lease had little incentive to rebuild. As for the landlords, there were long delays and huge cost in bringing disputes to the common-law courts. Even if the case was brought before the common-law courts, the powers of these courts were constrained by the existing tenancy agreements. As a result, the judges could not calibrate or void the existing contracts to achieve the best incentives for the parties to rebuild. Furthermore, due to the existing tenancy agreements, landlords could not prematurely re-enter the leased premises in order to facilitate reconstruction.

In order to expedite the rebuilding of London, the English Parliament established the Fire Court to adjudicate between landlords and tenants as to who would bear the burden of rebuilding (Fire of London Disputes Act 1666). The bill was passed in the House of Lords on January 23, 1667. A few days later, on January 31, 1667, the House of Commons assented to the bill.[3] Tidmarsh (2016) notes that the Fire Court heard a total of 1,585 cases. Some cases involved more than one property so the 1,585 cases understate the extent of the Court's work. As mandated by the Fire Court legislation, each case was heard by a panel of at least three judges. The judges were given the power to void existing contracts and decide the details of the new contracts (e.g., who rebuilds, new rent and length of the tenancy agreement, etc.). The typical process when a case is brought to the Fire Court is that the judges would first try to mediate and get the tenant and landlord to come to an agreement. In the event that the parties are unable to come to an agreement, the Court will then make a ruling which is legally binding.[4]

In concluding our discussion about the historical background of the Great Fire of 1666,

[3]The year of the enactment of the statute was listed as 1666 even though the bill was passed in the House of Lords on January 23, 1667. This is because based on the calendar that was used during that era, the new year began on Lady Day (March 25).

[4]Reddaway (1940), Porter (1996) and Tidmarsh (2016) note that besides setting up the Fire Court, the Parliament of England also put in place other legislation and measures to facilitate the rebuilding of the city. There were new building regulations to limit damage from subsequent fires (buildings must be made of brick, be of a minimum size, not exceed a certain height and must not cantilever over the streets). To determine boundaries and settle disputes among neighbors, a survey system was put in place. Since properties were taken for public purposes (e.g., widened streets), there was a formal channel to value property. To finance the reconstruction of public buildings, a tax on coal was introduced. To ensure that property owners rebuilt within a reasonable time, sanctions were meted out if this was not done. There were provisions requiring owners to share rebuilding costs that benefited multiple properties (e.g., party walls). Regulations on the price and quality of raw materials used for the rebuilding were implemented. Incentives were given to encourage skilled craftsmen to come to London to help with the rebuilding.

we would like to highlight that there were previously other fires in London that also resulted in substantial damage. For example, Richardson (2001) notes that the Great Fire of 1133 damaged St Paul's, St Bride's, London Bridge and properties as far east as Aldgate. Another example was the Great Fire of 1212 which began at Southwark, destroyed the church and spread to London Bridge. Legal issues surrounding the responsibility of the tenant to rebuild would have also existed back then. Why then was the Fire Court only set up after the 1666 Fire and not earlier? The existing literature is surprisingly silent on this.

One reason could be that while previous Great Fires caused substantial damage, the damage to property from the 1666 Fire was arguably the greatest (see for example Garrioch (2016)). London had grown substantially since the 12th and 13th century. Therefore, even if the entire city was almost destroyed due to the 1133 Fire, by 1666 the size of the city would have been far larger. Nevertheless, due to the lack of data (most of the evidence is qualitative), it remains debatable whether the damage from the 1666 Fire was the greatest. For example, Garrioch (2016) notes that about 3,000 people died in the 1212 Fire, far more than the eight people that was estimated to have died due to the 1666 Fire.[5]

Therefore, we think that the main reason was due to the joint occurrence of war, plague and Fire – a combination of events that was absent in the previous Great Fires. The Great Plague which began in 1665 resulted in the death of almost a quarter of London's population within 18 months. This means that there was now a huge excess supply of vacant properties which vastly increased the bargaining power of tenants. The King could wait for the landlords and tenants to reach a bargained outcome. For example, whether the landlord contributes to the rebuilding or changes the terms of the tenancy contract even though by law the tenant has to rebuild. However, given the ongoing Second Dutch War (1665 to 1667), King Charles II simply could not wait for this to play out. Tidmarsh (2016) argues that the King was relying on loans and taxes from London and its wealthiest citizens to finance the war. The destruction of the customs house, wharves and buildings caused a significant drop in royal revenue from custom and hearth taxes. The Fire Court was therefore a way to expedite reaching a somewhat equitable outcome. It gives the landlord class some portion of what prior precedent would suggest but it also tilts things sufficiently toward tenants to mirror the shift in bargaining power owing to

[5]Despite the destruction, the largest estimate of deaths directly due to the Fire was eight. This is a shockingly small number and historians such as Field (2018) have offered a number of explanations. First, the incineration of bodies in the Fire meant that corpses could not be recovered and so the death records are underestimates. Second, the Fire took place over three and a half days. This gave sufficient time for people to evacuate. Third, historians postulate that the relatively tight-knit nature of the neighborhoods meant that there was help and assistance for the vulnerable.

the plague.[6]

3 Data

Urban investment. In line with how the value of a property was assessed in 17th century London, we measure quality by the number of hearths that are in each property before and after the Fire. This information is available from the historical manuscripts of the hearth tax assessment records that are held at The National Archives, United Kingdom.

According to the University of Roehampton, Centre for Hearth Tax Research,[7] the hearth tax was introduced in England and Wales in 1662 to provide a regular source of income for King Charles II who was the newly restored monarch. Parliament had estimated that the King required an annual income of £1.2 million. However, by 1661, there was a shortfall of £300,000 and it was hoped that the hearth tax would make up for this. The hearth tax was essentially a property tax on dwellings graded according to the number of fireplaces in the property. The tax was paid in two equal installments at Michaelmas (September, 29) and Lady Day (March, 25) by the occupier. If the property was vacant, the landlord paid the tax. In order to administer the tax, a list of householders was compiled and this formed the hearth tax assessment records.

Our pre-Fire hearth data comes from two sources. First, we use the full records from the 1666 London and Middlesex hearth tax, along with portions of the 1663 and 1664 documents that have been cleaned and digitized by the London Hearth Tax project.[8] Since the hearth tax was collected twice a year in March and September, the 1666 records are based on the March collection which took place before the Fire in September. Second, we supplement this with the 1664/1665 Southwark hearth tax records that come from the assessment for Surrey. The Southwark data was manually transcribed by Field (2008) for his history PhD thesis.[9]

As for the post-Fire hearth data, we rely on the records from the 1675 London and

[6] We would like to thank Don Davis for helping us to sharpen this argument.

[7] https://www.roehampton.ac.uk/research-centres/centre-for-hearth-tax-research/

[8] In June 2007, the London Hearth Tax project was formed to systematically analyze and digitize the hearth tax records. The project united the expertise of the British Academy Hearth Tax Project, the Centre for Hearth Tax Research (University of Roehampton), Birkbeck College (University of London), and the Centre for Metropolitan History (Institute of Historical Research). In 2011, the full records from the 1666 London and Middlesex hearth tax, along with portions of the 1663 and 1664 documents, were published electronically via British History Online at https://www.british-history.ac.uk/london-hearth-tax/london-mddx/1666.

[9] While the data from Southwark is undated, Field (2008) notes that they are most certainly from the period between 1664 and 1665.

Middlesex hearth tax records as well as the 1673 Surrey (Southwark) hearth tax records. These data were also manually transcribed by Field (2008).[10]

The unit of geography for our analysis is at the parish level. Due to the differences in the scope and range of the hearth tax assessments, some parishes only appear in the pre-Fire records while others only appear in the post-Fire records. In our regressions, we only use data from the parishes that appear in both the pre- and post-Fire records. Table 1 shows the summary statistics of the hearth tax data which we use in our regressions.

Table 1: Summary statistics (Hearth Tax data)

	Mean	SD	Min	Max	N
Number of hearths (pre-Fire)	3.83	3.79	0	193	44,724
Number of hearths (post-Fire)	4.33	3.36	0	135	35,006
Number of parishes	.	.	.	.	70

Some might question whether the number of hearths is a reasonable way to measure the quality of the building. We believe that it is reasonable for a few reasons. First, unlike assessed values or market values, the number of hearths is an objective measure and is not based on a valuation. Second, Field (2008) documents that research has shown that there is some correlation between the number of hearths and wealth, as well as occupation. To the extent that the wealthier and those with higher social standing live in higher quality buildings, then we should expect the number of hearths to be a reasonable proxy for the quality of the building.

Details of Fire Court judges. The Fire Court was composed of England's twelve common-law judges. There were three common-law courts (Common Pleas, Kings Bench, and the Exchequer) with four justices appointed to each court. In the years after the Fire, some judges retired or passed away and hence our sample contains fourteen judges and not twelve.

In order to get details about the Fire Court judges, we referred to various books such as the Oxford Dictionary of National Biography (2004) and Sainty (1993). From these sources, we obtained information on the judges. Many seismic political events took place in 17th century England. For example, the English Civil War (1642-1651), the restoration of the monarchy (1660), as well as the Puritans' (English Protestants) con-

[10] Although the London and parts of the Middlesex hearth tax records were presented to Parliament sessions on February 1, 1675, Field (2008) states that a faded note on the manuscript linked it to a collection on 1674. Other parts of the Middlesex records were based on an assessment between 1674 and 1675. The data for Southwark come from an assessment for Surrey that was not dated. However, Field (2008) notes that it is probably associated with a collection in 1673.

tinuous attempts to get the Church of England (established church) to abandon its Roman Catholic practices. Therefore, from these books, we also obtained information on the judges' religious views and their views on the 1660 restoration of the monarchy (i.e., whether they were Royalists or Parliamentarians). We define binary variables for whether the judges were supportive of the restoration of the monarch (Royalists) and whether they were supportive of the established church. We assign the value of 0.5 if the judges had moderate views. In our IV analysis, we use the composition of the judging panels an instrument for the share of initial cases in the parish that had pragmatic rulings.

Table 2 shows us the summary statistics of the Fire Court judges. On average, the judges tend to be slightly pro-restoration of the monarchy and pro-established church. Around 36% of the judges attended Oxford University with the rest attending Cambridge University. The majority of the judges trained at the Inner Temple. Finally, 43% of the Fire Court judges were from the court of the Common Pleas and 29% of them were the respective heads of their common-law courts (i.e., Lord Chief Justice or Lord Chief Baron).

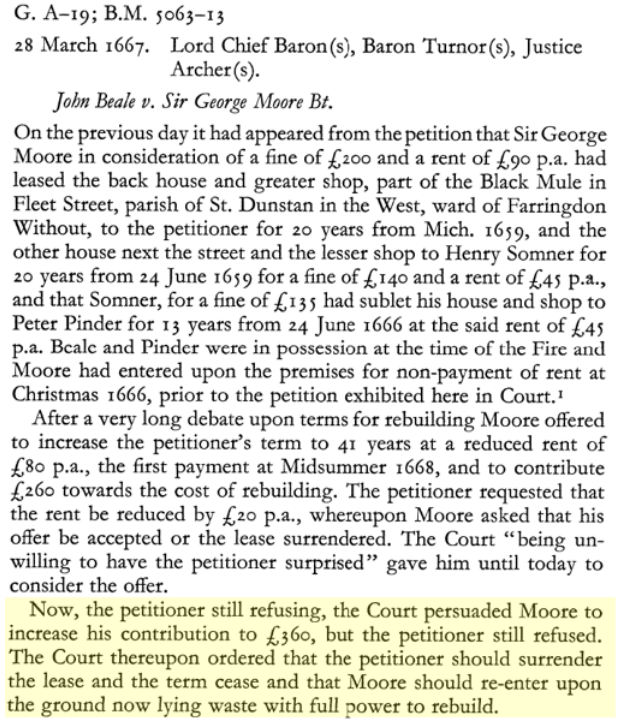
Table 2: Summary statistics (Judges)

	Mean	SD	Min	Max	N
Year of birth	1603.14	6.79	1587	1611	14
Year called to bar	1629.14	6.51	1614	1637	14
Year knighted	1658.71	7.02	1643	1668	14
Pro-restoration of monarchy	.57	.43	0	1	14
Pro-established church	.57	.43	0	1	14
Studied at Oxford University	.36	.5	0	1	14
Served in Grays Inn	.07	.27	0	1	14
Served in Lincolns Inn	.36	.5	0	1	14
Served in Inner Temple	.5	.52	0	1	14
Served in Middle Temple	.07	.27	0	1	14
From Common Pleas	.43	.51	0	1	14
From Kings Bench	.29	.47	0	1	14
From Exchequer	.29	.47	0	1	14
Head of common-law Court	.29	.47	0	1	14
Number of judges	.	.	.	.	14

Details of Fire Court cases. The transcripts of the cases that were heard by the Fire Court were compiled into nine volumes. These records survive up to today and are housed at the London Metropolitan Archives. To commemorate 300 years since the Fire, in 1966, four volumes (volumes A, B, C and D) were calendared (summarized) and converted to modern English by Philip E. Jones. These were subsequently published as two books – Jones (1966) and Jones (1970). The summaries contain extremely detailed

information. For example, they give us details on who the landlords and tenants are, the location of the property, the rent and tenure of the tenancy contract before the Fire, the day that the case was heard by the Fire Court, the judges who heard the case, as well as the new rent and tenure that were decreed by the panel of judges. Figure 2 shows an example of a case summary from the books while Figure C1 shows how some of the case characteristics evolved over time (within the first 716 days).

Figure 2: Example of Fire Court case summary



G. A–19; B.M. 5063–13

28 March 1667. Lord Chief Baron(s), Baron Turnor(s), Justice Archer(s).

John Beale v. Sir George Moore Bt.

On the previous day it had appeared from the petition that Sir George Moore in consideration of a fine of £200 and a rent of £90 p.a. had leased the back house and greater shop, part of the Black Mule in Fleet Street, parish of St. Dunstan in the West, ward of Farringdon Without, to the petitioner for 20 years from Mich. 1659, and the other house next the street and the lesser shop to Henry Somner for 20 years from 24 June 1659 for a fine of £140 and a rent of £45 p.a., and that Somner, for a fine of £135 had sublet his house and shop to Peter Pinder for 13 years from 24 June 1666 at the said rent of £45 p.a. Beale and Pinder were in possession at the time of the Fire and Moore had entered upon the premises for non-payment of rent at Christmas 1666, prior to the petition exhibited here in Court.[1]

After a very long debate upon terms for rebuilding Moore offered to increase the petitioner's term to 41 years at a reduced rent of £80 p.a., the first payment at Midsummer 1668, and to contribute £260 towards the cost of rebuilding. The petitioner requested that the rent be reduced by £20 p.a., whereupon Moore asked that his offer be accepted or the lease surrendered. The Court "being unwilling to have the petitioner surprised" gave him until today to consider the offer.

Now, the petitioner still refusing, the Court persuaded Moore to increase his contribution to £360, but the petitioner still refused. The Court thereupon ordered that the petitioner should surrender the lease and the term cease and that Moore should re-enter upon the ground now lying waste with full power to rebuild.

Source: Jones (1966)

As part of his history PhD thesis, Field (2008) transcribed some of the information associated with the cases in these four volumes into a data set. We augment this data set by transcribing additional information that was not captured by Field (2008). Table 3 shows us the summary statistics of the Fire Court data based on the cases which we have sufficient information. In 13% of the cases in our sample, the Fire Court voided the existing contracts (i.e., both landlord and tenant surrendered the existing contract) and assigned the cost of rebuilding to the landlord. In 1.3% of the cases, the judges altered the existing contracts (i.e., no surrendering) and assigned the rebuilding to the landlord. In 71% of the cases, the judges altered the existing contracts and assigned the rebuilding to the tenant. In 10% of the cases, the the Fire Court voided the existing contracts but decreed the sharing of cost in the rebuilding. Finally, in 5.2% of the cases, the judges

altered the existing contracts but decreed the sharing of cost in the rebuilding. The fine paid is the lump-sum payment made on execution of the lease. For each judging panel, we calculate the share of judges who were supportive of the established church and the share of judges who were supportive of the 1660 restoration of the monarchy (Royalists). On average, in each judging panel, 48% of the judges tend to be supportive of the restoration of the monarchy and 46% tend to be supportive of the established church. This suggests that the judging panels were on average quite moderate in their views.

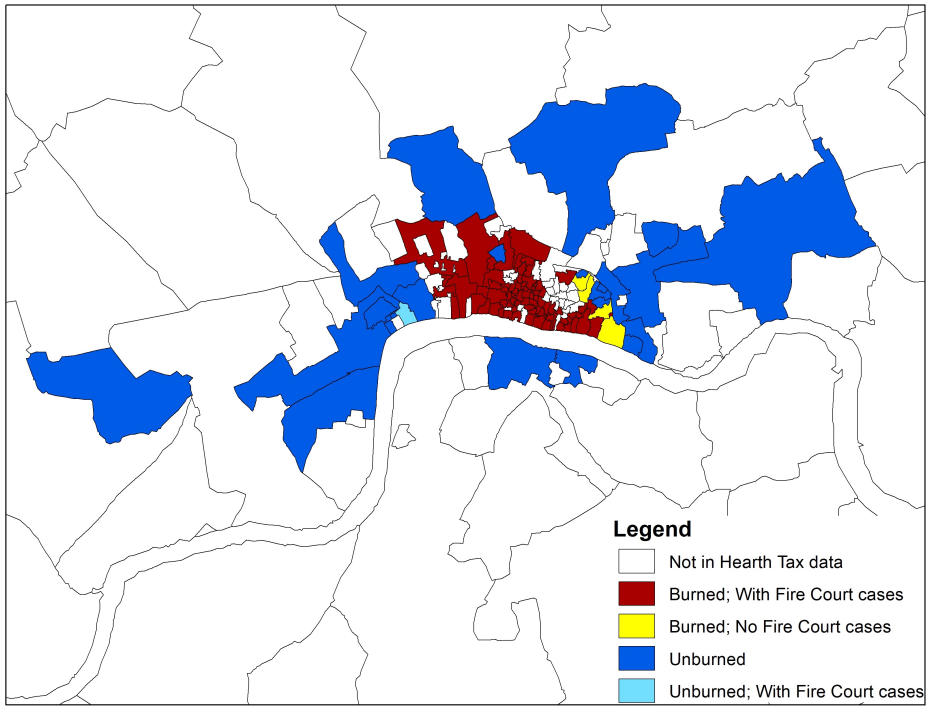
Finally, since we are interested in examining whether legal rulings of the initial cases in each parish can anchor expectations, we do not actually need to observe the rulings of all the cases that went to the Fire Court. Therefore, the four out of nine volumes which have been calendared would suffice for our purposes as these four volumes cover the earlier cases. We refer to these cases from the first four volumes as the "initial" cases.

Table 3: Summary statistics (Fire Court data)

	Mean	SD	Min	Max	N
Both parties surrender: Owner rebuilds	.13	.33	0	1	696
No surrender: Owner rebuilds	.01	.11	0	1	696
No surrender: Tenant rebuilds	.71	.46	0	1	696
Both parties surrender: Cost sharing	.10	.30	0	1	696
No surrender: Cost sharing	.05	.22	0	1	696
Degree of separation from owner	1.18	.52	1	6	696
Start year of tenancy	1655.74	10.23	1591	1666	679
Years left in tenancy	34.75	387.83	0	9996.92	663
Fine paid	69.08	202.61	0	4000	696
Rent per annum	31.29	36.91	0	474	692
Amount spent on improvements	48.46	233.07	0	3000	696
Average pro-monarchy of panel	.48	.22	0	1	696
Average pro-church of panel	.46	.21	0	1	696
Head of common-law Court	.57	.6	0	3	696
Number parishes	.	.	.	.	67
Number cases	.	.	.	.	696

Regression sample. Putting all our data sources together, Figure 3 shows the parishes that are included in our regressions. In the diagram, we label a parish as "burned" as long as any part of it was damaged by the Fire.

Figure 3: Regression sample



Source: Satchell, Kitson, Newton, Shaw-Taylor and Wrigley (2018); with authors' edits

4 The Effect of the Fire

The 1666 Great Fire of London had both quantity and quality effects in the development of London. On the quantity side, the Fire affected the total number of properties and hearths in each parish. As for quality, the Fire affected the number of hearths per property in each parish. In this paper, we focus on the effect that the Fire had on quality. This is because the plague wiped out about a quarter of London's population. Therefore, we should expect fewer properties to be rebuilt in the immediate aftermath since there are now fewer people to house. However, the effect on quality is not clear. In addition, the reduction in the number of properties is consistent with post-Fire regulations that stipulated that properties needed to be of a certain minimum size. Finally, our data end in 1675 (nine years after the Fire) so it could be the case that London had not reached a new stationary state – i.e., it is too early to tell if the number of properties converged to a new steady state. For these reasons, the main focus of our analysis is on quality as opposed to quantity. Nevertheless, in Appendix A, we examine the effect that the Fire had on the total number of properties and hearths in each parish.

4.1 Empirical strategy

To examine the effect of the Fire on the number of hearths per property, we use a DiD empirical strategy:

$$ln\left(Hearths_{ijt}\right) = \alpha_j + \delta PostFire_t + \beta Burned_j \times PostFire_t + \gamma' X_{jt} + \epsilon_{ijt} \quad (1)$$

$ln\left(Hearths_{ijt}\right)$ is the log number of hearths in property i in parish j in period t. The two periods are before the Fire and after the Fire. $Burned_j$ is an indicator variable that denotes whether property i was in a parish that experienced damage from the Fire. $PostFire_t$ is an indicator variable for the period after the Fire. X_{jt} is a vector of controls. Finally, α_j are parish fixed effects. We cluster the standard errors at the parish level. A null effect would suggest that there were no frictions to upgrading before the Fire – the quality of properties was optimal. By contrast, a positive effect suggests the presence of upgrading frictions which the Fire effectively removed.

For those interested in the cross-sectional regressions in each time period, the results are reported in Table D1. In the pre-Fire period, the number of hearths per property in burned versus unburned parishes was statistically indistinguishable.

4.2 Results and discussion

Higher quality structures. Table 4 reports the impact that the Fire had on the number of hearths per property in the burned parishes relative to the unburned parishes. The estimate in column 1 shows that controlling for parish and time fixed effects, burned parishes saw a highly statistically significant increase of around 26.3% more hearths compared to unburned parishes. While in percentage terms this magnitude might seem large, given that the average number of hearths before the Fire was 3.83, this translates to an increase of 1.01 hearths.

There could be concerns that there are other time varying parish-level variables that are driving the results. For example, larger or richer parishes may recover faster from the Fire as they are able to bring together more resources. To address these concerns, in column 2, we include a series of parish controls interacted with $PostFire_t$. These include the number of properties in the parish before the Fire, the share of peers,[11] high-ranking military personnel (i.e., Colonel or Captain) and doctors living in the parish. The estimated effect remains robust to the inclusion of these time varying parish-level controls.

[11] These are Duke, Duchess, Marquess, Marchioness, Earl, Countess, Viscount, Viscountess, Baron, Baroness, Lord, Lady, Sir, Dame and Ambassador.

Next, to control for geographical characteristics, we classified the parishes into broader locations (i.e., abutting the City of London walls, within the walls and outside the walls). In column 3, we show that the results are stable to the inclusion of these broader locations-by-post fixed effects. Finally, we grouped parishes into terciles based on the number of hearths in the parish before the Fire. This is to control for the possibility that there may be persistence in the number of hearths – properties with more hearths will rebuild with more hearths and those with fewer hearths will rebuild with fewer hearths. In column 4, we show that the results are relatively stable even when we include these pre-Fire hearth terciles-by-post fixed effects. Figure C2 shows the binned scatter plot of the results in column 4.

Table 4: Effect of Fire on the number of hearths per property

	(1)	(2)	(3)	(4)
VARIABLES	ln(No. hearths)			
Parish Burned X Post Fire	0.263***	0.239**	0.219*	0.283**
	(0.092)	(0.098)	(0.127)	(0.116)
Observations	77,093	77,093	77,093	77,093
R-squared	0.009	0.010	0.010	0.014
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	70	70	70	70

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Our results show that after the Fire, inhabitants of the parishes constructed more hearths per property. This suggests that there was indeed the presence of substantial frictions that was impeding development. By reducing the opportunity cost of waiting to rebuild to zero and forcing everyone to build at the same time, the Fire freed the parishes from the constraints imposed by their existing durable structures. This consequently spurred development through stronger cross-building spillovers and led to a new equilibrium which involved more hearths per property.

Finally, as our dependent variable has been log transformed, there could be issues of Jensen's inequality. In particular, running the regression with the log transformed dependent variable could result in an opposite treatment effect as compared to if we were to run the regression without taking logs. In Appendix B we provide a discussion about this potential issue and show that we get a positive treatment effect in both the regression without logs and the regression in logs.

Effect varied with the level of damage. A priori, we should expect the effect of the Fire to vary with the level of damage. For example, in the extreme, if the Fire was so small that it only damaged one building, then the Fire would not have been effective in removing rebuilding frictions and there would be no widespread reconstruction.

We use two different approaches to examine such heterogeneous effects. First, we split the $Burned_j \times PostFire_t$ variable into two dummy variables – $SlightlyBurned_j \times PostFire_t$ and $CompletelyBurned_j \times PostFire_t$. As the names suggest, $SlightlyBurned_j$ refers to parishes where less than half of the parish (in terms of geographical area) was burned while $CompletelyBurned_j$ refers to parishes where more than half of the parish was burned. Table 5 reports the results of this heterogeneous treatment effect regression. Across all columns, we see that the effect of the Fire was greater in parishes that were completely burned.

Table 5: Effect of the extent of Fire on the number of hearths per property

	(1)	(2)	(3)	(4)
VARIABLES		ln(No. hearths)		
Parish Completely Burned X Post Fire	0.403***	0.445***	0.471***	0.607***
	(0.064)	(0.089)	(0.073)	(0.102)
Parish Slightly Burned X Post Fire	0.173	0.136	0.144	0.192
	(0.117)	(0.133)	(0.146)	(0.130)
Observations	77,093	77,093	77,093	77,093
R-squared	0.012	0.013	0.013	0.018
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	70	70	70	70

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

The second approach which we adopt is to use whether the church in the parish was damaged as a proxy for the level of destruction in the parish due to the Fire. We think that this is reasonable given that the church was often the center of economic and social life during this period of time. To do this, we run Equation (1) comparing burned parishes where the church was damaged to unburned parishes. In the same regression, we also compare burned parishes where the church was not damaged to unburned parishes. Table 6 reports the results. Across all columns, we see that the effect of the Fire was greater in parishes where the church was also damaged by the Fire.

Table 6: Effect of the church being damaged on the number of hearths per property

VARIABLES	(1)	(2)	(3)	(4)
	ln(No. hearths)			
Parish burned and church damaged X Post Fire	0.393***	0.401***	0.400***	0.485***
	(0.073)	(0.100)	(0.115)	(0.122)
Parish burned but church not damaged X Post Fire	0.210*	0.189	0.190	0.252**
	(0.108)	(0.115)	(0.131)	(0.118)
Observations	77,093	77,093	77,093	77,093
R-squared	0.011	0.011	0.011	0.015
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	70	70	70	70

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Both approaches suggest that the effect of the Fire was greater in parishes where the destruction was more widespread. Nonetheless, some of the positive effect that we find in Table 4, Table 5 and Table 6 could be mechanical. This is because as noted by Field (2008), new houses had to be built according to strict regulations that specified the size and materials used. In addition, given the excess supply of land due to the plague, land was probably cheaper. This could lead to people wanting larger houses with more hearths per house. We discuss how we can rule out such mechanical effects in the next paragraph.

Effect varied with the level of damage in surrounding parishes. To rule out the mechanical effect of larger houses having more hearths, we split the $Burned_j \times PostFire_t$ variable into two dummy variables – $AllNeighorsBurned_j \times PostFire_t$ and $NotAllNeighorsBurned_j \times PostFire_t$ and re-run Equation (1). If the increase in the number of hearths per property is purely due to larger houses, then it should not vary with the level of damage in the surrounding parishes.

Table 7 shows that spatial spillovers matter. Burned parishes that were completely surrounded by other burned parishes experienced building investments that were two to three times higher than burned parishes that were not completely surrounded by burned parishes. This regression shows us strong evidence that the increase in hearths per building is driven by cross-building spillovers and not the mechanical effect of larger properties.

Table 7: Effect of spatial spillovers on the number of hearths per property

VARIABLES	(1)	(2)	(3)	(4)
	ln(No. hearths)			
Not All Neighbors Burned X Post Fire	0.174	0.143	0.145	0.178
	(0.113)	(0.123)	(0.144)	(0.131)
All Neighbors Burned X Post Fire	0.395***	0.410***	0.409***	0.474***
	(0.069)	(0.098)	(0.102)	(0.124)
Observations	77,093	77,093	77,093	77,093
R-squared	0.010	0.011	0.011	0.014
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	70	70	70	70

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

4.3 Robustness checks

Dropping parishes which merged after the Fire. One concern could be that the Fire led to the merging of some parishes and so our results could be driven by these enlarged parishes which might have more resources. In Table D2 we re-run Equation (1) using only parishes that did not merge with other parishes after the Fire. Reassuringly, the coefficient estimates remain very similar to our baseline results.

Using different control groups. There could be concerns that some of the unburned parishes in our control group may not be appropriate for our analysis. To see this, consider a hypothetical unburned parish (parish U) that is surrounded by many burned parishes. Given the destructive nature of the Fire, it is somewhat surprising that parish U did not suffer any damage from the Fire. This could suggest that parish U is fundamentally different from its neighboring parishes that were burned. For example, parish U could have been more wealthy and hence more able to quickly mobilize fire-fighting efforts. It could also be the case that more buildings in parish U were made of bricks as opposed to wood. In addition, parish U could have also pre-empted the spread of the Fire by tearing down buildings that were near to the parishes that were burning. The Fire Court records suggest that this indeed happened in parishes such as St Botolph Bishopsgate and St Mary-le-Strand. Therefore, it might not be suitable to use such parishes as the control group to the burned parishes.

To address this concern, we run separate regressions based on two samples. First, a "nearby" sample which consists of all burned parishes plus unburned parishes that share

a boundary with any burned parishes. The results for this are reported in Table D3. Second, a "further away" sample which consists of all burned parishes and unburned parishes that do not share any boundaries with any burned parishes. The results from this sample are reported in Table D4. The results using both samples are very similar, suggesting that our analysis is not sensitive to the choice of control groups.

Accounting for zeros in the outcome variable. There are 2,637 observations which are recorded as having zero hearths in the property. Taking logs results in these observations dropping out of the regression. Therefore, to account for the zeros in the outcome variables, we adopt two approaches. First, applying the inverse hyperbolic sine transform to hearths. Second, using a Poisson pseudo-likelihood (PPML) regression. The results are reported in Table D5 and Table D6 respectively. While the estimated effects remain positive, the magnitudes are now halved and are imprecisely estimated.

Trimming extreme values of the outcome variable. As another robustness check, we drop the top and bottom one percentile of $ln\left(Hearths_{ijt}\right)$. While the estimated effects are now halved, they remain positive and are mostly statistically significant. The results of this robustness check are reported in Table D7.

Checking for parallel trends. A key assumption of a DiD strategy is that of parallel trends. There are two methods to argue that the number of hearths per property in the burned versus unburned parishes was experiencing the same trends before the Fire. The first method is to rely on the historical context. The historical setting suggests that the Fire spread based on where the wind blew and not due to the economic or social characteristics of the parish. Referring to Figure 1, this seems to indeed have been the case. While the Fire started in Pudding Lane which was in the eastern part of the City of London, contemporaneous reporting by the The London Gazette (1666) notes that due to the "violent Easterly wind", the Fire spread mostly to the west. As a result, almost all the parishes that were damaged were to the west of Pudding Lane.

The wind blowing from the east to the west during the Fire is an important point. This is because Heblich, Trew and Zylberberg (2020) show that in England, the wind usually blows from the west or south-west. Therefore, we can make an argument that whether a parish ended up being burned was unexpected, random and independent of pre-trends.

The second method would be to run a placebo DiD regression to compare the number of hearths per property in the burned versus unburned parishes in the periods before the Fire. We should find no effect if there are parallel trends. Unfortunately, due to

data limitations, we are not able to do so in the most robust manner. This is because the hearth tax was introduced in 1662 and for the pre-Fire period, we only have data from the 1662, 1664, 1665 and 1666 hearth tax records. Due to the differences in the scope and range of the pre-Fire hearth tax assessments, almost all parishes (65 out of 70) appear only in one year. In addition, Table 8 shows that in some years, the data we have were either all for burned or unburned parishes. Therefore, we had to pool all the 1662, 1664, 1665 and 1666 data to form the pre-Fire period in our regressions.

Table 8: Pre-Fire data

Year	Burned parishes in the data	Unburned parishes in the data	Total
1662	16	0	16
1664	0	2	2
1665	0	2	2
1666	38	17	55

To run placebo regressions to test for pre-trends, we would need data for both burned and unburned parishes in at least two pre-Fire years. However, with the exception of 1666, the other three pre-Fire years only consist of data from either burned or unburned parishes. In order to try our best to provide statistical evidence to rule out pre-trends, what we can do is to classify the pre-Fire period into two categories. First, the year 1666 would be period $t-1$ and the years 1662 and 1664 would be $t-2$.[12] This method has some limitations such as assuming that the data in 1664 are very similar to 1662 and the two unburned parishes in 1664 are representative of all the other unburned parishes. Nevertheless, accepting these limitations allows us to run the following placebo regression to test for pre-trends:

$$ln\left(Hearths\,per\,property_{jt}\right) = \alpha_j + \delta PostPlaceboFire_t + \beta Burned_j \times PostPlaceboFire_t + \gamma' X_{jt} + \epsilon_{jt}$$

$PostPlaceboFire_t$ is an indicator variable for the period after a placebo fire. We set this as the period after 1665. Finding a large and statistically significant effect from this phantom event would cast serious doubts on the validity of our identification strategy. If our regression passes the parallel trends test, then we should expect β to be small and statistically insignificant. Table 9 shows that this is indeed what we get when we run this placebo regression, suggesting the absence of pre-trends.

[12] We do not include year 1665 because the two parishes that appear in 1665 are south of the river and are hence very different from the other parishes in the data for the placebo regression.

Table 9: Effect of placebo Fire on the number of hearths per property

VARIABLES	(1) ln(No. Hearths per Property)
Parish Burned X Post Placebo Fire	-0.009
	(0.023)
Observations	40,517
R-squared	0.004
Parish FE	✓
Post FE	✓

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

5 The Effect of Pragmatic Legal Rulings

5.1 Overview

In the previous section, we found that the 1666 Great London Fire resulted in a higher number of hearths per property in burned parishes relative to unburned parishes. While our results suggest that individuals had positive expectations about how much other individuals in their parish would be investing, it does not tell us what is driving these expectations. Therefore, in this section, we examine if legal rulings could be a driver of these expectations.

5.2 Defining pragmatic legal rulings

The Fire Court judges were given the power to completely void existing contracts or alter the terms of these contracts. Voiding the existing contract means that both the landlord and tenant surrender their contracts. This allows both parties to negotiate a new contract with each other or other parties. In addition, voiding the contract does away with the judging panels arbitrarily setting a new rent and lease. By contrast, altering the terms of an existing contract means that the tenant remains the same but the Fire Court decrees a new rent and/or length of lease. In addition, the Fire Court would also decree that either the landlord or tenant rebuilds, or that both parties are to contribute towards the rebuilding. In our paper, we define the voiding of existing contracts and the assigning of the cost of rebuilding to the landlord as pragmatic legal rulings. 12.7% of cases fall into this category.

We define such rulings as pragmatic because they help to facilitate a higher number of hearths per property for the following reasons. First, tenants are likely to be more credit-constrained compared to landlords and are hence more likely to rebuild at a lower quality (i.e., fewer hearths per property). Second, assigning the rebuilding to the land-

lord represents a clear assignment and alignment of property-rights. Third, since the occupant is responsible for paying the hearth tax, if the tenant was assigned the rebuilding, she is likely to rebuild with fewer hearths to reduce her tax burden. For these reasons, assigning the rebuilding to the landlord rather than the tenant facilitates the rebuilding of London.

In theory, we could have expanded our definition of pragmatic rulings to also include cases where the judges altered existing contracts (i.e., did not void the contract) but assigned the rebuilding responsibility to the landlord. However, in such cases the Fire Court's rulings were often multi-dimensional. For example, it could be the case that although the rebuilding responsibility was assigned to the landlord, the judges could have decreed a lower rent. In this instance, the landlord may then choose to rebuild at a lower quality since the rent she is receiving is now lower. In order to circumvent the issue of multi-dimensional rulings, we focus on the most extreme of case outcomes – cases where the Fire Court voided existing contracts and assigned the rebuilding responsibility to the landlord.[13]

5.3 Model: Legal rulings, expectations and investment

How exactly did legal rulings drive expectations and hence help to coordinate investment (i.e., the number of hearths in each property)? We show this using a Nash (1950) bargaining game where tenants and landlords bargained over the terms of rebuilding. The bargaining game consists of two stages. In Stage 1, in each parish j, the landlord and tenant of each property i bargain over a contract given their respective outside options. The outside options are based on the rulings established by the Fire Court in its initial cases for each parish. Therefore, the outside options vary across parishes. For simplicity, we suppress the subscripts j and i. We define the contract $\left\{r, t, I^l\right\}$ in terms of the annual rent (r), the tenancy length (t), and the amount of contributions (investment) that the landlord makes towards the rebuilding (I^l).[14] If the tenant and landlord reach an agreement, they move to the second stage where the tenant decides on her amount of contributions (investment) towards the rebuilding. The total amount of building investment (measured in terms of the number of hearths in the property) is given by the sum of the landlord's investment (determined in the first stage) and the tenant's investment (determined in the second stage). If they fail to reach an agreement, they bring their case to the Fire Court. In this framework, the Fire Court's rulings

[13]Finally, we could have also expanded our definition of pragmatic rulings to include cases where the judges voided the existing contracts but decreed cost sharing in the rebuilding. However, the reason why we do not do this is because we wanted our definition to reflect the complete burden of rebuilding falling on the landlord. This will be clearer when we discuss the model in the next section.

[14]More accurately, the parties bargain over the split of the total surplus. In doing so, the parties are implicitly choosing $\left\{r, t, I^l\right\}$.

affect the outside options and hence the bargaining dynamics between the landlords and tenants. The model is solved by backward induction.

5.3.1 Solving the Nash bargaining game: Stage 2

The tenant's problem in Stage 2 is to choose I^t to maximize utility given the contract $\left\{r, t, I^l\right\}$ that was determined in Stage 1:

$$\underset{I^t}{max}\ U\left(r, t, I^l, I^t\right) = \frac{1-\beta^t}{1-\beta}\left[h(I^l, I^t) - r\right] + \frac{\beta^t}{1-\beta}u^0 - pI^t \tag{2}$$

β is the discount factor. p is cost per unit of investment. u^0 is the tenant's utility after the tenancy ends. We assume that $h(I^l, I^t)$ is concave and that the amount of investments that the landlord and tenant make towards the building are complements ($\frac{\partial h}{\partial I^l \partial I^t} > 0$).

The first order condition is:

$$\frac{1-\beta^t}{1-\beta}h'(I^l, I^t) = p \tag{3}$$

Equation (3) suggests that the tenant's investment does not depend on the rent.

Proposition 1. *The tenant's investment is increasing in tenancy length.*
Proof: From Equation (3), let $\psi\left(t, I^l, I^t\right) \equiv \frac{1-\beta^t}{1-\beta}h'(I^l, I^t) - p$. By the implicit function theorem and since $h(I^l, I^t)$ is concave:

$$\begin{aligned}\frac{\partial I^t}{\partial t} &= -\frac{\psi_t}{\psi_{I^t}} \\ &= \frac{-\beta^t\beta h'(I^l, I^t)}{-\frac{1-\beta^t}{1-\beta}h''(I^l, I^t)} > 0\ \square\end{aligned} \tag{4}$$

Let the optimal tenant investment be denoted as: $I^{t*} \equiv g\left(t, I^l\right)$. Therefore, total investment is: $I\left(t\right) \equiv g\left(t, I^l\right) + I^l$

5.3.2 Solving the Nash bargaining game: Stage 1

In this stage, the tenant and landlord bargain over the surplus given their respective outside options π^c and u^c. Their outside options are based on the Fire Court's rulings in the initial cases. We assume that π^c and u^c vary across parishes. We represent the distribution of Fire Court decisions in the initial cases as $F(r, t, I^l)$. λ is the bargaining weight which we assume to be exogenous. The Nash bargaining game solution can be

characterized as:

$$\underset{\{r,t,I^l\}}{max} \left[\Pi\left(r,t,I^l\right)-\pi^c\right]^{\lambda}\left[U\left(r,t,I^l\right)-u^c\right]^{(1-\lambda)} \tag{5}$$

where the landlord's utility is $\Pi\left(r,t,I^l\right)=\frac{1-\beta^t}{1-\beta}r+\frac{\beta^t}{1-\beta}r^0\left(I(t)\right)-pI^l$. $r^0\left(I(t)\right)$ is the rent that the landlord receives from the next tenant after the tenancy agreement with the current tenant expires. In addition,

$$\pi^c=\int\int\int \frac{1-\beta^y}{1-\beta}x+\frac{\beta^y}{1-\beta}r^0\left(I(y)\right)-pz\ dF(x,y,z)$$

and

$$u^c=\int\int\int \frac{1-\beta^y}{1-\beta}\left[h(I(y))-x\right]+\frac{\beta^y}{1-\beta}u^0-pg(y,z)\ dF(x,y,z)$$

The Nash bargaining solution for the landlord is:

$$\Pi\left(r,t,I^l\right)=\lambda[\Pi\left(r,t,I^l\right)+U\left(r,t,I^l\right)-\pi^c-u^c]+\pi^c \tag{6}$$

and that for the tenant is:

$$U\left(r,t,I^l\right)=(1-\lambda)[\Pi\left(r,t,I^l\right)+U\left(r,t,I^l\right)-\pi^c-u^c]+u^c$$

Rearranging Equation (6), we get that:

$$(1-\lambda)\left[\frac{1-\beta^t}{1-\beta}r+\frac{\beta^t}{1-\beta}r^0\left(I(t)\right)-pI^l\right]-\pi^c=\lambda\left[\frac{1-\beta^t}{1-\beta}\left[h(I(t))-r\right]+\frac{\beta^t}{1-\beta}u^0-pg(I^l,t)-Q^c\right] \tag{7}$$

where $Q^c\equiv\pi^c+u^c=\int\int\int \frac{1-\beta^y}{1-\beta}h(I(y))+\frac{\beta^y}{1-\beta}\left[r^0\left(I(y)\right)+u^0\right]-pI(y)\ dF(x,y,z)$

Next, we assume that the judging panel's preferences for the landlord's contribution to the rebuilding is orthogonal to r and t. In other words, $F(x,y,z)\equiv F_{XY}(x,y)F_Z(z)$. Assuming that $F(x,y,z)\equiv F_{XY}(x,y)F_Z(z)$ has two implications. First, this assumption implies that the sum of the outside options is not affected by the transfer of the burden to rebuild:

$$Q^c=\int\int \frac{1-\beta^y}{1-\beta}h(I(y))+\frac{\beta^y}{1-\beta}\left[r^0\left(I(y)\right)+u^0\right]-pI(y)\ dF(x,y)$$

Second, while the sum of the outside options is not affected by the transfer of the burden to rebuild, the outside option of the landlord still depends on $F_Z(z)$. Given these two

implications, Equation (7) can be expressed as:

$$(1-\lambda)\left[\frac{1-\beta^t}{1-\beta}r+\frac{\beta^t}{1-\beta}r^0\left(I(t)\right)-pI^l\right]-\pi^c\left(F_Z(z)\right)=\lambda\left[\frac{1-\beta^t}{1-\beta}\left[h(I(t))-r\right]+\frac{\beta^t}{1-\beta}u^0-pg(I^l,t)-Q^c\right]$$

Now suppose that there is a contract $\left\{\bar{r},\bar{t},\bar{I}^l\right\}$ that satisfies the Nash bargaining game solution. However, we switch from $F_Z(z)$ to $F'_Z(z)$, where $F'_Z(z)$ first order stochastically dominates $F_Z(z)$. Recall that $F(r,t,I^l)$ represents the distribution of Fire Court rulings in the initial cases. In our empirical context, moving from $F_Z(z)$ to $F'_Z(z)$ corresponds to the initial cases getting assigned judging panels that have a greater probability of voiding the existing contracts and assigning the cost of rebuilding to the landlord. As explained in the previous section, this is what we define as pragmatic legal rulings.

Proposition 2. *The landlord's outside option* (π^c) *falls when the initial cases are assigned judging panels that have a greater preference for the landlord to contribute more to the rebuilding.*
Proof: Since $F'_Z(z)$ first order stochastically dominates $F_Z(z)$, this implies that the landlord's outside option under $F'_Z(z)$ is now smaller:

$$\begin{aligned} &F'_Z(z)\leq F_Z(z)\ \forall z \quad \text{and} \quad F'_Z(z)<F_Z(z) \text{ for some } z \\ \Rightarrow\ &\pi^c\left(F'_Z(z)\right)<\pi^c\left(F_Z(z)\right) \end{aligned} \tag{8}$$

The last inequality is because $\pi^c=\int\int\frac{1-\beta^y}{1-\beta}x+\frac{\beta^y}{1-\beta}r^0\left(I(y)\right)\ dF_{XY}(x,y)-p\int z\ dF_Z(z)$. Since $F'_Z(z)<F_Z(z)$, $p\int z\ dF'_Z(z)>p\int z\ dF_Z(z)$ and so $\pi^c\left(F'_Z(z)\right)<\pi^c\left(F_Z(z)\right)$.□

Since the landlord now has a smaller outside option, the contract $\left\{\bar{r},\bar{t},\bar{I}^l\right\}$ no longer satisfies the Nash bargaining game solution and Equation (7) no longer holds with equality. Instead, the landlord now has too much of the surplus and so:

$$(1-\lambda)\left[\frac{1-\beta^{\bar{t}}}{1-\beta}\bar{r}+\frac{\beta^{\bar{t}}}{1-\beta}r^0\left(I(\bar{t})\right)-p\bar{I}^l\right]-\pi^c\left(F'_Z(z)\right)>\lambda\left[\frac{1-\beta^{\bar{t}}}{1-\beta}\left[h(I(\bar{t}))-\bar{r}\right]+\frac{\beta^t}{1-\beta}u^0-pg(\bar{I}^l,\bar{t})-\bar{Q}^c\right] \tag{9}$$

In order to achieve equality, changes in the Nash bargained contract should (1) lower the left-hand side of the inequality and increase the right-hand side or (2) increase the right-hand side more than the left-hand side.

Proposition 3. *The Nash bargained rent* $(\bar{r})$ *decreases when the judging panels have a greater preference for the landlord to contribute more to the rebuilding.*
Proof: Referring to inequality 9, since $\frac{\partial LHS}{\partial\bar{r}}=\frac{1-\beta^{\bar{t}}}{1-\beta}(1-\lambda)>0$ and $\frac{\partial RHS}{\partial\bar{r}}=-\frac{1-\beta^{\bar{t}}}{1-\beta}\lambda<0$,

in order for the left-hand side to equal to the right-hand side, $\bar{r}$ has to decrease. A decrease in $\bar{r}$ decreases the left-hand side and increases the right-hand side.□

Proposition 4. *The Nash bargained landlord investment to the rebuilding ($\bar{I}^l$) increases when the judging panels have a greater preference for the landlord to contribute more to the rebuilding.*
Proof: Referring to inequality 9, since $\frac{\partial LHS}{\partial \bar{I}^l} = -p(1-\lambda) < 0$ and $\frac{\partial RHS}{\partial \bar{I}^l} = -p\lambda g_{\bar{I}^l}\left(\bar{I}^l, \bar{t}\right) = p\lambda > 0$, in order for the left-hand side to equal to the right-hand side, $\bar{I}^l$ has to increase. An increase in $\bar{I}^l$ decreases the left-hand side and increases the right-hand side.□

Proposition 5. *If the relative bargaining weight of the landlord is more than the relative marginal benefit of increasing the tenancy length, then the Nash bargained tenancy length ($\bar{t}$) increases when the judging panels have a greater preference for the landlord to contribute more to the rebuilding.*
Proof: Referring to inequality 9, $\frac{\partial LHS}{\partial \bar{t}} = (1-\lambda)\left\{(-ln\beta)\beta^{\bar{t}}\left[\bar{r} - r^0(I(\bar{t}))\right] + \frac{\beta^{\bar{t}}}{1-\beta} r_1^0(I(\bar{t}))g_{\bar{t}}\left(\bar{I}^l, \bar{t}\right)\right\} = (1-\lambda)\Pi_{\bar{t}}\left(\bar{r}, \bar{t}, \bar{I}^l\right)$ and $\frac{\partial RHS}{\partial \bar{t}} = \lambda\left\{(-ln\beta)\beta^t\left[h(I(\bar{t})) - \bar{r} - u^0\right] + \left[\frac{\beta^{\bar{t}}}{1-\beta}h'(I(\bar{t})) - p\right] g_{\bar{t}}\left(\bar{I}^l, \bar{t}\right)\right\} = \lambda U_{\bar{t}}\left(\bar{r}, \bar{t}, \bar{I}^l\right)$. To achieve equality, we need:

$$\begin{aligned} \frac{\partial LHS}{\partial \bar{t}} &< \frac{\partial RHS}{\partial \bar{t}} \\ \Rightarrow (1-\lambda)\Pi_{\bar{t}}\left(\bar{r}, \bar{t}, \bar{I}^l\right) &< \lambda U_{\bar{t}}\left(\bar{r}, \bar{t}, \bar{I}^l\right) \\ \Rightarrow \frac{\lambda}{1-\lambda} &> \frac{\Pi_{\bar{t}}\left(\bar{r}, \bar{t}, \bar{I}^l\right)}{U_{\bar{t}}\left(\bar{r}, \bar{t}, \bar{I}^l\right)} \quad \square \end{aligned}$$

This gives us a necessary and sufficient condition for $\bar{t}$ to increase. In other words, if the relative bargaining weight of the landlord is more than the relative marginal benefit of increasing the tenancy length, then $\bar{t}$ increases. Given the historical context that tenants are obliged to repair or rebuild the premises in the event of disasters or wars, this condition is likely to hold. In addition, in the Fire Court data, we see that the judging panels decreed that the tenant had to rebuild 70.9% of the time.

Putting everything together, our model suggests that if the initial cases are assigned judging panels that have a greater preference for the landlord to contribute more to the rebuilding, then this lowers the landlord's outside option (proposition 2). As a result of the lowering of the landlord's outside option, the Nash bargained annual rent (r) decreases (proposition 3), the amount of investment that the landlord makes towards the rebuilding (I^l) increase (proposition 4), and the effect on the tenancy length (t) increases (proposition 5). Crucially, our model shows us that by changing outside options, the rul-

ings of the Fire Court affected **all** tenants and landlords even if they did not bring their case to the Fire Court. This is how legal rulings affect expectations.

5.3.3 Empirical implication

In our empirical analysis, we estimate the change in the average number of hearths per property in parish j as a result of the initial cases in the parish getting assigned judging panels that have a greater propensity to void existing contracts (i.e., pragmatic rulings). This corresponds loosely to:

$$\frac{\partial E_j(I_i)}{\partial E_j(F_Z(z))} = \frac{\partial E_j(I_i^t)}{\partial E_j(F_Z(z))} + \frac{\partial E_j(I_i^l)}{\partial E_j(F_Z(z))}$$

$$= \underbrace{\frac{\partial E_j(I_i^t)}{\partial E_j(r_i)}}_{=0 \text{ by eq. 2}} \times \underbrace{\frac{\partial E_j(r_i)}{\partial E_j(F_{Z_j}(z))}}_{<0 \text{ by prop. 3}} + \underbrace{\frac{\partial E_j(I_i^t)}{\partial E_j(I_i^l)}}_{>0 \text{ by assumption}} \times \underbrace{\frac{\partial E_j(I_i^l)}{\partial E_j(F_{Z_j}(z))}}_{>0 \text{ by prop. 4}} + \underbrace{\frac{\partial E_j(I_i^t)}{\partial E_j(t_i)}}_{>0 \text{ by prop. 1}} \times \underbrace{\frac{\partial E_j(t_i)}{\partial E_j(F_{Z_j}(z))}}_{>0 \text{ by prop. 5}} \tag{10}$$

$$+ \underbrace{\frac{\partial E_j(I_i^l)}{\partial E_j(r_i)}}_{\text{ambiguous}} \times \underbrace{\frac{\partial E_j(r_i)}{\partial E_j(F_{Z_j}(z))}}_{<0 \text{ by prop. 3}} + \underbrace{\frac{\partial E_j(I_i^l)}{\partial E_j(I_i^l)}}_{=1} \times \underbrace{\frac{\partial E_j(I_i^l)}{\partial E_j(F_{Z_j}(z))}}_{>0 \text{ by prop. 4}} + \underbrace{\frac{\partial E_j(I_i^l)}{\partial E_j(t_i)}}_{\text{ambiguous}} \times \underbrace{\frac{\partial E_j(t_i)}{\partial E_j(F_{Z_j}(z))}}_{>0 \text{ by prop. 5}} \tag{11}$$

The second line gives us the effect on the tenant's investment and this effect is unambiguously positive. However, the effect on the landlord's investment (third line) is ambiguous. The signs of $\frac{\partial E_j(I_i^l)}{\partial E_j(r_i)}$ and $\frac{\partial E_j(I_i^l)}{\partial E_j(t_i)}$ are ambiguous because the landlord can in principle trade off a higher amount of investment to the building process with a lower rent or longer tenancy length. This happens because there are three variables $\left\{r, t, I^l\right\}$ that are governed by a single Nash bargaining equation (see Equation (6)). If the positive effect on the tenant's investment (second line) dominates the ambiguous effect on the landlord's investment (third line) then pragmatic legal rulings can result in a higher number of hearths per property.

To conclude this section, our model shows that even though landlords and tenants of different properties are bargaining separately and do not bring their case to the Fire Court, they end up choosing similar levels of hearths per property. This is because they have the same focal point and hence expectations of what others will do. This focal point is how the Fire Court ruled in the initial cases in their parish.

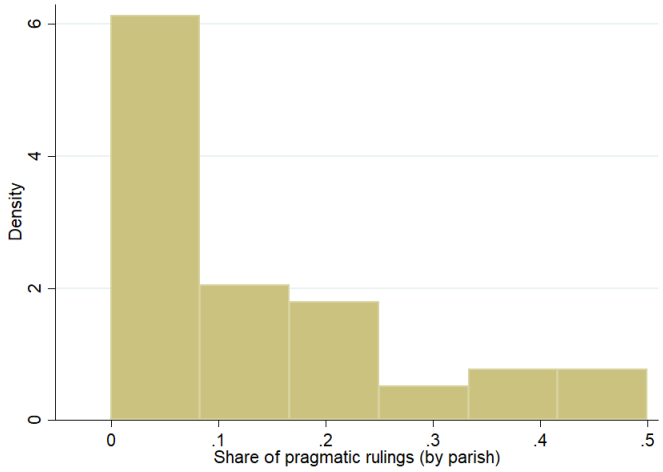
5.4 Empirical strategy

To examine the effect of legal rulings, we continue to use a DiD empirical strategy:

$$ln\left(Hearths_{ijt}\right) = \alpha_j + \delta PostFire_t + \beta PragmaticRulings_j \times PostFire_t + \gamma' X_{jt} + \epsilon_{ijt} \tag{12}$$

$ln\left(Hearths_{ijt}\right)$ is the log number of hearths in property i in parish j in period t. The two periods are before the Fire and after the Fire. $PragmaticRulings_j$ denotes the share of initial cases in parish j where the Fire Court judging panels' rulings were pragmatic.[15] Specifically, this is the share of cases in parish j where the Fire Court judging panels voided the existing contracts and assigned the rebuilding to the landlord. Figure 4 shows the distribution of the share of pragmatic rulings across the parishes. $PostFire_t$ is an indicator variable for the period after the Fire. X_{jt} is a vector of controls. Finally, α_j are parish fixed effects. We cluster the standard errors at the parish level.

Figure 4: Distribution of the share of pragmatic rulings across parishes



It is important to note that we are not able to distinguish in the data properties that went to the Fire Court and those that did not. The regression therefore includes all properties in the parish – those that went to the Fire Court and those that did not. However, this should not affect our results substantially since the proportion of properties in each parish that went to the Fire Court is a relatively small number. Based on the initial cases, the average proportion of properties in each parish that went to the Fire Court was 6%, the median was 4% and the maximum was 30%. In Table C1 we report the proportion of properties in each parish where the landlord and the tenant went to the Fire Court (based on the data that we have). Therefore, in the regression, β also

[15] As we only have data from the first four out of nine volumes of the Fire Court cases, these figures are calculated based on the first four volumes.

tells us whether the rulings in a small share of properties in the parish that went to the Fire Court affected the quality of other properties in the parish.

For those interested in the cross-sectional regressions in each time period, the results are reported in Table D8. In the pre-Fire period, the number of hearths per property in parishes where all the legal rulings were pragmatic versus parishes with no pragmatic legal rulings was statistically indistinguishable.

Recall that in the previous section, our DiD regression compares burned parishes to unburned parishes. As a result, there could be concerns that any positive effect is purely mechanical since rebuilt properties had to be built according to strict regulations that specified the size and materials used. However, in this section, since our sample consists only of burned parishes, the DiD strategy helps to net off these mechanical effects. This allows us to more cleanly attribute the effect that we estimate to legal rulings.

5.5 Results and discussion

Higher quality structures. Table 10 reports whether the rulings in a small share of properties in the parish that went to the Fire Court affected the quality of other properties in the parish (i.e., number of hearths per property). The estimate in column 1 shows that controlling for parish and year fixed effects, parishes where all the initial cases saw pragmatic Fire Court rulings experienced a highly statistically significant increase of around 144% more hearths compared to parishes where all the initial cases saw unpragmatic rulings.

Importantly, the share of pragmatic rulings differ across parishes because of both exogenous and endogenous reasons. Therefore, in our regression, we control for as many endogenous reasons as we can. In column 2, we include a series of parish controls interacted with $PostFire_t$. These include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. This helps to address concerns that our results could be driven by the politics and resources of the parishes. Reassuring, our results remain extremely stable to the inclusion of these controls.

In column 3, we show that the results are stable to the inclusion of broader locations-by-post fixed effects. Finally, in column 4, we include pre-Fire hearth terciles-by-post fixed effects. The estimated effect continues to be robust to the inclusion of these controls. In particular, parishes where all the initial cases saw pragmatic Fire Court rulings experienced a highly statistically significant increase of around 98.1% more hearths compared

to parishes where all the initial cases resulted in unpragmatic rulings. Given that the average number of hearths before the Fire was 3.83, this translates to an increase of 3.76 hearths. Expressed in a different way, what our result suggests is that in terms of the share of pragmatic rulings, going from the 25th percentile parish (0% pragmatic rulings) to the 75th percentile parish (20% pragmatic rulings) resulted in a 19.6% increase in the number of hearths. In absolute terms, this corresponds to an increase of 0.75 hearths. Figure C3 shows the binned scatter plot of the results in column 4.

Table 10: Effect of legal rulings on the number of hearths per property

	(1)	(2)	(3)	(4)
VARIABLES		ln(No. hearths)		
Pragmatic X Post Fire	1.444***	1.253***	1.103***	0.981***
	(0.449)	(0.393)	(0.286)	(0.246)
Observations	31,582	31,582	31,582	31,582
R-squared	0.014	0.024	0.026	0.031
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	46	46	46	46

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

The results show us that while only a small share of properties in each parish went to the Fire Court, the rulings of these few cases had an outsized effect on the quality of other buildings in the parish. Why would this be the case? We argue that this is because the small share of cases was enough to anchor expectations. This positive result is even more remarkable considering that the hearth tax was introduced in 1662. Occupants of properties would have been incentivized to rebuild with fewer hearths to avoid the tax. Despite this, we still see a positive effect due to legal rulings. This suggests the possibility that even before the Fire, there was latent demand for structures with more hearths. Consequently, the simultaneous building after the Fire led to greater cross-building spillovers and helped to address this demand. Our results hence provide us with evidence that pragmatic legal rulings can indeed anchor expectations of what others will do.

Finally, our regression uses parish-level variations in Fire Court rulings. A natural question to ask is why do the Court rulings in your own parish matter? We take two approaches to address this. First, using the historical context. In early modern London, most interactions took place at the parish-level. Individuals often worked, lived and worshiped in the same parish. Moreover, parishes were given quite a bit of autonomy in

civil matters. For example, the Highways Act 1555 made road maintenance the responsibility of the parish and Poor Relief Act 1601 (Poor Law) outlined the responsibility of the parish to look after its own poor. Therefore, because of the context, we argue that what is most salient to inhabitants of the parish is what happens within their own parish.

Second, we show statistical evidence that the rulings of previous cases in your own parish predicts future rulings in your parish. To show this, we run the following regression using the Fire Court cases of parishes that appear in the hearth tax data:

$$PragmaticRuling_{ijp} = \theta_p + \beta PragmaticRulingFirstFewCases_j + \lambda' X_{ijp} + \epsilon_{ijp}$$

$PragmaticRuling_{ijp}$ is a dummy variable that indicates whether the judging panel p for case i in parish j decreed a pragmatic ruling. $PragmaticRulingFirstFewCases_j$ is the share of pragmatic rulings in the first few cases preceding the current case in parish j. When running the regressions, we try different definitions of "first few cases". For example, the first two cases, the first three cases, etc. Taking the average across the first few cases accounts for the fact that the first case may not be precedential and precedents may take some time to be firmly established. θ_p are judging panel fixed effects. X_{ijp} is a vector of controls. These include pre-Fire case characteristics such as the degree of subletting in the property, the number of years left in the tenancy, the rent, the fine paid to secure the contract and whether the tenant spent any money to improve the property. Importantly, the vector of controls also includes the share of pragmatic rulings in other parishes before case i in parish j. The standard errors are clustered at the parish level.

Table 11 reports the results. In column 1, the definition of first few cases is the first case, in column 2, the definition of the first few cases is the first two cases, and so on in the other columns. Across all columns, the coefficient estimate of β is positive. This suggests that past rulings in your own parish predicts future rulings. In addition, the coefficient estimates increase as we move across the columns. This reflects the fact that the first case may not be precedential and precedents may take some time to be firmly established. Column 5 suggests that if the first five cases in your parish had pragmatic rulings, the probability that the current case receives a pragmatic ruling increases by 40.5%-points. Therefore, the Court rulings in your own parish matter because they predict future rulings in your parish.

Table 11: Effect of past rulings in your own parish on current ruling

VARIABLES	(1)	(2)	(3)	(4)	(5)
	Whether current case ruling is pragmatic				
Share of pragmatic rulings (first few cases)	0.024	0.022	0.202*	0.327**	0.405*
	(0.061)	(0.075)	(0.117)	(0.144)	(0.199)
Observations	303	246	195	163	139
R-squared	0.350	0.417	0.443	0.510	0.488
Judging panel FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
First few cases	1	2	3	4	5

Notes: Case controls include pre-Fire case characteristics such as the degree of subletting in the property, the number of years left in the tenancy, the rent, the fine paid to secure the contract and whether the tenant spent any money to improve the property. It also includes the share of pragmatic rulings in other parishes before the current case. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Effect holds when controlling for spillovers from neighboring parishes. Next, we check if our results are sensitive to potential spatial spillovers. In particular, burned parishes with Fire Court cases tend to be located near each other. Therefore, a burned parish with Fire Court cases not only generated spillovers to other burned parishes but also received inward spillovers from these other parishes. If these spillovers are large, our estimated effects of legal rulings within each parish could be overstated. Therefore, we include as a control the weighted share of cases in neighboring burned parishes where the Fire Court judging panels decreed pragmatic rulings. Table 12 shows that our results are robust to controlling for the legal outcomes in neighboring parishes.

Table 12: Effect of legal rulings on the number of hearths per property (controlling for spillovers)

VARIABLES	(1)	(2)	(3)	(4)
	ln(No. hearths)			
Pragmatic X Post Fire	1.485***	1.149***	1.119***	0.963***
	(0.478)	(0.326)	(0.278)	(0.253)
Pragmatic Spillover X Post Fire	-0.513	0.230	0.013	0.349
	(0.635)	(0.358)	(0.315)	(0.286)
Observations	31,582	31,582	31,582	31,582
R-squared	0.015	0.025	0.026	0.031
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	46	46	46	46

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. We also include the log number of cases in neighboring parishes. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Adding in other dimensions of the rulings as controls. The Fire Court judges were given the power to decree who rebuilds, the rent, as well as length of the new contract. Such multi-dimensional rulings make it difficult to define what constitutes pragmatic rulings that helped to facilitate the rebuilding of London. In order to circumvent the issue of multi-dimensional rulings, in our analysis, we had focused on the most extreme of pragmatic case outcomes – cases where the Fire Court voided existing contracts and assigned the rebuilding to the landlord. Nonetheless, it could well be the case that the newly decreed rent or tenancy length could be playing a role in anchoring expectations and consequently, the number of hearths per property in each parish.

To address this concern, we include as controls the average change in rent and tenancy length in each parish arising from the Fire Court rulings. For example, if there were five cases in parish A that went to the Fire Court, and the judging panel increased the tenancy length for all five of the cases by 10 years, then the average change in tenancy length arising from the Fire Court rulings for parish A would be 10 years. Referring to Table D9, the coefficient estimate on our treatment variable $PragmaticRulings_j \times PostFire_t$ remains stable to the inclusion of the other dimensions of the Fire Court's rulings. The coefficient estimates on the average change in the tenancy length interacted with post are extremely close to zero and statistically insignificant. The coefficient estimates on the average change in the rent interacted with post while significant, is relatively small in magnitude. Taken together, the results across the columns provide evidence that it is indeed pragmatic Fire Court rulings (as defined by the share of initial cases where the judging panels voided the contracts and assigned the rebuilding to the landlord) that are affecting individuals' expectations and not the other dimensions of the Fire Court's decisions.

5.6 Competing hypotheses/mechanisms

The results in Table 10 show us that while only a small share of properties in each parish went to the Fire Court, the rulings of these few cases had an outsized effect on the quality of other buildings in the parish. We argue that this is because the small share of cases was enough to anchor expectations. Are we able to rule out competing hypotheses/mechanisms?

One competing hypothesis is that our results have nothing to do with the small share of cases anchoring expectations. Instead, it is simply picking up the direct effect of the Fire Court rulings. This is because we are not able to distinguish in the data properties that went to the Fire Court and those that did not. The regression therefore includes all properties in the parish – those that went to the Fire Court and those that did not.

To see why this might be a problem, consider the following example of a parish where there are 100 properties (see Table 13). Before the Fire, the average number of hearths per property was 10. Now assume that of the 100 properties, 60 did not go to Court but 40 went to Court. Let us further assume that the average number of hearths in the 60 properties was the same before and after the Fire (i.e., 10 hearths). However, in the 40 cases that went to the court, the average number of hearths increased by three per property to 13 hearths. Consequently, the overall average number of hearths per property increased by 1.2 to 11.2. This stylized example shows us how the average number of hearths can increase even without any anchoring of expectations of those that did not go to Court.

Table 13: Stylized example

	Total Hearths	Number of properties	Avg. hearths per property
Before Fire	100*10=1,000	100	10
After Fire	(60*10)+(40*13)=1,120	100	11.2

However, we think that this should not affect our results substantially since the proportion of properties in each parish that went to the Fire Court is a relatively small number. Based on the initial cases, the average percentage of properties in each parish that went to the Fire Court was 6%, the median was 4% and the maximum was 30%. In addition, Table 14 shows the results when we drop all parishes where more than 14.6% (column 2), 7.4% (column 3), 4.1% (column 4) and 2.3% (column 5) of the properties went to the Fire Court. 14.6%, 7.4%, 4.1% and 2.3% correspond to the 95th, 75th, 50th and 25th percentiles respectively.

Table 14: Effect of legal rulings on the number of hearths per property (dropping parishes where a "large" proportion of properties went to Court)

	(1)	(2)	(3)	(4)	(5)
VARIABLES	ln(No. Hearths per Property)				
Pragmatic X Post Fire	0.981***	0.893***	0.897***	1.051***	1.121*
	(0.246)	(0.241)	(0.245)	(0.232)	(0.527)
Observations	31,582	30,993	29,210	26,289	21,953
R-squared	0.031	0.032	0.033	0.035	0.042
Parish FE	✓	✓	✓	✓	✓
Post FE	✓	✓	✓	✓	✓
Parish controls X Post FE	✓	✓	✓	✓	✓
Broader location X Post FE	✓	✓	✓	✓	✓
Pre-fire hearth tercile X Post FE	✓	✓	✓	✓	✓
Number of clusters	46	43	34	24	12
Sample	All	< 95 pct	< 75 pct	< 50 pct	< 25 pct

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Column 1 shows the results using the full sample. Our results remain robust to dropping parishes where a "large" proportion of properties went to the Fire Court. If anything, our results seem to get bigger when we drop more parishes which is against what we should see if our results are purely picking up the direct effect of the Fire Court rulings.

Another competing hypothesis is that of the share of owner-occupied properties in the parish. In parishes where there is a large share of owner-occupied properties, the share of properties that go to Court must by definition be small since owners cannot sue themselves. Therefore, our results may have nothing to do with the small share of cases anchoring expectations. Instead, they could simply be reflecting the fact that these parishes have a greater share of owner-occupied properties. Landowners are less likely to be credit constrained and can thus allocate more resources towards building more hearths per property. In addition, if the owner occupies the property, then there is a clear assignment/alignment of property-rights. Consequently, the owner builds at a higher quality because the owner is able to accrue the full benefits of living in a higher quality property.

Unfortunately, the data do not tell us whether a property is owner occupied – they only tell us who the main occupant of the property is. To overcome this limitation, we count the share of peers, high-ranking military personnel and doctors living in the parish. To the extent that this group of individuals are more likely to own their own homes, this variable gives us a proxy for the share of owner-occupied properties in the parish. We then include these three variables as controls in our regression. Referring to Table 10 column 2, we can see that the coefficient estimate on $PragmaticRulings_j \times PostFire_t$ remains stable to the inclusion of these variables as controls. This suggests that there are aspects of the rulings in the small share of properties that went to Court that cannot be attributed to the share of owner-occupied properties in the parish. Therefore, our results lend credence to our proposed mechanism that the small share of cases was enough to anchor expectations for everyone in the parish.

5.7 Robustness checks

Dropping parishes which merged after the Fire. One concern could be that the Fire led to the merging of some parishes and so our results could be driven by these enlarged parishes which might have more resources. In Table D10 we re-run Equation (12) using only parishes that did not merge after the Fire. Reassuringly, the coefficient estimates remain similar and even larger than our baseline results, suggesting that our baseline results are conservative.

Accounting for zeros in the outcome variable. For burned parishes with Fire Court cases, there are 801 observations which are recorded as having zero hearths in the property. Taking logs results in these observations dropping out of the regression. Therefore, to account for the zeros in the outcome variables, we adopt two approaches. First, applying the inverse hyperbolic sine transform to hearths. Second, using a Poisson pseudo-likelihood (PPML) regression. The results are reported in Table D11 and Table D12 respectively. The estimated effects are very similar to our baseline results.

Trimming extreme values of the outcome variable. Another robustness check that we run is to drop the top and bottom 1 percentile of $ln\,(Hearths_{ijt})$. The results of this robustness check is reported in Table Table D13 and are similar to our baseline results.

5.8 Using an IV estimation strategy

As there could be concerns that there are time-varying parish-level omitted variables which we have not controlled for, we augment our DiD strategy with an instrumental variable (IV) strategy. Our IV DiD strategy exploits the fact that Fire Court judging panels with different political alignments (i.e., whether they were predominantly Royalists or Parliamentarians) were assigned to the cases in the parishes. The 1666 Great Fire took place in the midst of the Second Dutch War (1665-1667) and the Great Plague which began in 1665. King Charles II was relying on taxes and loans from London and its wealthiest citizens to finance the war. The destruction of the customs house, wharves and more than 13,000 buildings caused a significant drop in royal revenue and thus the King had a vested interest for London to be quickly rebuilt. Therefore, judging panels that were predominantly Royalists (more aligned with the King) were more likely to decree pragmatic rulings so as to facilitate the rebuilding of London. As a result, we can use the composition of the judging panels as an instrument for the share of initial cases in the parish that had pragmatic rulings. This gives us exogenous variations in legal rulings for each of the parishes. Of the 46 parishes in our regression with Fire Court cases, 17 of them (37.0%) had the majority of their initial cases presided by judging panels that consisted predominantly of Royalists.

5.8.1 Relevance of instrument

We estimate the following regression to examine the first-stage relationship between the composition of the judging panels in the initial cases and the share of initial cases in the

parish that had pragmatic rulings:

$$PragmaticRulings_j \times PostFire_t = \alpha_j + \delta PostFire_t + \beta MajorityRoyalist_j \times PostFire_t + \gamma' X_{jt} + u_{ijt} \quad (13)$$

Table 15 presents the first-stage results which suggest that if the majority of the initial cases in the parish were heard by judging panels that were predominantly Royalists, then the share of initial cases in the parish that had pragmatic rulings increased by 9.5%-pts.

Table 15: First-stage – Effect of Royalist on legal rulings

VARIABLES	(1)	(2)	(3)	(4)
	Pragmatic X Post			
Majority royalist in judging panels X Post	0.119***	0.087*	0.095***	0.095***
	(0.038)	(0.045)	(0.031)	(0.029)
Observations	31,582	31,582	31,582	31,582
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	46	46	46	46
KP F-stat	9.895	3.795	9.122	10.37

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

The first-stage relationship is robust to the inclusion of a series of parish controls interacted with $PostFire_t$, broader locations-by-post fixed effects and pre-Fire hearth terciles-by-post fixed effects. In addition, in most of the regressions, the first-stage has a KP F-statistic value of around 10. Figure C4 shows the binned scatter plot of the results in column 4.

5.8.2 Validity of instrument

Conditional Independence. The validity of our instrument depends crucially on whether there were other parish-level factors involved in determining the composition of the judging panels in the initial cases. We verify this by running a balancing test. This is similar to the type of statistical test that is used to verify random assignment in a randomized controlled trial. Table 16 shows the result of this balancing test. The coefficients on the share of peers, high-ranking military personnel and doctors seem sizable. However, this is because the mean values of these variables are extremely small. For example, the mean value for the share of peers is 0.005, that of high-ranking military personnel is 0.0004 and that for doctors is 0.003.

Table 16 shows that parish-level factors were not predictive of the composition of the

judging panels in the initial cases. In column 4, for parishes where data were available, we included the average rent in the parish in 1638. The 1638 rental data comes from "The Inhabitants of London in 1638".[16] Column 4 shows that historical rents were not predictive of the composition of the judging panels. Importantly, across all of the columns, all of the estimates are statistically insignificant at the 1% level and are not jointly significant with p-values ranging from 0.84 to 0.93.

Table 16: Testing for random assignment of judging panels to parishes

VARIABLES	(1)	(2)	(3)	(4)
	Majority royalist in judging panels			
ln(No. properties before the Fire)	-0.033	0.004	0.034	0.029
	(0.064)	(0.086)	(0.095)	(0.133)
Share of peers	-2.071	-3.816	-6.501	-4.256
	(9.929)	(10.247)	(11.106)	(23.191)
Share of high-ranking military personnel	-30.800	-7.468	-10.069	-21.936
	(29.296)	(42.349)	(51.972)	(81.006)
Share of doctors	1.689	7.120	2.283	-7.803
	(17.258)	(19.069)	(19.263)	(27.936)
Broader location 1		-0.097	-0.075	0.098
		(0.324)	(0.322)	(0.494)
Broader location 2		0.161	0.169	0.220
		(0.288)	(0.300)	(0.518)
Pre-Fire hearth tercile 1			-0.177	-0.315
			(0.236)	(0.292)
Pre-Fire hearth tercile 2			-0.049	-0.063
			(0.210)	(0.250)
ln(Average rent in 1638)				-0.175
				(0.289)
Observations	46	46	46	37
Adjusted R-squared	-0.081	-0.109	-0.152	-0.259
F-stat for joint test	0.350	0.431	0.373	0.361
p-value for joint test	0.843	0.853	0.928	0.932

Notes: Robust standard errors. Notation for statistical significance:
*** p<0.01, ** p<0.05, * p<0.1.

Exclusion. This restriction requires that the composition of the judging panels in the initial cases affected the quality of building investment in the parish only through its effect on legal rulings. While it is not possible to formally test the exclusion condition, the fact that our instrument passes the balancing test is reassuring.

However, there could still be concerns that the exclusion restriction could be violated since it is possible that monarchist officials may had some influence in the assignment of judges to the cases. For example, these officials could be expecting some parishes to grow

[16] "The Inhabitants of London in 1638" was originally published by the Society of Genealogists in London in 1931 (see Dale (1931)) and can be accessed at the British History Online website. This publication was based on the manuscript "Settlement of Tithes, 1638", found in the Lambeth Palace Library. The manuscript contains a list of the householders in 93 out of the 107 parishes in the City of London, as well as the rentals paid for the houses and the tithes paid.

more, and so they wanted to make sure that the parliamentarian judges did not derail their plans. In addition, it could be the case that some parishes had more monarchist landowners and so the officials assigned monarchist judges to protect the interest of these landowners.

In order to address such concerns, we are in the process of collecting data to measure what is the share of peers in each parish that was loyal to the King. From the hearth tax data, we are able to identify the names of the peers (i.e., Duke, Duchess, Marquess, Marchioness, Earl, Countess, Viscount, Viscountess, Baron, Baroness, Lord, Lady, Sir, Dame and Ambassador) living in each parish. For the parishes in our regression, there are about 500 peers in total. We can then refer to various historical sources (e.g., the Oxford Dictionary of National Biography (2004)) to find out what were the views of these peers on the 1660 restoration of the monarchy. Once we have determined the share of monarchist peers in each parish, we can then include this variable as a control in the regression. This would hopefully help to control for the possibility that monarchist judges were being assigned to parishes where the monarchists wanted to have greater influence over or benefit more from.

In any case, even if the exclusion restriction is violated, our reduced form estimates can still be interpreted as the causal effect of the composition of the judging panels in the initial cases on the number of hearths per property in the parish.

Monotonicity. The monotonicity assumption requires a monotonic relationship between the instrument and the variable that is being instrumented. The monotonicity assumption ensures that our IV estimate can be interpreted as a local average treatment effect (LATE). In our context, this is the average causal effect among the subgroup of parishes that invested differently in their buildings because of the composition of the judging panels in the initial cases.

If the monotonicity assumption is violated, then in the classical IV framework, our results can only be interpreted as causal **constant** effects. On the other hand, in a heterogeneous treatment effects framework, if the monotonicity assumption is violated, Angrist, Imbens and Rubin (1996) and Heckman and Vytlacil (2005) show that the IV estimates would still be a weighted average of marginal treatment effects. However, because the weights do not sum to one, this leads to an ill-defined local average treatment effect.

One testable implication of the monotonicity assumption is that the first-stage estimates should be non-negative for any subsample. To test this, we split the sample into

various subsamples and estimated the first-stage relationship for each of these subsamples. The results are reported in Table D14. In columns 1 and 2, we split the sample into whether the church in the parish was damaged by the Fire. In columns 3 to 5, we split the sample into three broader geographical locations (i.e., abutting the City of London walls, within the walls and outside the walls). In columns 6 to 8, we split the sample based on terciles of the number of hearths in each parish before the Fire. In columns 9 to 11, we split the sample based on terciles of the number of properties in each parish before the Fire. In columns 12 to 14, we split the sample based on terciles of the share of peers in the parishes. In columns 15 and 16, we split the sample into two based on the share of doctors in the parishes. Finally, in columns 17 and 18, we split the sample into two based on the share of high-ranking military personnel in the parishes. Out of these 18 subsamples, there are only three subsamples where the first-stage estimate is negative. In the other 15 subsamples, the first-stage estimates are positive, consistent with the monotonicity assumption.

5.8.3 IV results and discussion

Table 17 reports the results from the IV DiD regressions. In column 4, the results suggest that parishes where all the initial cases saw pragmatic Fire Court rulings experienced a highly statistically significant increase of around 200% more hearths compared to parishes where all the initial cases saw unpragmatic rulings. In other words, going from the 25th percentile parish in terms of the share of pragmatic rulings (0% pragmatic rulings) to the 75th percentile parish (20% pragmatic rulings) resulted in a 40% increase in the number of hearths. In absolute terms, this corresponds to an increase of 1.53 hearths. For completeness, we also report the reduced form estimates in Table D15 and show the associated binned scatter plot of the residues (based on all the controls) in Figure C5.

Table 17: IV – Effect of legal rulings on the number of hearths per property

VARIABLES	(1)	(2)	(3)	(4)
	ln(No. hearths)			
Pragmatic X Post Fire	2.276***	2.550***	2.247***	2.001**
	(0.698)	(0.917)	(0.753)	(0.789)
Observations	31,582	31,582	31,582	31,582
R-squared	0.010	0.016	0.020	0.026
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	46	46	46	46
KP F-stat	9.895	3.795	9.122	10.37

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

The IV results are around twice as large as the DiD results (1.53 hearths vs. 0.75 hearths). This could suggest two things. First, the DiD regression suffers from omitted variables and thus fails the parallel trend assumption. Second, even if the DiD estimate is unbiased, we should still expect the IV estimate to be different from it. This is because the IV estimate gives us the local average treatment effect for the compilers. Nevertheless, the fact that the IV estimates are positive and highly statistically significant re-affirms our DiD results. This gives us greater confidence in concluding that pragmatic legal rulings affected individuals' expectations about how much other individuals in their parish would be investing. This in turn resulted in a higher number of hearths per property in the parish.

Adding in other dimensions of the rulings as controls. To address concerns that our definition of pragmatic rulings fails to consider changes in rent and tenancy length, we include these as controls in our regression. Table D16 provides evidence that it is indeed pragmatic Fire Court rulings that are affecting individuals' expectations and not the other dimensions of the Fire Court's decisions.

5.8.4 Robustness checks

Dropping parishes which merged after the Fire. Similar to the other sections, in Table D17 we re-run our IV analysis using only parishes that did not merge after the Fire. The coefficient estimates remain very similar to our baseline results.

Accounting for zeros in the outcome variable. To account for the zeros in the outcome variables, we apply the inverse hyperbolic sine transform to hearths. The results are reported in Table D18. The estimated effects remain positive and continue to be highly statistically significant.

Trimming extreme values of the outcome variable. Another robustness check that we run is to drop the top and bottom 1 percentile of $ln\left(Hearths_{ijt}\right)$. The results of this robustness check is reported in Table D19 and are similar to our baseline results.

6 Conclusion

The development of cities often involves the rejuvenation and replacement of outdated buildings. However, the sunk cost of existing durable structures often serves as an impediment. While disasters are destructive, an unintended silver lining is that they may help to remove development frictions. By lowering the opportunity cost of waiting to rebuild to zero, disasters could potentially spur the development of neighborhoods and even cities. However, disasters do not necessarily guarantee higher quality buildings.

What ultimately matters is what each individual expects other individuals to do. Our paper highlights this by providing causal evidence of how legal rulings can be a main driver in the formation of these expectations. While there is a relatively large theoretical literature on how legal institutions can affect expectations and hence the behavior of individuals, there is relatively less empirical work on this. Our paper thus addresses this gap in the literature. Although the setting of our paper is 17th century England, even today, legal rulings continue to be a key aspect in society. This has policy implications as it suggests scope for laws to influence expectations and in so doing, facilitate the continual development of cities.

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Appendix A: The Effect of the Fire on Quantity

First, did the Fire result in fewer properties being rebuilt? To answer this, we run a difference-in-differences regression where we collapse the data to the parish-level:

$$ln\left(Properties_{jt}\right) = \alpha_j + \delta PostFire_t + \beta Burned_j \times PostFire_t + \gamma' X_{jt} + \epsilon_{jt}$$

$ln\left(Properties_{jt}\right)$ is the log number of properties in parish j in period t. The two periods are before the Fire and after the Fire. $Burned_j$ is an indicator variable that denotes whether the parish experienced damage from the Fire. $PostFire_t$ is an indicator variable for the period after the Fire. X_{jt} is a vector of controls. These include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. These variables are interacted with post-Fire. Broader locations-by-post fixed effects are also included to control for geographical characteristics. Finally, α_j are parish fixed effects. The standard errors are clustered at the parish-level.

Table A1 presents the results of this regression. The coefficient estimates of β are negative. This is expected as the plague wiped out about a quarter of London's population so we should expect fewer properties to be rebuilt in the immediate aftermath since there are now fewer people to house. The results in column 4 suggest that burned parishes saw a highly statistically significant decrease of around 67.6% properties as compared to unburned parishes. In addition, the reduction in the number of properties is consistent with post-Fire regulations that stipulated that properties needed to be of a certain minimum size.

Table A1: Effect of Fire on the number of properties

VARIABLES	(1)	(2)	(3)	(4)
	ln(No. Properties)			
Parish Burned X Post Fire	-1.059***	-1.256***	-0.790***	-0.676**
	(0.240)	(0.267)	(0.283)	(0.258)
Observations	140	140	140	140
R-squared	0.205	0.354	0.429	0.460
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	70	70	70	70

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Second, did the total number of hearths in the parishes decline after the rebuilding?

To answer this, we again run a difference-in-differences regression where we collapse the data to the parish-level:

$$ln\left(Hearths_{jt}\right) = \alpha_j + \delta PostFire_t + \beta Burned_j \times PostFire_t + \gamma' X_{jt} + \epsilon_{jt}$$

$ln\left(Hearths_{jt}\right)$ is the log number of hearths in parish j in period t. The other variables are the same as previously defined and the standard errors are clustered at the parish-level. Table A2 presents the results from this regression. The results are similar to what happens to the total number of properties being rebuilt after the Fire (Table A1). In particular, the coefficient estimates are negative.

Table A2: Effect of Fire on the number of hearths

	(1)	(2)	(3)	(4)
VARIABLES	ln(No. Hearths)			
Parish Burned X Post Fire	-0.866***	-1.043***	-0.643**	-0.518*
	(0.232)	(0.251)	(0.293)	(0.271)
Observations	140	140	140	140
R-squared	0.147	0.324	0.387	0.427
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	70	70	70	70

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Appendix B: Discussion about Jensen's Inequality

Running a regression with a log transformed dependent variable could result in an opposite treatment effect as compared to if we were to run the regression without taking logs. To see this, consider the following stylized example:[17]

Table B1: Stylized example about Jensen's inequality

Parish 1

Property	Hearths	ln(Hearths)
1	10	2.30
2	20	3.00
Parish Average	15	2.65

Parish 2

Property	Hearths	ln(Hearths)
1	5	1.61
2	30	3.40
Parish Average	18	2.51

In this example, the average number of hearths per property and the total number of hearths are higher in parish 2 than in parish 1. However, if we ran a regression using the log of each property's hearths on a parish dummy, we will find that parish 2 on average has fewer log hearths per property.

This stylized example shows the possibility that this could happen but it does not mean that this would definitely happen for other values. Therefore, what we do is to replicate this stylized example using the actual data that we have. In particular, we collapse the data into two groups – burned and unburned parishes. We then compare the differences of the averages (in both logs and without logs) across the burned and unburned groups in the pre- and post-Fire periods. Table B2 reports the averages from this exercise. It shows us that both a regression without logs and a regression with logs will give us a positive effect. In particular, from the regression without logs we will get a difference-in-differences effect of: $(6.07 - 4.70) - (4.74 - 4.41) = 1.04$. In the regression with logs we get: $(1.80 - 1.55) - (1.56 - 1.48) = 0.17$. Fortunately, the reversal of signs issue does not happen when we use the actual data.

[17] We would like to thank David Weinstein for providing us with this stylized example.

Table B2: Stylized example using actual data

UNBURNED

Post-Fire	Parish Average: Hearths	Parish Average: ln(Hearths)
0	4.41	1.48
1	4.74	1.56

BURNED

Post-Fire	Parish Average: Hearths	Parish Average: ln(Hearths)
0	4.70	1.55
1	6.07	1.80

The second approach would be to directly run the quality regression without taking logs on the left hand-side variable. This guarantees that the regression will not suffer from Jensen's inequality issues but it comes at the expense of failing the parallel trends assumption and the results being potentially driven by the skewed data. Nevertheless, Table B3 shows that the estimated coefficient from this regression is positive. Since both the regressions in logs and without logs give us positive coefficient estimates, this should allay the worry that the estimated effect could actually be positive without taking logs but negative with a log transformation due to Jensen's inequality.

Table B3: Effect of Fire on the number of hearths per property (no logs)

VARIABLES	(1)	(2)	(3)	(4)
	No. Hearths per Property			
Parish Burned X Post Fire	0.614	0.569	0.407	0.524
	(0.371)	(0.399)	(0.497)	(0.468)
Observations	79,730	79,730	79,730	79,730
R-squared	0.002	0.003	0.003	0.004
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	70	70	70	70

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Appendix C: Additional Figures and Tables

Figure C1: Case characteristics over time

Figure C1.1: Years left in tenancy

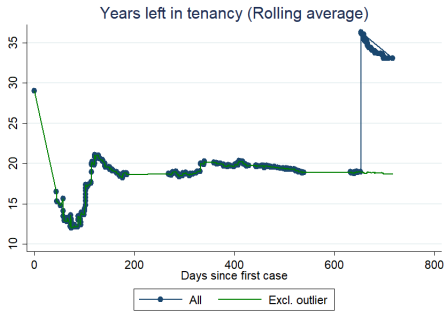

Figure C1.2: Pre-Fire rent

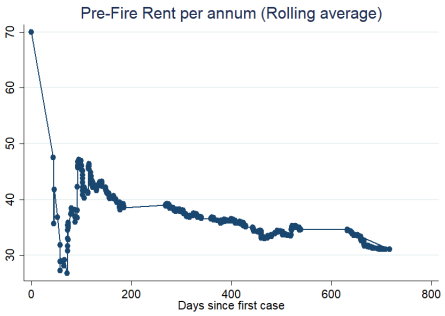

Figure C1.3: Pre-Fire fine

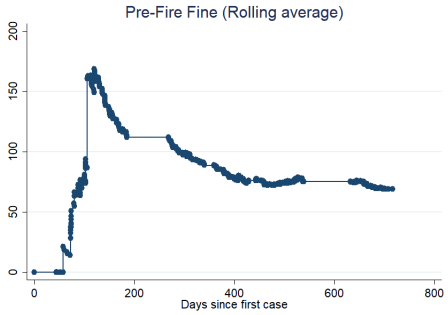

Figure C1.4: Pre-Fire improvements

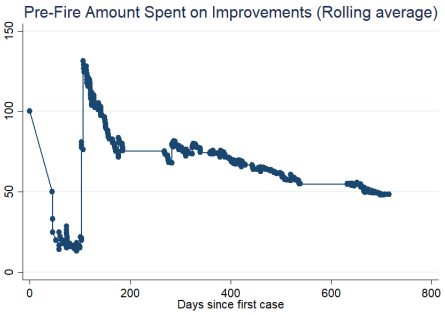

Figure C1.5: Degree from owner

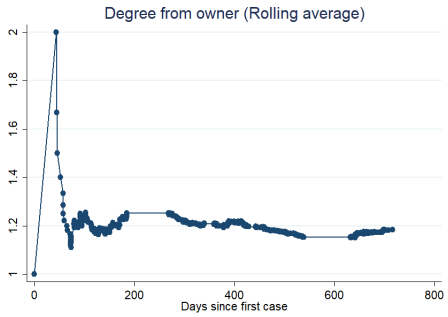

Figure C1.6: Number of parishes

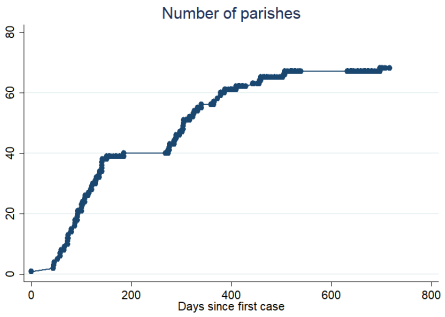

Figure C2: Binscatter of the effect of Fire on the number of hearths per property (All controls)

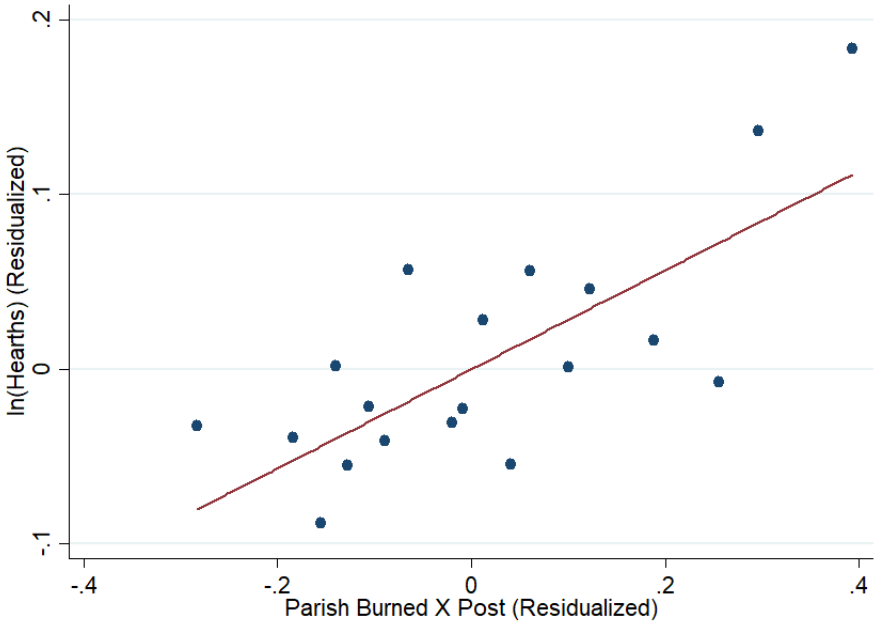

Figure C3: Binscatter of the effect of legal rulings on the number of hearths per property (All controls)

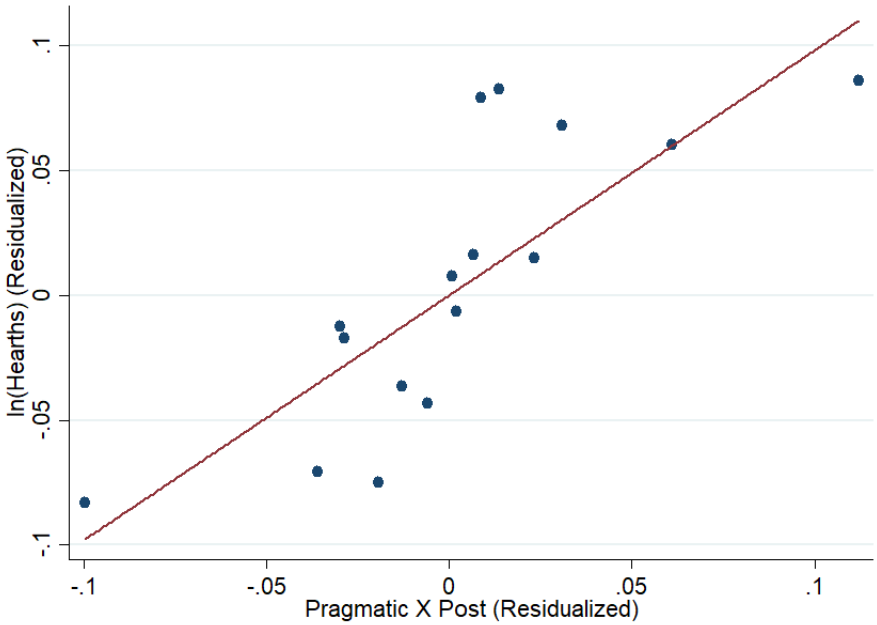

Figure C4: Binscatter of the first-stage (All controls)

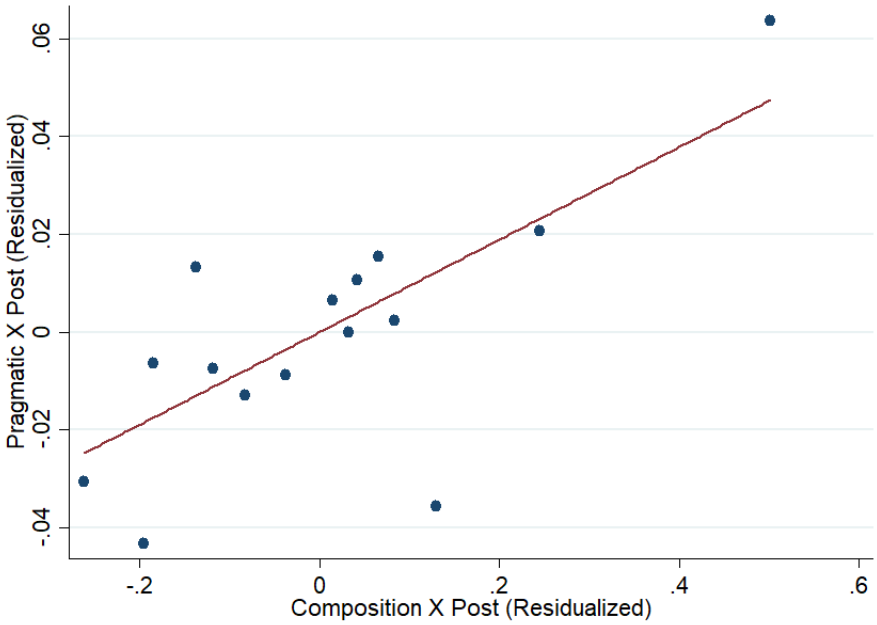

Figure C5: Binscatter of the reduced-form (All controls)

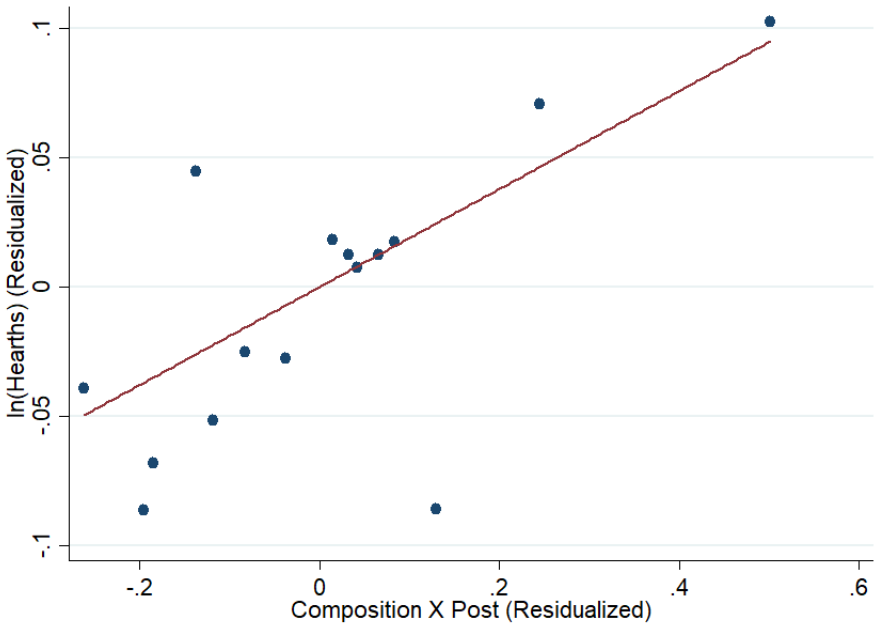

Table C1: Share of properties in each parish that went to the Fire Court

Parish	Cases	No. properties before Fire	Share
St Botolph Aldersgate	1	3969	0.000
St Giles Cripplegate	1	4967	0.000
St Andrew Holborn	4	1757	.002
All Hallows Staining	1	158	.006
St Antholin Budge Row & St John Walbrook	3	204	.015
St Bartholomew The Less	2	124	.016
St Mary Somerset & St Mary Mounthaw	4	223	.018
St Sepulchre Without Newgate	19	999	.019
All Hallows Barking	9	455	.02
Whitefriars Precinct	4	204	.02
St Bride Fleet Street	34	1614	.021
St Alphage London Wall	4	174	.023
St Martin Ludgate	6	241	.025
Holy Trinity The Less & St Michael Queenhithe	6	226	.027
St Andrew Hubbard & St Mary At Hill	7	255	.027
St Benet Pauls Wharf & St Peter Pauls Wharf	8	298	.027
St Mary Staining & St Michael Wood Street	3	112	.027
St Martin Vintry & St Michael Paternoster Royal	3	105	.029
St Alban Wood Street & St Olave Silver Street	8	257	.031
All Hallows The Great & All Hallows The Less	14	417	.034
St Mary Aldermary & St Thomas Apostle	4	109	.037
St Dunstan In The West	40	1001	.04
St Botolph Billingsgate & St George Botolph Lane	6	148	.041
St Gabriel Fenchurch Street & St Margaret Pattens	6	148	.041
St Swithin London Stone & St Mary Bothaw	7	171	.041
St Dunstan In The East	16	378	.042
Christchurch Newgate Street & St Leonard Foster Lane	24	468	.051
St Magnus The Martyr & St Margaret New Fish Street	12	235	.051
St Peter Le Poer	6	117	.051
St Nicholas Olave & St Nicholas Cole Abbey	6	107	.056
St Matthew Friday Street & St Peter Westcheap	7	117	.06
St Martin Pomeroy & St Olave Old Jewry	7	109	.064
St Michael Le Querne & St Vedast Foster Lane	17	238	.071
St Andrew By The Wardrobe & St Anne Blackfriars	12	167	.072
St Lawrence Jewry & St Mary Magdalen Milk Street	17	231	.074
St Mary Colechurch & St Mildred Poultry	8	108	.074
St Mary Magdalen Old Fish Street	5	68	.074
St Clement Eastcheap & St Martin Orgar	5	65	.077
St Mary Aldermanbury	13	153	.085
St Mary Le Bow & All Hallows Honey Lane & St Pancras Soper Lane	20	194	.103
St Margaret Moses & St Mildred Bread Street	12	107	.112
St Stephen Walbrook & St Benet Sherehog	15	109	.138
St Augustine Watling Street & St Faith Under St Paul	29	203	.143
St Gregory By St Paul	53	364	.146
St Lawrence Pountney & St Mary Abchurch	5	17	.294
All Hallows Bread Street & St John The Evangelist Friday Street	8	27	.296

Appendix D: Additional Results

Table D1: Comparing parishes before and after the Fire (burned vs unburned)

VARIABLES	(1) ln(No. hearths)	(2) ln(No. hearths)
Parish Burned	0.101	0.311***
	(0.084)	(0.070)
Observations	42,174	34,919
R-squared	0.141	0.197
Parish controls	✓	✓
Broader location controls	✓	✓
Number of clusters	70	70
Sample	Pre-Fire	Post-Fire

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D2: Effect of Fire on the number of hearths per property (Dropping parishes which merged after the Fire)

VARIABLES	(1)	(2)	(3)	(4)
	ln(No. hearths)			
Parish Burned X Post Fire	0.241**	0.224**	0.222*	0.277**
	(0.105)	(0.109)	(0.129)	(0.120)
Observations	69,466	69,466	69,466	69,466
R-squared	0.007	0.008	0.008	0.010
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	40	40	40	40

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D3: Effect of Fire on the number of hearths per property
(Using different control groups - Nearby sample)

VARIABLES	(1)	(2)	(3)	(4)
	ln(No. hearths)			
Parish Burned X Post Fire	0.336***	0.296***	0.327***	0.392***
	(0.094)	(0.080)	(0.116)	(0.121)
Observations	48,103	48,103	48,103	48,103
R-squared	0.013	0.021	0.022	0.025
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	61	61	61	61

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D4: Effect of Fire on the number of hearths per property
(Using different control groups - Further away sample)

VARIABLES	(1)	(2)	(3)	(4)
	ln(No. hearths)			
Parish Burned X Post Fire	0.225**	0.136	0.137	0.236*
	(0.105)	(0.134)	(0.151)	(0.137)
Observations	62,466	62,466	62,466	62,466
R-squared	0.007	0.009	0.010	0.015
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	60	60	60	60

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D5: Effect of Fire on the number of hearths per property
(Applying the inverse hyperbolic sine transform to hearths)

VARIABLES	(1)	(2)	(3)	(4)
	ln(No. hearths)			
Parish Burned X Post Fire	0.124	0.101	0.056	0.086
	(0.112)	(0.134)	(0.159)	(0.159)
Observations	79,730	79,730	79,730	79,730
R-squared	0.002	0.007	0.008	0.009
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	70	70	70	70

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D6: Effect of Fire on the number of hearths per property
(Using a Poisson pseudo-likelihood regression)

VARIABLES	(1)	(2)	(3)	(4)
	No. hearths			
Parish Burned X Post Fire	0.129	0.103	0.070	0.101
	(0.088)	(0.108)	(0.129)	(0.121)
Observations	79,730	79,730	79,730	79,730
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	70	70	70	70

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D7: Effect of Fire on the number of hearths per property
(Trimming extreme values of the outcome variable)

VARIABLES	(1)	(2)	(3)	(4)
	ln(No. hearths)			
Parish Burned X Post Fire	0.124**	0.101*	0.086	0.103
	(0.056)	(0.055)	(0.065)	(0.065)
Observations	64,402	64,402	64,402	64,402
R-squared	0.004	0.005	0.005	0.006
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	70	70	70	70

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D8: Comparing parishes before and after the Fire (by legal rulings)

VARIABLES	(1)	(2)
	ln(No. hearths)	
Pragmatic	-0.036	0.769**
	(0.289)	(0.304)
Observations	21,017	10,565
R-squared	0.140	0.196
Parish controls	✓	✓
Broader location controls	✓	✓
Number of clusters	46	46
Sample	Pre-Fire	Post-Fire

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D9: Effect of legal rulings on the number of hearths per property
(Adding in other dimensions of the rulings as controls)

VARIABLES	(1)	(2)	(3)	(4)
	ln(No. hearths)			
Pragmatic X Post Fire	1.050***	1.031***	0.926***	0.835***
	(0.294)	(0.306)	(0.249)	(0.224)
Avg. change in tenancy length X Post Fire	0.003	0.000	-0.000	-0.001
	(0.003)	(0.002)	(0.002)	(0.002)
Avg. change in rent X Post Fire	-0.016**	-0.013*	-0.012**	-0.009*
	(0.007)	(0.007)	(0.006)	(0.005)
Observations	31,582	31,582	31,582	31,582
R-squared	0.018	0.026	0.027	0.031
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	46	46	46	46

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D10: Effect of legal rulings on the number of hearths per property
(Dropping parishes which merged after the Fire)

VARIABLES	(1)	(2)	(3)	(4)
	ln(No. hearths)			
Pragmatic X Post Fire	2.128***	2.038***	1.917***	2.025***
	(0.657)	(0.249)	(0.304)	(0.409)
Observations	24,384	24,384	24,384	24,384
R-squared	0.024	0.037	0.037	0.037
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	17	17	17	17

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D11: Effect of legal rulings on the number of hearths per property (Applying the inverse hyperbolic sine transform to hearths)

VARIABLES	(1)	(2)	(3)	(4)
	ln(No. hearths)			
Pragmatic X Post Fire	1.122**	1.091***	1.015***	0.915***
	(0.422)	(0.338)	(0.246)	(0.254)
Observations	32,383	32,383	32,383	32,383
R-squared	0.009	0.017	0.018	0.021
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	46	46	46	46

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D12: Effect of legal rulings on the number of hearths per property (Using a Poisson pseudo-likelihood regression)

VARIABLES	(1)	(2)	(3)	(4)
	No. hearths			
Pragmatic X Post Fire	0.921**	0.934***	0.885***	0.784***
	(0.424)	(0.285)	(0.210)	(0.206)
Observations	32,383	32,383	32,383	32,383
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	46	46	46	46

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D13: Effect of legal rulings on the number of hearths per property (Trimming extreme values of the outcome variable)

VARIABLES	(1)	(2)	(3)	(4)
	ln(No. hearths)			
Pragmatic X Post Fire	0.852***	0.709***	0.663***	0.620***
	(0.220)	(0.193)	(0.151)	(0.148)
Observations	25,965	25,965	25,965	25,965
R-squared	0.010	0.015	0.015	0.016
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	46	46	46	46

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D14: First-stage by different subsamples

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Pragmatic X Post Fire					
Majority royalist in judging panels X Post	0.135***	0.046	0.038	-0.010	0.179***	0.125**
	(0.048)	(0.049)	(0.062)	(0.060)	(0.032)	(0.051)
Observations	20,737	10,845	1,726	8,419	21,437	19,984
Parish FE	✓	✓	✓	✓	✓	✓
Post FE	✓	✓	✓	✓	✓	✓
Sample	Church not destroyed	Church destroyed	Abutting walls	Within walls	Outside walls	Hearth tercile 1
Number of clusters	11	35	5	33	8	14

VARIABLES	(7)	(8)	(9)	(10)	(11)	(12)
	Pragmatic X Post Fire					
Majority royalist in judging panels X Post	0.036	0.133**	0.022	-0.096	0.159***	-0.011
	(0.097)	(0.054)	(0.099)	(0.064)	(0.036)	(0.072)
Observations	4,094	7,504	3,094	5,470	23,018	6,690
Parish FE	✓	✓	✓	✓	✓	✓
Post FE	✓	✓	✓	✓	✓	✓
Sample	Hearth tercile 2	Hearth tercile 3	Size tercile 1	Size tercile 2	Size tercile 3	Peers tercile 1
Number of clusters	16	16	19	17	10	21

VARIABLES	(13)	(14)	(15)	(16)	(17)	(18)
	Pragmatic X Post Fire					
Majority royalist in judging panels X Post	0.153***	0.159***	0.073	0.166***	0.062	0.188***
	(0.035)	(0.043)	(0.069)	(0.024)	(0.065)	(0.021)
Observations	17,727	7,165	16,383	15,199	19,135	12,447
Parish FE	✓	✓	✓	✓	✓	✓
Post FE	✓	✓	✓	✓	✓	✓
Sample	Peers tercile 2	Peers tercile 3	Doctors quantile 1	Doctors quantile 2	Military quantile 1	Military quantile 2
Number of clusters	11	14	27	19	39	7

Notes: All regressions include parish FEs and post FE. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D15: Reduced-form – Effect of Royalist majority on the number of hearths per property

VARIABLES	(1)	(2)	(3)	(4)
	ln(No. hearths)			
Majority royalist in judging panels X Post	0.270***	0.221**	0.213***	0.189**
	(0.098)	(0.087)	(0.069)	(0.074)
Observations	31,582	31,582	31,582	31,582
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	46	46	46	46

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D16: IV – Effect of legal rulings on the number of hearths per property (Adding in other dimensions of the rulings as controls)

VARIABLES	(1)	(2)	(3)	(4)
	ln(No. hearths)			
Pragmatic X Post Fire	2.198*	2.483**	2.141**	1.951**
	(1.152)	(1.058)	(0.835)	(0.866)
Avg. change in tenancy length X Post Fire	0.004	0.001	0.001	-0.000
	(0.004)	(0.003)	(0.002)	(0.002)
Avg. change in rent X Post Fire	-0.005	-0.004	-0.006	-0.003
	(0.016)	(0.011)	(0.009)	(0.008)
Observations	31,582	31,582	31,582	31,582
R-squared	0.012	0.017	0.022	0.027
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	46	46	46	46
KP F-stat	6.427	3.548	8.863	10.63

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D17: IV – Effect of legal rulings on the number of hearths per property (Dropping parishes which merged after the Fire)

VARIABLES	(1)	(2)	(3)	(4)
	ln(No. hearths)			
Pragmatic X Post Fire	2.065***	1.886***	1.904***	1.945***
	(0.549)	(0.404)	(0.361)	(0.441)
Observations	24,384	24,384	24,384	24,384
R-squared	0.024	0.036	0.037	0.037
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	17	17	17	17
KP F-stat	16.12	7.611	40.30	25.22

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D18: IV – Effect of legal rulings on the number of hearths per property (Applying the inverse hyperbolic sine transform to hearths)

VARIABLES	(1)	(2)	(3)	(4)
	ln(No. hearths)			
Pragmatic X Post Fire	1.866***	2.271**	1.909**	1.712*
	(0.636)	(0.942)	(0.719)	(0.872)
Observations	32,383	32,383	32,383	32,383
R-squared	0.005	0.010	0.015	0.019
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	46	46	46	46
KP F-stat	10.03	3.831	9.195	10.43

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D19: IV – Effect of legal rulings on the number of hearths per property (Trimming extreme values of the outcome variable)

VARIABLES	(1)	(2)	(3)	(4)
	ln(No. hearths)			
Pragmatic X Post Fire	1.663***	1.688***	1.560***	1.490***
	(0.386)	(0.556)	(0.438)	(0.439)
Observations	25,965	25,965	25,965	25,965
R-squared	0.001	0.005	0.008	0.010
Parish FE	✓	✓	✓	✓
Post FE	✓	✓	✓	✓
Parish controls X Post FE		✓	✓	✓
Broader location X Post FE			✓	✓
Pre-fire hearth tercile X Post FE				✓
Number of clusters	46	46	46	46
KP F-stat	12.07	4.716	9.581	10.83

Notes: Parish controls include the number of properties in the parish before the Fire, the share of peers, high-ranking military personnel and doctors living in the parish. Standard errors are clustered at the parish level. Notation for statistical significance: *** p<0.01, ** p<0.05, * p<0.1.