Mortality in the Long Shot: Lasting Legacies of Screening the "Birth of a Nation"*

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Abstract

This paper investigates the influence of being born in an environment of racial hate and violence induced by media on later-life mortality outcomes. We examine the infamous case of the "Birth of a Nation" movie that premiered in 1915. The movie was a cornerstone in American cinema, a megahit budget success, and seen by tens of millions of people. It offered a fictional reconciliation story of the American Civil War at the expense of re-subjugating African-Americans and glorifying white supremacy. The movie resulted in race riots, racial hate, racial crime, lynching, and enforcing social and economic discrimination against the black population. We employ Social Security Administration death records linked with the 1940 census and show that in-utero and early-life exposure to the post-screening era in showing counties is associated with 6.5 months lower longevity for males. Further, we find that these reductions are concentrated among people of lower socioeconomic status families, those born in non-southern states, and the subsample of non-migrants. Moreover, we find reductions in education and income during adulthood that may operate as mechanism channels.

Keywords: Mortality, Longevity, Racial Violence, Early-Life Exposures, Media

JEL Codes: I10, I14, J15, L82, N31, N32

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1. Introduction

An old and hotly debated strand of research has explored the role of media on societal attitudes, behaviors, and outcomes in various settings (DellaVigna & La Ferrara, 2015; Meyrowitz, 1986; Rizzica & Tonello, 2021). An important line of this research suggests a link between media and racial violence (Ang, 2023; Johnson et al., 1997; Lindo et al., 2022). Media may build stereotypes by selectively reporting historical facts and normalizing violence against specific subpopulations. It may also pick up traces of existing beliefs and biases and enhance racial animosity. One prominent example of such influences of media on racial hate is the infamous case of D.W. Griffith's "The Birth of a Nation" movie that premiered in 1915. The movie's plot offered a revisionist retelling of American Civil War history by distorting historical information, depicting black individuals as the main cause of the war, and glorifying white supremacy with the heroic portrait of the Ku Klux Klan. Despite its bigoted and exclusionary content, the movie was a megahit and commercial success. It is estimated that about 50 million people had watched the movie by 1930, roughly 41% of the country's population (Stokes, 2007). Ang (2023) shows that counties that premiered the movie experienced a spike in race riots and lynchings. In the long-run, counties that screened the movie reveal higher rates of hate crimes and hate groups almost a century later.

On the other end, a growing body of evidence points to the relevance of in-utero and early life conditions for lifecycle outcomes (Almond et al., 2018; Almond & Currie, 2011; Barker, 1994, 1995, 1997; Hayward & Gorman, 2004). This literature posits that shocks during critical stages of development change the trajectory of health outcomes and can be detected in later-life outcomes, including old-age health and mortality. Racial violence is a prominent example of these adverse shocks. Several studies show that exposure to racial discrimination and hostility is associated with adverse birth outcomes among infants of sufferers as well as negative impacts on a wide range of children's outcomes, including socioemotional development, cognitive ability, and mental health (Bécares et al., 2015; Guantai & Kijima, 2020; Lauderdale, 2006; Rosenthal & Lobel, 2011). A narrow strand of this research also examines the influence of racial violence on long-term mortality outcomes. For instance, several studies show that counties that experienced historical lynching events have higher rates of contemporary infant mortality a century later, have lower life expectancy, reveal higher interracial homicides, and experience higher mortality rates specifically among African-Americans (Abbott et al., 2022; Kihlström & Kirby, 2021; Messner et al., 2016;

Probst et al., 2019). Vu et al. (2023) document that prenatal exposure to lynching events is associated with lower longevity among black males during adulthood and old-age.

The current study extends these two strands of research by providing an empirical evaluation of the influence of early life exposure to the "Birth of a Nation" on later-life old-age mortality outcomes. Our empirical method relies on the timing and location of the screening of the movie and the fact that the resulting racial violence associated with the movie screening was concentrated among African-Americans. We employ recently released Social Security Administration death records to examine the long-run effects of exposure to the movie on male individuals' longevity. The death records are linked to the full count 1940 census at the individual level and allow us to employ historical census linking rules to track individuals and their families in historical censuses 1900-1930, to infer county of birth/childhood. We find that exposure to the post-screening era in showing counties during in-utero and early-life is associated with significant reductions in age at death of black individuals compared with whites and all unexposed children. For instance, full exposure (i.e., being born after the movie screening in the so-called *Roadshow* counties) is associated with 6.5 months lower longevity. These effects are considerably larger in the subsample of black children in low socioeconomic status families, and those born in non-southern counties. The effects become smaller in size when we look at black individuals who migrated from their county of birth by the year 1940 and appear larger for the sample of non-migrants, suggesting a mitigating influence of migration for adverse early life exposures. Exposed individuals reveal lower socioeconomic measures and wage income in 1940. They are also less likely to be enlisted and drafted for World War II. Among those people who were drafted, exposed black individuals are more likely to be underweight based on their body mass index measure.

We show the robustness of these results to a wide range of tests. First, the fact that we observe extremely small coefficients among nonblack individuals lends credibility to our claim that the effects run through the exposure of black individuals to racial violence rather than picking up on secular trends of treated counties. Second, we document the robustness to a battery of alternative specifications, alternative functional forms, alternative measures of exposure, and alternative difference-in-difference estimations. Third, we show that there were no significant associations between the exposure measure and the sample sociodemographic and socioeconomic characteristics that could otherwise explain the observed reductions in longevity. Fourth, we employ an instrumental variable strategy to account for endogeneity and document even larger

coefficients resulting from this method, suggesting that the main results may be biased downward and provide a floor for the true effects. Fifth, a series of event-study analyses rule out the concern that the impacts ride on pre-existing trends in longevity and reveal that the effects rise in magnitude for early life exposure among black individuals rather than older ages at exposure or white children.

This paper makes three contributions to the literature. First, this is the first study to examine the long-term impacts of screening of the "Birth of a Nation" on health outcomes. The movie was an unprecedented budget success and claimed to be the first blockbuster of American cinema (Stokes, 2007). It is noteworthy that the movie premiered in an era when there were fewer alternative media (e.g., radio and television). Fewer substitute media could mean a larger influence of the movie's content on the impressionable minds of people, specifically young individuals. The movie facilitated the second rise of the Ku Klux Klan and spread white supremacy ideas among the public. While the reconciliation narrative of the movie reunited white populations of the South and the North, it exacerbated the prejudice and discrimination against African-Americans in public, the social environment, and the labor force. Therefore, it brought a colossal change in the living landscape of black populations. Surprisingly, little evidence exists to investigate the influence of these changes on the long-term health outcomes of black people. Second, this paper also contributes to the ongoing and growing literature that evaluates the effects of in-utero, early-life, and childhood conditions on later-life health and mortality outcomes. Studies have linked various early life exposures and insults with old-age longevity, including pesticide exposure, environmental disasters, economic conditions, policy environment, and other adversities (Aizer et al., 2016; Fletcher & Noghanibehambari, 2023, 2024; Lindeboom et al., 2010; Noghanibehambari & Engelman, 2022; Noghanibehambari & Fletcher, 2023a, 2023b, 2023c, 2024; Scholte et al., 2015; Van Den Berg et al., 2006). The current paper contributes to this literature by studying the potential influence of racial violence and racial discrimination experienced in early life on later-life health and old-age mortality outcomes. Third, this paper also contributes to policy debates related to the role of media on societal behavior (Rizzica & Tonello, 2021). While many of these studies usually focus on short-term outcomes, our study points to the long-run impacts of media on health outcomes. Although the case of the "Birth of a Nation" was an exceptionally rare and extreme case of racial prejudice content in an era in which people were receptive and even in need of a reconciliation narrative (no matter how fictional it may be), it still bears implications for the

potential influence of media today, specifically given the fact that the effects are observed many decades after the screening of the movie.

The rest of the paper is organized as follows. Section 2 provides a short background on the "birth of a nation" movie. Section 3 discusses data sources and sample selection strategy. Section 4 introduces the econometric method. Section 5 reviews the results. Section 6 concludes the paper.

2. A Short Background on the "Birth of A Nation"

During the late 19th century, the reconciliationist narratives of the Civil War prevailed among elite groups, especially among the defeated Southerners. These stories glorified white supremacy and attempted to reunite the South and the North by offering salvation and redemption to white Southerners at the expense of re-subjugating African Americans. These verbal Chronicles of the Civil War and Reconstruction era were later depicted in the novel "The Clansman: A Historical Romance of the Ku Klux Klan" by Thomas Dixon in 1905. The novel was later adopted by the young and talented D.W. Griffith, the son of an ex-Confederate colonel, into a feature-length movie. Despite its fictional and biased retelling of American history and its racist story, the "Birth of A Nation" was a landmark in the history of art. It was the first feature-length movie in American cinema. In addition to advancing the techniques and technology to produce a feature-length film, the production team invented and introduced a series of techniques that continue to be fundamental to cinema in contemporary times, including close-up, long-shot, and fade-in and fade-out in editing stages.

The movie was premiered in February 1915 in Los Angeles. It was an inconceivable budget success. Recent estimates suggest the movie generated a cash revenue of up to $100M, equivalent to $2.6B in 2020 dollars, which makes the movie among the top five highest sales of all time (Monaco, 2009). It was seen by tens of millions of Americans and created controversies, debates, and riots across many cities. The National Association for the Advancement of Colored People (NAACP) launched a campaign to ban the movie. Several cities and the state of Kansas finally prohibited the screening of the movie. Many civil rights activists criticized the movie in local newspapers and public debates. However, in many other cities, the movie caused race riots and lynching and facilitated the rebirth of the old Ku Klux Klan.

3. Data and Sample

The primary source of data is the Social Security Administration Death Master Files (DMF). The DMF data covers deaths that occurred to male individuals between the years 1975–2005.[4] The data is extracted from the Censoc Project (Goldstein et al., 2021). The data reports the exact date of birth and date of death, allowing for calculating longevity. Although this variable is an extreme measure of health during adulthood and old age, it provides an accurate assessment of health condition which makes it superior to other subjective self-reported measures of health. Further, several studies provide a link between longevity and other economic and social outcomes, making this measure relevant for policies in several settings (Murphy & Topel, 2006).

The Censoc-DMF data is linked to the full count 1940 census at the individual level (extracted from Ruggles et al. (2020)). It provides an initial sample of roughly 8 million observations which allows for a large sample size after necessary sample selections. This large sample size is in sharp contrast with alternative data sources that offer only several thousand observations (also linkable to the 1940 census), such as the Health and Retirement Study. The linked data allows us to observe individual and family-level characteristics as reported in the 1940 census, which we employ in our regressions.

Since our main treatment variable, i.e., screening of the "birth of the nation", is measured at the local area and we assign this treatment during early-life ages, we need a reliable proxy for the county of birth. We implement three strategies to infer the county of birth. First, we link the DMF data to the Numerical Identification death records (the so-called Numident data) extracted from the Censoc Project (Goldstein et al., 2021). The Numident data has the disadvantage of a considerably narrower death window compared with DMF (1988-2005 versus 1975-2005, respectively) but reports county of birth as recorded in the Social Security Administration. Therefore, we are able to identify the county of birth exactly as reported in death records. Using this method, we can identify the county of birth for about 49% of observations. Second, we use cross-census linking rules provided by Price et al. (2021) to link DMF-1940-census data to historical censuses 1900-1920. We then use the county of 1900-1920 censuses as a proxy for

[4] In Appendix C, we employ Berkeley Unified Numident Mortality Database (BUNMD) data replicate the main results. The BUNMD has a more limited death window compared to the DMF data and is not linked to the 1940 census. However, it reports death to both males and females. We find that the effects are considerably larger among male individuals, by a factor of 3.4.

county of birth for successfully linked individuals (roughly 37% of initial observations). Third, for the remaining 13% of observations, we use the county of 1940 as a proxy for the county of birth.[5]

We extract the information on screening of the "Birth of a Nation" from Esposito et al. (2023). The data reports the number of times the movie was screened in each county and in each year-month. We aggregate the screening counts at the county-by-year level and merge them with our DMF data based on county and cohort/age as explained below. We restrict the sample to individuals born between 1905 and 1937 to have several cohorts in late years of childhood when showings start, several cohorts who experience the showings during in-utero and early-life, and several cohorts who are born post-showing era.[6] Further, we restrict the sample to non-Hispanic Blacks and non-Hispanic whites.

The primary exposure variable in our regressions calculates the share of early life ages (0-3 years) that the child, who was born in the showing county, was exposed to post-showing era. For instance, for a child that turned age 1 at the time the movie was screened and thus exposed to the new era during ages 1-3, exposure takes a value of 0.75. Similarly, exposure is assumed to be zero for children who turn age 4 and older at the time of showing. Further, the exposure equals one for children born post-screening in showing counties. Although the choice of ages 0-3 seems arbitrary, it is based on the empirical results of investigating the impacts across different ages at exposure. We explain the respective method for such analysis in section 4 and review the results in section 5.1. Moreover, in Appendix A, we show the robustness of the results for expanding and contracting the bandwidth of early-life years.

The top panel of Figure 1 depicts the screening of the movie across counties. In Appendix F, we show the time-series of showings across counties and states. In most cases, the showing was short-lived, usually with a start and end date within the same month. The bottom panel of Figure 1 illustrates the spatial distribution of age at death based on county of birth. Summary statistics of the final sample are reported in the two panels of Table 1 for individuals with zero exposure to the screening of the movie and individuals who were born in counties in which the movie was

[5] Although this method might raise concerns regarding endogeneity of being in the linked sample, in Appendix E, We simply use county of 1940 as a proxy for county of birth and observe a considerable drop in the main results, suggesting that the measurement errors of such a naïve proxy could underestimate the true effects.

[6] One concern in this cohort selection is the potential influence of the infamous Spanish flu of 1918-1919. In Appendix B, we show that the results are robust to the inclusion of a control variable for influenza mortality for a subsample of cities with available data.

screened. The sample covers roughly 5.6 million observations. The average exposure measure in screening counties was 0.73. Screening counties have a slightly higher share of whites (94.3% versus 93.1%) and a lower share of Blacks (5.2% versus 6.6%). Individuals born in screening counties have a slightly lower share of literate mothers than all other cohorts (68.5% versus 72%).

4. Econometric Method

Our identification method exploits three sources of variation: spatial-temporal variation in the movie screening and the fact that the resulting social, emotional, and economic pressure associated with the movie adversely affected African-Americans and not other racial groups. The latter variation has a theoretical background with studies showing the negative short-run and long-run impacts of racial violence on race-specific outcomes (Abbott et al., 2022; Mujahid et al., 2021; Vu et al., 2023). We operationalize this method using the following ordinary least square estimation:

$$y_{icst} = \alpha_0 + \alpha_1 Black_i \times Exposure_{cst} + \alpha_2 Exposure_{cst} + \alpha_3 Black_i + \alpha_4 X_i + \gamma_{st} + \lambda_c \times T + \varepsilon_{icst} \quad (1)$$

The outcome is age-at-death of individual i was born in county c in state s in year t. The variable $Exposure$ measures the share of early life ages (0-3 years) that the child was born in showing county was exposed to the post-showing era.[7] In X, we include individual and family characteristics, including dummies for ethnicity, dummies for maternal socioeconomic status, and dummies for maternal literacy. To account for all other state-level policies that affect long-run outcomes, we include state-by-year fixed effects (represented by γ). Therefore, the variation comes from comparing the same cohorts born in the same state but in counties with differential exposure to the screening of the movie. To control for all unobserved time-invariant county characteristics, we include county fixed effects (represented by λ). To account for the secular evolution of county characteristics across birth cohorts, we also include a county-specific linear trend in birth year. Finally, ε is a disturbance term. Standard errors are clustered on county.

To complement this analysis, we implement event studies to observe the evolution of longevity by race in showing versus non-showing counties, as follows:

[7] Although in the main results we use a continuous variable for BON, in Appendix A, we show the robustness to alternative functional forms of measuring BON.

$$y_{icst} = \alpha_0 + Black_i \times \sum_{j \neq -1} \phi_j S_{cs} \times I(t - t^*_{cs} = j) + \sum_{j \neq -1} \Phi_j S_{cs} \times I(t - t^*_{cs} = j) + \alpha_1 X_i + \gamma_{st} + \lambda_c \times T + \varepsilon_{icst} \quad (2)$$

In this specification, S is a dummy variable indicating showing county, i.e., equals one for individuals whose county of birth experienced the movie screening and zero otherwise. The function $I(.)$ Is a unit function that turns on if the inside argument is true. As in equation 1, t indexes birth cohort. t^*_{sc} represents the county-specific year in which the movie was screened for the first time.Therefore, the parameters ϕ_j represent changes in the longevity of black individuals in showing counties versus non-showing counties who were born in different years relative to the first screening of the movie. Similarly, parameters Φ reveal the evolution of longevity of whites in showing versus not showing counties across different cohorts relative to the first showing in the county of birth.

Although the event study provides a transparent evolution of effects over the years, it does not take into account age at exposure to the screening and post-screening era as well as cases in which the county experienced recurrent showings. To examine such impacts, we reformulate the event study to examine the associations between age at post-screening exposure and longevity, as follows:

$$y_{icst} = \alpha_0 + Black_i \times \sum_{j=-5}^{9} \zeta_j I(BON_{cs,t+j} > 0) + \sum_{j=-5}^{9} \xi_j I(BON_{cs,t+j} > 0) + \alpha_1 X_i + \gamma_{st} + \lambda_c \times T + \varepsilon_{icst} \quad (3)$$

Where BON measures the total number of showings in the county of birth in the year the individual turned age j. $I(.)$ indicates whether the individual was exposed to any showing at age j. All other parameters are the same as in Equation 1.

5. Results

5.1. Event Study

The event study results of Equation 2 are reported in the top panel and bottom panel of Figure 2. Prior to the movie screening in the county, we do not observe discernible and significant pre-existing patterns and trends in the coefficients for both Blacks (top panel) and whites (bottom

panel). However, we find significant reductions in longevity for those who were born after the movie screening among black populations (top panel) while no significant and consistent movement among white individuals (bottom panel). In Figure 3, we replicate the event study results using the method developed by Sun & Abraham (2021) to address the potential endogeneity and the OLS-produced coefficients. We observe a quite comparable set of coefficients in the overall pattern and the estimated effects.

We report the fully parameterized regression results of Equation 3 in Figure 4. Although the exposure measure in this formulation is defined slightly differently than equation 2, the overall pattern of coefficients is consistent with the event study results of Figure 2. We find a reduction in longevity of black individuals who were exposed to the screening during in utero, early life, and childhood up to age 2. The fact that we observe similar reductions in longevity for exposure in several years prior to birth suggests that the negative influences triggered by the screening of the movie were long-lasting and remained in the county for many post-screening years. We do not find significant, consistent, and discernible changes in the longevity of whites.

5.2. Main Difference-in-Difference Results

The main results of the paper are reported in Table 2. We start with a parsimonious specification that includes only county and birth-year fixed effects in column 1. We then add fixed effects, trends, and covariates across consecutive columns. We observe a very robust set of coefficients across specifications. The fully parametrized model of column 5 suggests that full exposure is associated with a 6.5-month reduction in longevity among Blacks. The respective coefficient of $Exposure$ among whites points to about 0.46 months increases in longevity, a quite small and statistically insignificant effect.

The observed reduction in longevity among Blacks is a nontrivial change that can be better understood if we put it into perspective. In column 5, the coefficient of black represents the black versus white differences in longevity, conditional on fixed effects and controls. Therefore, the observed reduction in longevity (6.5 months) is roughly 86% of the black-white gap in longevity.

We can also understand the magnitude of the effects by comparing them with other early life shocks documented in the literature. For instance, Aizer et al. (2016) investigate the long-run effects of eligibility for a cash transfer program in early childhood. The Mother Pension (MP) program offered a relatively generous welfare benefit to poor single mothers over three years. The

benefits were equivalent to roughly 30-40 percent of mothers' pre-transfer income. They show that the MP resulted in 11.6 months of increases in longevity. Therefore, the effects of BON as calculated above is roughly 56% of the effects of receiving a 3-year generous transfer under MP. Noghanibehambari & Fletcher (2024) examine the early life exposure to the Dust Bowl of the 1930s on old-age mortality. They find intent-to-treat effects of about a 1-month reduction in longevity. Noghanibehambari & Fletcher (2023d) examine the effects of exposure to the 1918 influenza pandemic during childhood on later-life male mortality. They find that children residing in high-exposure areas between ages 0-11 experience reductions of about 7-11 months in longevity during adulthood and old age. Vu et al. (2023) examine the influence of in-utero exposure to lynching on later life longevity and find a reduction of roughly 3.7 months. All these effects that are associated with important and large-scale shocks provide a benchmark for the effects of the post-BON era and point to the economic significance of our findings.

Another way to put the results into perspective is by using the concept of Value of Statistical Life (VSL). While the VSL estimates differ by study, cohort, and location, some studies suggest a VSL of roughly $10M (Colmer, 2020; Kniesner & Viscusi, 2019). The average age-at-death in the final sample is 874 months. Therefore, each month of life has a value equivalent to about $11.44K. Using the estimated effect of Table 2, we can estimate a $74.37K loss due to reductions in longevity for each black individual. In the full count 1940 census, there are roughly 1,099,430 black individuals in screening counties with a non-zero exposure to the BON who belong to the same cohorts as in the final sample. A back-of-an-envelope calculation suggests a $81.76B loss due to reductions in longevity of black people because of their exposure to the BON.

5.3. Heterogeneity Analysis

In Table 3, we investigate the heterogeneity in the effects across different subsamples. Studies suggest that the impact of early life shocks and exposures may have differential impacts across families of different socioeconomic statuses and different parental education (Almond et al., 2018). In columns 1-2, we examine such potential heterogeneities based on paternal socioeconomic index. We observe larger coefficients among black children of lower socioeconomic status fathers (-5.5 vs -3.9). These differences may suggest that the racial violence is concentrated in local areas in which lower socioeconomic status families reside or that the higher socioeconomic status families may mitigate the negative early-life shocks by providing better

resources for their affected children. However, in columns 3 – 4, we find a relatively larger interaction coefficient for children of literate mothers versus illiterate mothers.

Further, many race riots specifically the reemergence of the KKK occurred in the southern states. If the effects are due to the enforced racial violence, one would expect to observe larger impacts for individuals in the South. On the other hand, the impacts among the southern black population could operate at the margin on top of a wide range of already established discriminations and institutional segregations. Therefore, one would expect smaller effects for southern states and counties. In columns 5-6 of Table 3, we explore the heterogeneity by birth region. We observe a reduction of about 3 months for black people who were born in the South and 8.4 months for those who were born in non-South regions. Although both interaction terms are economically and statistically significant, the larger effects among non-South regions are consistent with our latter expectation.

Another potential source of heterogeneity is driven by the influence of migration on later-life outcomes. There is evidence that migration out of adversities brings short-term and long-term benefits for migrants, which may have spillover impacts on their health and longevity (Black et al., 2015; Collins, 2021; Derenoncourt, 2022). In column 1 of Table 4, we examine the impacts on the likelihood of migrating based on the comparison between county of birth and county of residence in 1940. We observe very small coefficients. Although the negative signs suggest a reduction in the probability of migration among both Blacks and whites, their implied changes with respect to the mean of the outcome are quite small. However, when we examine the heterogeneity in the effects based on the subsamples of non-migrants and migrants in columns 2-3, we observe larger coefficients in the subsample of non-migrants. The interaction term for the subsample of migrants suggests a reduction of about 3.2 months, while the respective coefficient of non-migrants implies a significant drop of about 5.7 months. This comparison points to the potential role of migration in mitigating adverse early-life shocks.

5.4. Robustness Checks

In Table 5, we examine the robustness of the results across alternative specifications. To have a benchmark comparison, we report the results of column 5 of Table 2 in column 1 of Table 5. In column 2, we interact county-of-birth fixed effects with 1940-county fixed effects to control for the role of migration. The results are quite comparable to those of column 1. In column 3, we

add State Economic Area (SEA) by birth year fixed effects. The SEA is a below-state geographic identifier that contains several counties that are similar in terms of economic characteristics. Therefore, the variation in column 3 specification is driven by the comparison of cohorts born in the same SEA and year but in counties with differential $exposure$. This specification absorbs all unobserved sociodemographic and socioeconomic changes related to local areas that are common across counties within the same SEA-year. We observe almost identical effects to those of column 1. In this model, the main effect of exposure becomes larger in magnitude and statistically significant.

In column 4, we allow for fixed effects of counties to have differential impacts on longevity based on parental socioeconomic status and literacy. Specifically, we interact county fixed effects with father socioeconomic status and maternal literacy dummies. We observe an almost identical interaction term compared to that of column 1.

Studies document the influence of seasonality in birth on later-life health and longevity (Vaiserman, 2021). Further, there is evidence of seasonality in mortality associated with changes in season-specific pollution and other climatic variables (Marti-Soler et al., 2014). We control for these potential confounders by including month-of-birth fixed effects and month-of-death fixed effects. These results are reported in column 5. We observe very similar coefficients compared to column 1.

In columns 6-9, we examine the robustness of alternative functional forms. We replace the outcome with the log of age at death in column 6. Full exposure among Blacks is associated with a 0.8% reduction in longevity. This change is very similar to the implied percentage change of the coefficient in column 1 with respect to the mean of the outcome (0.74%). In columns 7-9, we replace the outcome with dummy variables indicating longevity beyond ages 65, 70, and 75, respectively. Compared with the mean of outcomes, full exposure among Blacks is associated with 3.5%, 4.7%, and 4.1%, respectively. Therefore, one possible conclusion is that the effects appear to rise as we reach older ages.

In columns 10-11, we explore the robustness of standard errors to alternative correction methods. In column 10, we use Huber-White robust standard errors to correct for heteroscedasticity. In column 11, we employ a two-way clustering by state and birth year to correct

for both spatial and serial correlations in error terms. We observe fairly robust standard errors compared with those of column 1.

In column 12, we remove non-showing counties to exploit only cross-cohort and cost age variation in exposure. The interaction term drops by about 15% but remains economically and statistically significant.

One concern in interpreting the main results is the influence of endogenous merging between death records and the 1940 census. For instance, if individuals in the linked sample are different from others based on characteristics that correlate with our exposure measure, the resulting coefficients might pick up on these unobserved differences. We empirically test this concern in Appendix D. In so doing, we start from the original cohorts of 1910-1920 in the full count 1940 census and merge them with the final sample of the paper. We then generate a dummy variable indicating successful merging. We regress this indicator on BON measures, conditional on covariates and fixed effects. We find that exposed black individuals are indeed less likely to be in the merged sample. However, the documented associations are very small in magnitude which rules out the role of endogenous merging in driving the results. To further examine this issue, we implement the Heckman two-step estimate (Heckman, 1979). This method estimates the successful merging indicator (from the sample of Appendix D) as a function of observable characteristics in the first-stage regression. Based on the first stage estimation, it calculates an Inverse Miller Ratio (IMR) which is used as a covariate in the second stage regression (i.e., longevity equation). Therefore, it controls for selection bias due to data linking based on observables. The results are reported in column 13. We observe a comparable interaction coefficient compared to that of column 1.

Finally, we use Accelerated Failure Time regressions assuming an exponential distribution in the outcome. The results, reported in column 14, provide virtually identical coefficients to those of column 6 that employ a logarithmic outcome but estimates using ordinary least squares.

5.5. Balancing Tests

Another concern in the results is that screening counties are inherently different from non-screening counties. This becomes problematic in our identification strategy if exposed cohorts (i.e., exposed during ages 0-3 to post-screening era for those born in showing counties) have characteristics that are different from non-exposed cohorts and those characteristics correlate with

their health and longevity. We can empirically test this concern by regressing a series of pre-determined observable characteristics on exposure, conditional on fixed effects and trends. These results are reported in Table 6. We observe very small coefficients across different outcomes. For instance, full exposure is associated with roughly 0.38%, 0.41%, 2.3%, and 0.67% change in the share of whites, Individuals with father's socioeconomic index below-median, individuals with father's socioeconomic index above-median, and children of literate mothers (columns 1, 4, 5, and 7, respectively). The results of this table do not provide convincing evidence that changes in the sample sociodemographic and socioeconomic characteristics as a result of exposure to BON may confound our estimates.

5.6. Two-Stage Least-Square Estimations

Esposito et al. (2023) examine the effects of BON (as a reconciliation narrative) on cultural convergence, public discourse, and military enlistment. They employ an instrumental variable strategy using the fact that screening of movies in the early 20th century was limited to places that had the infrastructure of exhibiting film reels and other logistical and technological limitations. Therefore, movie screenings followed a recurring pattern. They use the "Million Dollar Mystery" movie that was screened 281 days prior to the "Birth of A Nation" as an instrument that varied across counties and over time. Using their suggested instrumental variable, we report the two-stage least-square estimations in Table 7. The interaction term suggests a 9.2-month reduction in longevity. This estimate implies that possible unobserved confounders may bias the OLS coefficients downward and our reduced form estimates provide a floor for the true effects. The Local Average Treatment Effect (LATE) of this regression is driven by the variation in locations with screening infrastructure prior to the movie showing. Therefore, another interpretation is that the impacts are considerably larger in these locations compared with no infrastructure counties. For instance, areas with higher urbanization were more likely to possess the required infrastructure for movie screening and these areas were more concentrated in non-southern states (Boustan et al., 2018). Based on the results of Table 3 that the effects are larger and non-southern regions, it is not surprising to observe larger (LATE) effects in the two-stage least-square estimations.

5.7. Mechanisms

Several studies provide a link between longevity and measures of education and socioeconomic status (Chetty et al., 2016; Fletcher, 2015; Halpern-Manners et al., 2020; Lleras-

Muney, 2005; Lleras-Muney et al., 2020). Therefore, these measures offer a pathway between early-life exposures and later-life longevity. We examine these pathways in columns 1-4 of Table 8 using the information in the 1940 census. In column 1, we observe increases in the probability of having less than 12 years of education for exposed black individuals. Full exposure is associated with 141 basis-points change in the outcome, equivalent to a roughly 2.3% change. In column 2, we observe a similar increase in the probability of having less than a college education (i.e., education less than or equal to 12 years).

The results also suggest significant reductions in wage income. Full exposure to BON is associated with a reduction of about 51.2% in wage income (column 3). In column 4, we find that a one-unit change in $exposure$ (going from zero to one) results in a reduction of about 1.3 income percentiles. Chetty et al. (2016) examine the income-longevity relationship using death records and tax returns data over the years 1999-2014 and find that an additional income percentile is associated with roughly 1.6 months reduction in longevity among males. Combining their estimate and the interaction term of column 5 of Table 8, we calculate a reduction of about 2 months in longevity for a full exposure to BON. This is about 32% of the reduced form effect of Table 2. Therefore, although a considerable portion of the effects run through changes in socioeconomic status during adulthood, there could be other pathways at play.

One of the advantages of the DMF data is that it can be linked to the World War II Enlistment data. This data is useful as it reports limited anthropometric measures: height and weight (hence the Body Mass Index (BMI)). In column 6, we examine the association between BON exposure measures and the probability of being in the enlistment data. We find insignificant reductions in being in the World War II enlistment data among both Blacks and whites. In column 6, we examine the effects on a dummy variable that indicates being underweight.[8] Exposed black individuals are significantly more likely to be categorized as underweight. Full exposure to BON is associated with 48 basis points increases in the probability of being underweight. This is equivalent to a 21% change from the mean of the outcome. We do not observe a significant change among whites. Further, we observe reductions in the Army General Classification Test (AGCT) score. The AGCT measures the cognitive ability of enlistees, including their verbal, mathematical,

[8] The BMI is calculated as weight (in Kg) divided by height (in meter) squared. Underweight is a dummy variable that equals one if the BMI is less than 18.5.

and reasoning skills. The interaction term suggests roughly 1% drop in AGCT score, although it is noisy and limits further interpretation.

Another possible mechanism relates to the lasting impact of maternal stress and deteriorating mental health during pregnancy as a result of social bigotry and pressure. Although this link is difficult to test empirically due to data scarcity, several studies point to the role of maternal mental health and infants' outcomes (Carlson, 2015; Duncan et al., 2017; Kim et al., 2017). One way to detect such deteriorations is to examine the evolution of infant mortality rates. Despite the fact that infant mortality is a complex function of many factors, studies suggest a link between maternal mental health and stress and the probability of their infants' survival (Catalano et al., 2005; Class et al., 2013; Pike, 2005). County-level infant mortality rates are available from 1915 for a small subset of highly populated counties. This data is extracted from Manson et al. (2017). We link these data with the BON data at the county and year level and limit the sample to the years 1915-1930. To have an empirical method similar to that of equation 1, we calculate the share of Blacks in each county using the 1910 census and generate a dummy variable that indicates the county is at the top-quartile of the share of Blacks. We then interact this *top-quartile black* dummy with a dummy variable indicating BON screening and use it as the main independent variable. We regress measures of infant mortality rate and birth rate on these variables, conditional on county fixed effects, state-year fixed effects, and county trend. These results are reported in Table 9. Screening of the movie in top-quartile black counties is associated with about 5.5 infant deaths per 1,000, off a mean of 75.8 (column 1). We observe smaller effect size when we examine the log of infant mortality rate (column 2). The interaction coefficient of column 2 suggests an increase in infant mortality rate in top quartile black counties by about 3.8%. The results of column 3 and 4 suggests reductions in fertility in these counties. This fact may partly reflect parental fertility decisions but it could also reveal the higher rates of miscarriage, stillbirth, and incidences of fetal death. However, we do not observe significant associations between the movie screening and measures of infant mortality and fertility in bottom-three quartiles of share of black counties.

Another testable channel of impact relates to the possible enforcement of labor market discrimination against African Americans. Such discrimination could result in lowering labor market outcomes of the affected populations and can be detected in their socioeconomic measures. Worsening socioeconomic conditions of parents may have adverse and long-lasting impacts on their children's health outcomes, specifically for their later-life longevity (Hayward & Gorman,

2004; Montez & Hayward, 2011, 2014; Smith et al., 2009). We test this channel using the full count 1900-1930 censuses. Since we are interested in the outcomes of adult individuals, we refine the exposure measure and empirical method as follows. For each county, we calculate the total annual screening of the movie and assign this value to individuals residing in that county. We then examine the changes in race specific outcomes in these counties in census years 1920-1930 versus 1900-1910 in the following regressions:

$$y_{ict} = \alpha_0 + \alpha_1 Post_t \times Black_i \times BON_c + \alpha_2 Post_t \times Black_i + \alpha_3 Post_t \times BON_c \quad (4)$$
$$+ \alpha_4 Black_i \times BON_c + \alpha_5 Black + \alpha_6 X_i + \xi_c + \gamma_t + \varepsilon_{ict}$$

Where i, c, and t represent individual, county, and year. The variable $post$ indicates 1920-1930 years. The main effect of this variable is absorbed by year fixed effects (represented by γ). The variable BON is a dummy indicating screening county, i.e., the movie was ever screened in the county or not. The main effect of this variable is absorbed in county fixed effects (represented by ξ). In X, we include dummies for age (in single year), ethnicity, and gender.

The results are reported in Table 10.[9] Among black people in screening counties during post-screening years, we observe reductions in socioeconomic index and occupational prestigious score (columns 1 and 2, respectively). However, we observe positive coefficients among whites with a significant interaction term of $Post \times BON$ for occupational prestigious score, suggesting the overall improvements in their socioeconomic conditions in screening counties post-screening. To put these results into perspective, we can compare them with the overall black versus nonblack difference in outcomes (in non-screening counties in 1900-1910). For instance, the gap in occupational prestigious score among Blacks versus nonblacks is 7.1 units. The triple interaction term is roughly 6% of this gap. These reductions in socioeconomic measures could translate into increased mental pressure among parents, decreased availability of vital resources such as nutrition, and reductions in health-related resources such as healthcare, all of which are documented to influence infants' and children's short run and long-run health outcomes (Currie & Goodman, 2020; Lindeboom et al., 2010; Lindo, 2011; Vaiserman, 2014).

[9] We should note that we use the population of male individuals in full count censuses (1900-1930) between ages 25-45 for the analysis of socioeconomic measures and the population of female individuals between the same age range for the analysis of fertility.

Another channel of impact is parental fertility decisions as a response to the new environment created by the screening of the movie. We examine this channel using the same method as in equation 4 and report the results in column 3 of Table 10. We observe a significant reduction in the number of children among black parents (aged 25-45) in screening counties post-screening years. We do not observe a significant change in fertility of white individuals in screening counties in the periods after the movie screening. In non-screening counties post-screening, black individuals even reveal significant increases in fertility, which is in sharp contrast with reductions of fertility in screening counties. Although we observe a significant triple interaction term, the estimated coefficient is small compared to the mean of the outcome. It implies a change of about 0.8% with respect to the mean.

6. Conclusion

In recent years, a growing body of evidence has investigated the role of in-utero and early-life exposures and later-life outcomes, specifically for cases of old-age health and mortality. This literature suggests that variations in contemporary mortality and longevity can partly be explained by experiences during early life and childhood. These findings contain vital policy implications considering the aging population in the US and the increasing share of the elderly population in the coming decades (Mather & Scommegna, 2024; Rice & Fineman, 2004). Our study joined this literature by providing evidence of the long-lasting effects of being born in an environment of racial hate and violence.

We investigated the legacies of the Hollywood infamous blockbuster the “Birth of A Nation”. The widespread reception of the movie across the nation in an era with fewer alternative media in addition to its narrative that offered a reconciliation story of the American Civil War made the movie highly popular and influential. However, its fictional and racist plot depicted the war as a result of racial inferiority and the presumed threat of African Americans. The screening of the movie was associated with race riots, enforcing social and economic discrimination against the black population, lynching, and the rise of Ku Klux Klan membership.

We showed that black individuals were exposed to the post-screening era in showing counties experienced considerable reductions in longevity during adulthood and old age. Employing Social Security Administration death records linked with the 1940 census, we documented that full exposure to the post-screening era during early life and childhood is

associated with 6.5 months lower age-at-death. Further, we provided evidence that the effects are considerably larger among black individuals of lower socioeconomic status families and in non-southern counties. Moreover, we show that the effects are larger among those people who have stayed in their county of birth up to 1940 and smaller for the sample of migrants. Using the 1940 census, we found that exposure is associated with significant reductions in education and wage income. Using the World War II enlistment data, we found increases in the prevalence of being underweight (for their height) among black people with exposure to the movie during their first year of life.

Male life expectancy in the US increased from 46.3 years to 60.8 years for birth cohorts of 1900 to 1940. Our results suggested that exposure to the post-screening era in showing counties is associated with 6.5 months lower longevity. This change is equivalent to roughly 44.8% of the rise in life expectancy across these cohorts.

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Tables

Table 1 - Descriptive Statistics

	Non-Showing Counties		Showing Counties	
	Mean	SD	Min	Max
Death age (month)	873.897	122.053	874.953	121.429
Birth Year	1918.654	8.498	1918.702	8.422
Death Year	1991.483	8.572	1991.619	8.596
Black × Exposure	0	0	.038	.19
Exposure	0	0	.725	.446
White	.931	.254	.943	.231
Black	.066	.247	.052	.221
Other	.004	.062	.005	.07
Father Socioeconomic Status ≤ Median	.528	.499	.332	.471
Father Socioeconomic Status > Median	.354	.478	.524	.499
Father Socioeconomic Status Missing	.118	.322	.145	.352
Mother Literate	.72	.449	.685	.465
Mother Literate Missing	.236	.425	.26	.438
Observations	3,113,936		2,521,498	

Notes. The data covers cohorts of 1905-1937 who died between 1975-2005.

Table 2 - Exposure to Premiering the *Birth of a Nation* Movie at Year-of-Birth and Old-Age Longevity

	Outcome: Age-at-Death (Months)				
	(1)	(2)	(3)	(4)	(5)
Black × Exposure	-7.02182***	-6.64907***	-6.59756***	-6.59756***	-6.51561***
	(.98548)	(.96897)	(1.01568)	(1.01568)	(.99827)
Exposure	1.52404**	1.11736**	.48955	.48955	.46999
	(.62839)	(.56725)	(.41371)	(.41371)	(.41623)
Black	-7.82668***	-7.96441***	-8.00781***	-8.00781***	-7.62978***
	(.31602)	(.31055)	(.31247)	(.31247)	(.30388)
Observations	5635434	5635434	5635434	5635434	5635434
R-squared	.35096	.35204	.35264	.35264	.35267
Mean DV	874.370	874.370	874.370	874.370	874.370
Birth-County FE	✓	✓	✓	✓	✓
Birth-Year FE	✓	✓	✓	✓	✓
Birth-State-by-Birth-Year FE		✓	✓	✓	✓
Birth-County Trend			✓	✓	✓
Individual Control				✓	✓
Family Control					✓

Notes. Standard errors, clustered on county, are in parentheses. Individual controls include dummies for ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies.
**** p<0.01, ** p<0.05, * p<0.1*

Table 3 - Heterogeneity of the Effects of Exposure to Premiering the Birth of a Nation Movie at Year-of-Birth and Old-Age Longevity

	Outcome: Age-at-Death (Months), Subsamples:					
	Paternal Socioeconomic Index < Median	Paternal Socioeconomic Index ≥ Median	Mother Illiterate	Mother Literate	Birth-Region: South	Birth-Region: Non-South
	(1)	(2)	(3)	(4)	(5)	(6)
Black × Exposure	-5.56558***	-3.9421***	-5.38229***	-7.5994***	-3.05906***	-8.42647***
	(.82612)	(1.28745)	(1.0768)	(1.14936)	(.75293)	(1.30399)
Exposure	.53911	.53265	.95666*	.10223	-.37472	.45358
	(.36503)	(.54572)	(.5277)	(.4188)	(.55846)	(.5008)
Black	-7.0344***	-10.88946***	-7.81143***	-7.26609***	-6.8587***	-12.47231***
	(.36498)	(.81097)	(.45199)	(.35019)	(.32964)	(.78411)
Observations	2481118	2423130	2318516	3316917	1564448	4070582
R-squared	.35165	.34915	.30432	.33936	.36978	.34504
Mean DV	876.417	867.483	844.362	895.345	863.804	878.426

Notes. Standard errors, clustered on county, are in parentheses. All regressions include county fixed effects, birth-year fixed effects, birth-state by birth-year fixed effects, birth-county trend, individual controls, and family controls. Individual controls include dummies for ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies.
**** p<0.01, ** p<0.05, * p<0.1*

Table 4 - Heterogeneity of the Effects of Exposure to Premiering the Birth of a Nation Movie at Year-of-Birth on Old-Age Longevity by Migration from Birth-County to 1940-County

	Outcome: Migration	***Outcome: Age-at-Death (Months)***	
		Non-Migrants	Migrants
	(1)	(2)	(3)
Black × Exposure	-.00996	-5.67458***	-3.19193***
	(.01338)	(.74515)	(1.05142)
Exposure	-.00421**	.42202	2.32278***
	(.00208)	(.30766)	(.72361)
Black	-.02919***	-7.2729***	-6.09937***
	(.00323)	(.37985)	(.43971)
Observations	5635434	3836200	1799234
R-squared	.15389	.36422	.26019
Mean DV	0.319	859.549	905.971

Notes. Standard errors, clustered on county, are in parentheses. All regressions include county fixed effects, birth-year fixed effects, birth-state by birth-year fixed effects, birth-county trend, individual controls, and family controls. Individual controls include dummies for ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies.
**** p<0.01, ** p<0.05, * p<0.1*

Table 5 - Robustness Checks

	Outcome: Age-at-Death (Months)						
	Column 5 Table 2	Adding 1940-County by Birth-County FE	Adding State Economic Area (SEA) by Birth-Year FE	Adding Birth-County FE by Parental Dummies Interaction	Adding Birth-Month FE and Death-Month FE	Outcome: Log Age-at-Death	Outcome: Age-at-Death > 65
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Black × Exposure	-6.51561***	-6.1441***	-6.55867***	-6.48651***	-6.5095***	-.00802***	-.02788***
	(.99827)	(.79517)	(1.00316)	(.95599)	(.9953)	(.00126)	(.00367)
Exposure	.46999	.59155	1.34418**	.6497	.57078	.00035	.00235
	(.41623)	(.40805)	(.5242)	(.40085)	(.41958)	(.00044)	(.00185)
Black	-7.62978***	-7.24559***	-7.6159***	-7.86951***	-7.65885***	-.00951***	-.0202***
	(.30388)	(.3316)	(.30169)	(.31557)	(.30348)	(.00038)	(.00109)
Observations	5635434	5381981	5635428	5635433	5635434	5635434	5635434
R-squared	.35267	.37545	.35465	.3536	.35329	.35318	.20364
Mean DV	874.370	872.467	874.370	874.370	874.370	6.763	0.763

	Outcome: Age-at-Death > 70	Outcome: Age-at-Death > 75	Using Huber-White Robust Standard Errors	Two-Way Clustering on State and Birth-Year	Removing Non-Showing Counties	Heckman (1979) Estimate	Accelerated Failure Time Model
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Black × Exposure	-.02761***	-.01691***	-6.51561***	-6.51561***	-5.49117***	-5.31517***	-.00788***
	(.00362)	(.00282)	(.43413)	(1.35079)	(.85642)	(.46339)	(.00052)
Exposure	.00406	.0043*	.46999**	.46999	1.24149***	1.21362***	.00127***
	(.00275)	(.00244)	(.21491)	(.7259)	(.45382)	(.05954)	(.00022)
Black	-.02231***	-.02177***	-7.62978***	-7.62978***	-8.49291***	-6.29827***	-.00924***
	(.00121)	(.00116)	(.24234)	(.65437)	(.75504)	(.26813)	(.00029)
Observations	5635434	5635434	5635434	5635434	2521493	29253719	5635434
R-squared	.23077	.2135	.35267	.35267	.34823	---	---
Mean DV	0.585	0.394	874.370	874.370	874.953	874.370	874.370

Notes. Standard errors, clustered on county, are in parentheses. All regressions include county fixed effects, birth-year fixed effects, birth-state by birth-year fixed effects, birth-county trend, individual controls, and family controls. Individual controls include dummies for ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies.
**** p<0.01, ** p<0.05, * p<0.1*

Table 6 - Balancing Tests: Exposure to the Birth of a Nation Premiering and Individual Characteristics

	Outcomes:							
	White	Black	Other Race	Father SEI < Median	Father SEI ≥ Median	Father SEI Missing	Mother Literate	Mother Literate Missing
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposure	.00368***	-.00372***	.00005	.00181	.00995**	-.01176**	.00476	-.00707
	(.00077)	(.0007)	(.00026)	(.00197)	(.00436)	(.00502)	(.00426)	(.00476)
Observations	5635434	5635434	5635434	5635434	5635434	5635434	5635434	5635434
R-squared	.22953	.24264	.09277	.20532	.17082	.05759	.35232	.42693
Mean DV	0.936	0.059	0.004	0.440	0.430	0.130	0.704	0.247

Notes. Standard errors, clustered on county, are in parentheses. All regressions include county fixed effects, birth-year fixed effects, birth-state by birth-year fixed effects, and birth-county trend.
**** p<0.01, ** p<0.05, * p<0.1*

Table 7 - The Results of Two-Stage Least-Square Regressions

	Second-Stage Outcome: Age-at-Death (Months)	***First-State Outcome: BON***
	(1)	(2)
Black × Exposure	-9.23264***	
	(1.28497)	
Exposure	.53273	
	(.66387)	
Black	-6.78802***	
	(.38164)	
Million Dollar Mystery Screening		.49512***
		(.01608)
Observations	5634669	5634669
R-squared	.00055	.87953
Mean DV	874.363	0.324
First-Stage F-Stat	481.51	

Notes. Standard errors, clustered on county, are in parentheses. All regressions include county fixed effects, birth-year fixed effects, birth-state by birth-year fixed effects, birth-county trend, individual controls, and family controls. Individual controls include dummies for ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies.
**** p<0.01, ** p<0.05, * p<0.1*

Table 8 - Exploring Mechanisms Using Education-Income in 1940 and World War II Enlistment Data

	Outcomes:						
	Education <12	Education ≤ 12	Log Wage Income	Income Percentile	Being in Enlistment Data	Underweight	Army General Classification Test Score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Black × Exposure	.0141***	.00884***	-.51275***	-1.25314***	-.00155	.00486**	-.87136
	(.00467)	(.00322)	(.06784)	(.25937)	(.00257)	(.00223)	(.82565)
Exposure	-.00869***	-.00549***	.08771***	.48051***	-.00139	.00016	.41669
	(.00212)	(.00167)	(.02938)	(.13842)	(.00207)	(.00117)	(.84506)
Black	.21122***	.08968***	.02402	-10.31205***	-.01481***	-.02143***	-11.21694***
	(.00174)	(.00119)	(.02968)	(.12121)	(.0013)	(.00114)	(.54822)
Observations	2921137	2921137	2921137	2762389	5635403	848967	82803
R-squared	.06971	.03316	.05052	.12563	.11974	.01504	.17656
Mean DV	0.607	0.840	3.683	56.651	0.172	0.023	94.492

Notes. Standard errors, clustered on county, are in parentheses. All regressions include county fixed effects, birth-year fixed effects, birth-state by birth-year fixed effects, birth-county trend, individual controls, and family controls. Individual controls include dummies for ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies.

**** p<0.01, ** p<0.05, * p<0.1*

Table 9 - Exploring Mechanisms Using County-Level Vital Statistics Data

	Outcomes:			
	Infant Mortality Rate	Log Infant Mortality Rate	Birth Rate (per 1,000)	Log Birth Rate (per 1,000)
	(1)	(2)	(3)	
Top-Quartile Black County × BON	5.48702***	.03759**	-2.04739**	-.12608**
	(1.96452)	(.0183)	(.8643)	(.04935)
BON	.03311	.00069	.0112	-.00001
	(.10537)	(.00116)	(.01766)	(.00106)
Observations	26378	26219	26378	26378
R-squared	.83806	.80332	.96285	.96964
Mean DV	75.823	4.289	18.624	2.884

Notes. Standard errors, clustered on county, are in parentheses. All regressions include county fixed effects, year fixed effects, state by year fixed effects, and county trend. The sample covers the years 1915-1920. Since the dummy variable "Top-Quartile Black County" is at the county level, its main effect is absorbed by county fixed effects.
**** p<0.01, ** p<0.05, * p<0.1*

Table 10 - Exploring Mechanisms Using the Full-Count Censuses 1910-1920

	Outcomes:		
	Socioeconomic Index	Occupational Prestigue Score	Number of Children
	(1)	(2)	(3)
Black × BON × Post	-.55156**	-.42027**	-.02265**
	(.27083)	(.20485)	(.00966)
BON × Post	.04078	.11315*	-.00261
	(.05488)	(.05816)	(.0032)
Black × Post	-2.88187***	-.59885***	.0664***
	(.1943)	(.14205)	(.01115)
Black × BON	-4.04437***	-1.76925***	-.09494***
	(.70115)	(.43034)	(.02712)
Black	-10.00689***	-7.10814***	.12653***
	(.38228)	(.20652)	(.01466)
Observations	32094844	32068293	31258418
R-squared	.11937	.06516	.10925
Mean DV	26.683	36.068	2.740

Notes. Standard errors, clustered on county, are in parentheses. All regressions include county fixed effects, birth-year fixed effects, year fixed effects, state-by-year fixed effects, and county trend in year. The sample contains male individuals aged 20-60 in the full-count censuses 1910-1920. BON in this table is the annual average of screening of the movie at the county level. Therefore, the main effect of BON is absorbed in County fixed effect.

**** p<0.01, ** p<0.05, * p<0.1*

Figures

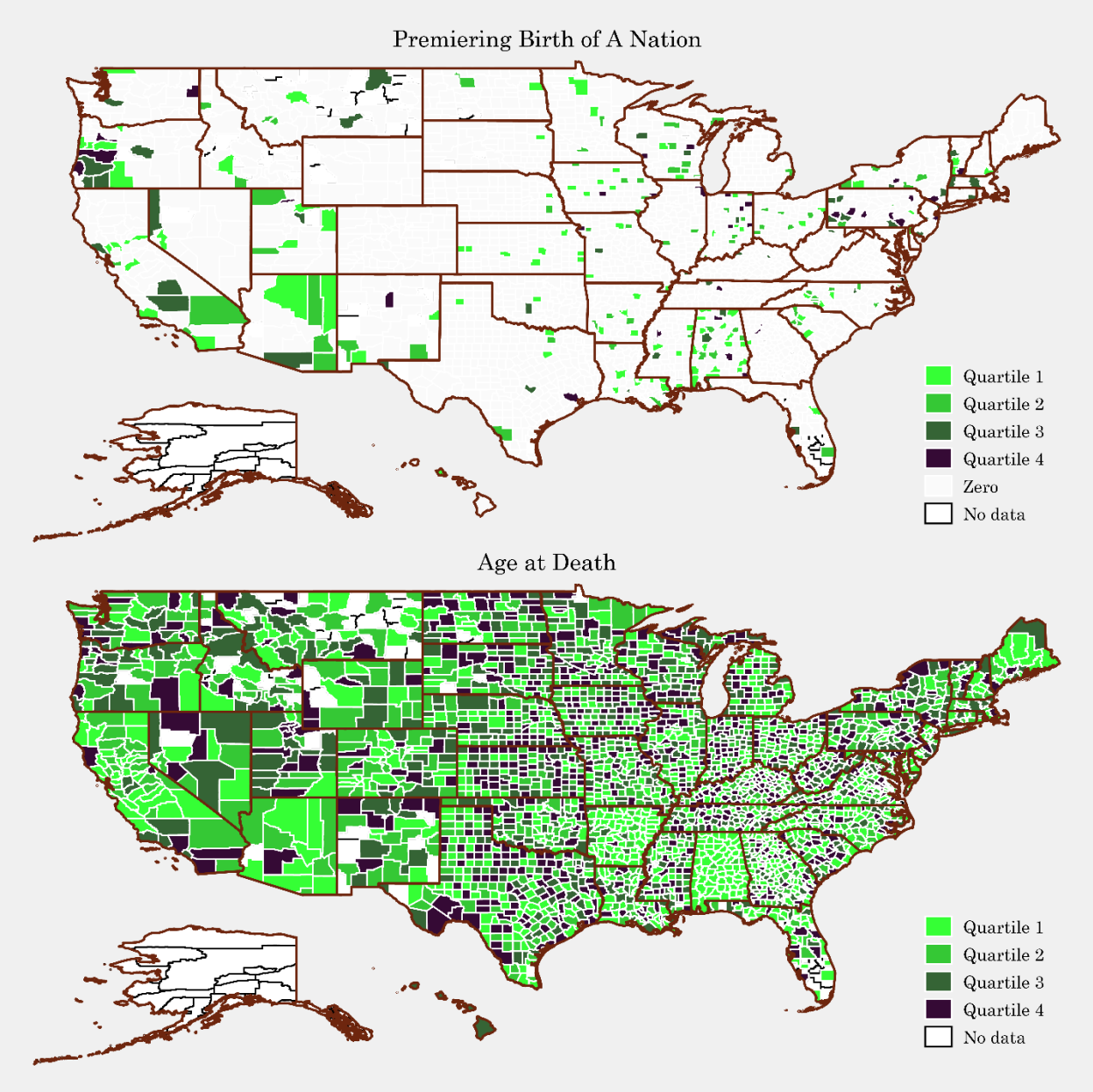


Figure 1 - Geographic Distribution of Birth of a Nation and Longevity across Counties

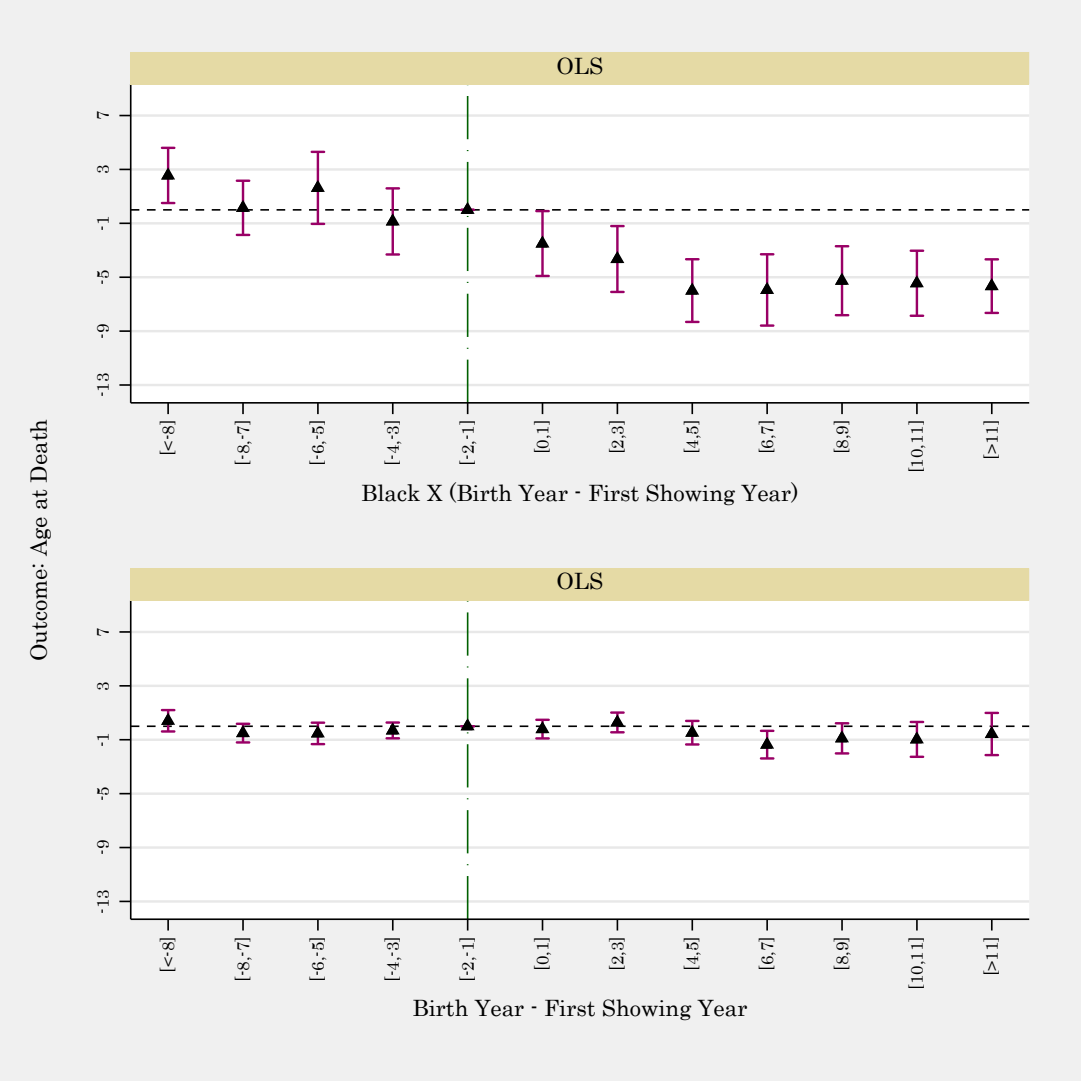


Figure 2 - Event Study Analysis to Explore the Effects of Exposure to Showing of the Birth of a Nation on Longevity Using (Sun & Abraham, 2021)

Notes. Point estimates and standard errors are depicted. Standard errors are clustered on county. All regressions include county fixed effects, birth-year fixed effects, birth-state by birth-year fixed effects, birth-county trend, individual controls, and family controls. Individual controls include dummies for race and ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies.

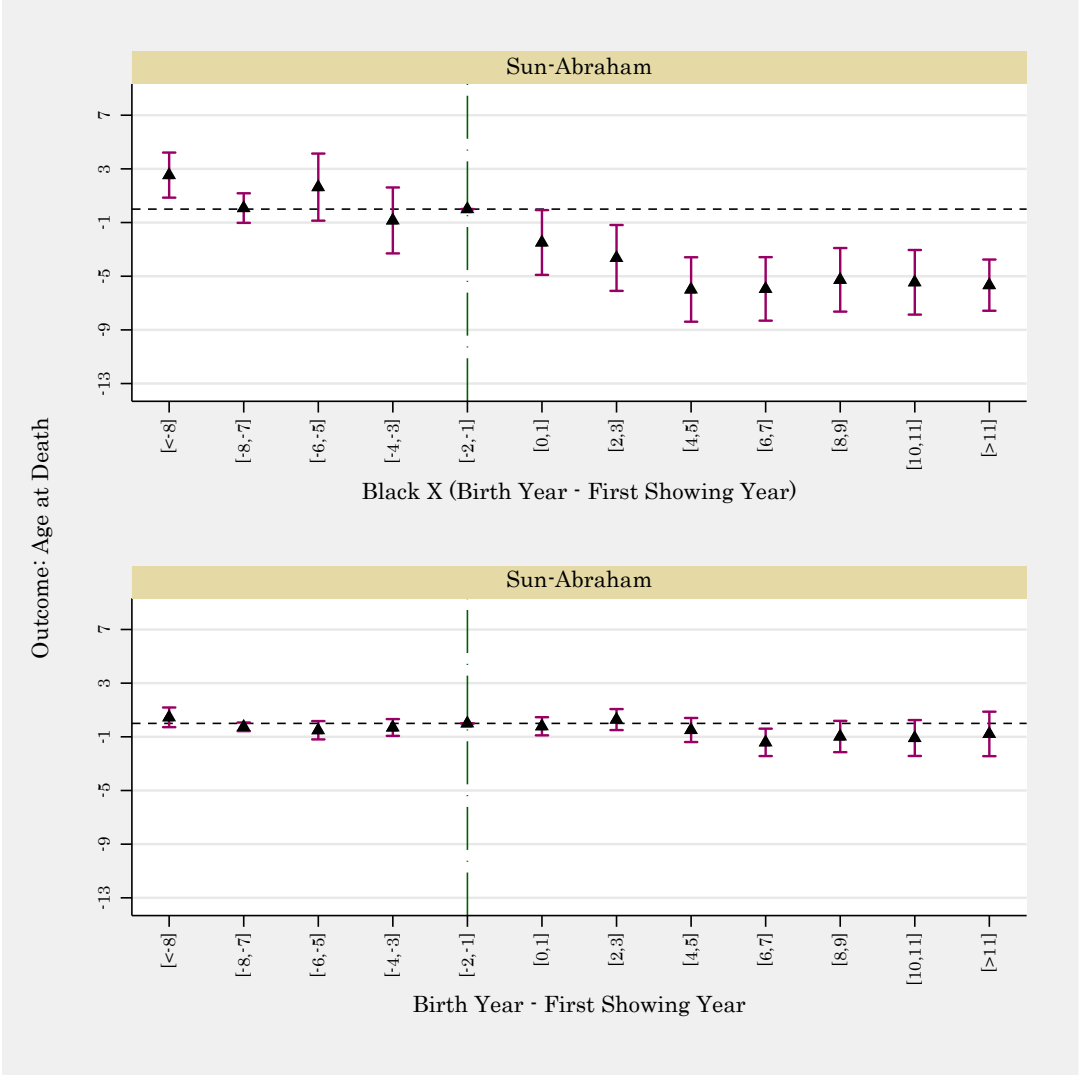


Figure 3 - Event Study Analysis to Explore the Effects of Exposure to Showing of the Birth of a Nation on Longevity Using (Sun & Abraham, 2021)

Notes. Point estimates and standard errors are depicted. Standard errors are clustered on county. All regressions include county fixed effects, birth-year fixed effects, birth-state by birth-year fixed effects, birth-county trend, individual controls, and family controls. Individual controls include dummies for race and ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies.

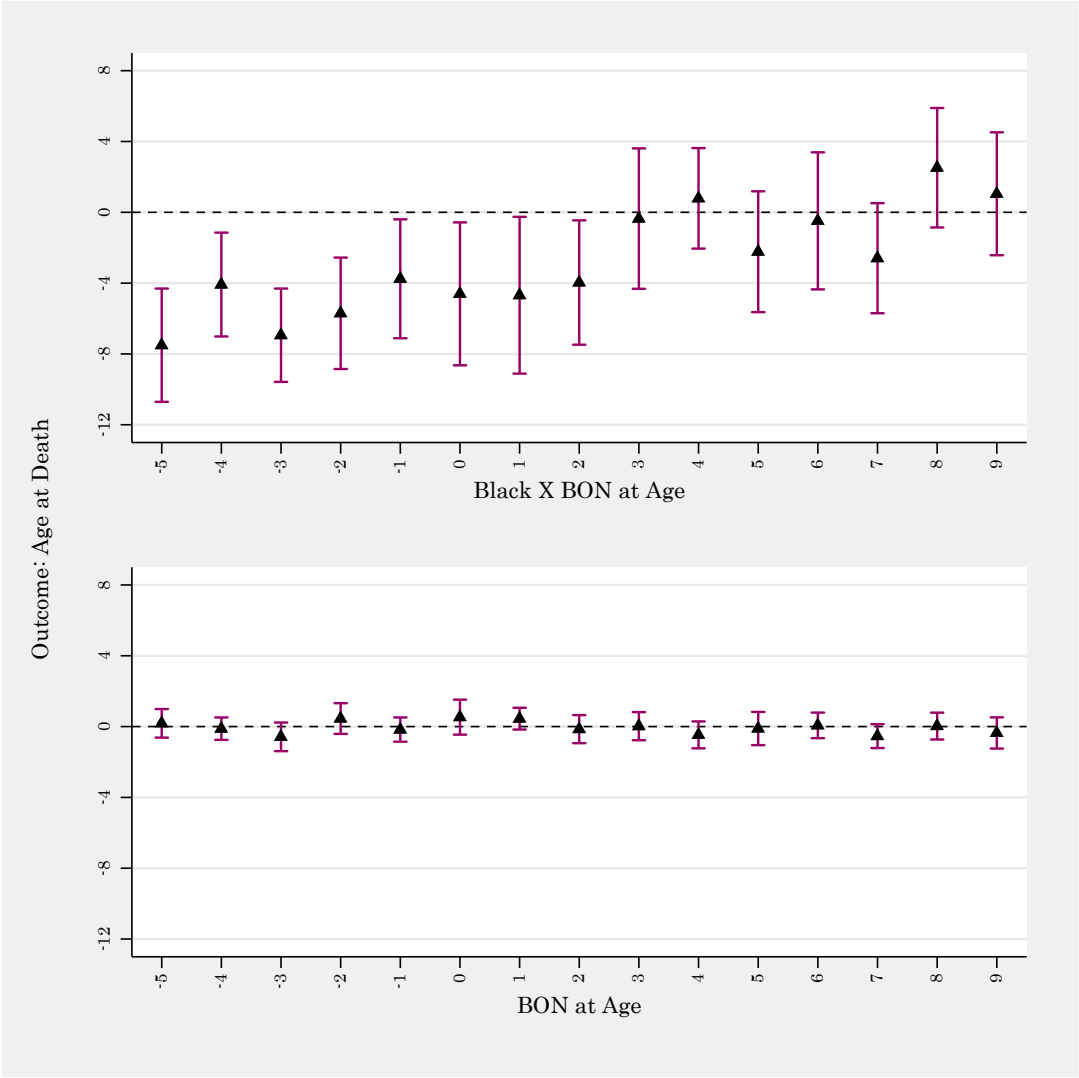


Figure 4 - Exposure to the Birth of a Nation across Different Ages and Old-Age Longevity

Notes. Point estimates and standard errors are depicted. Standard errors are clustered on county. All regressions include county fixed effects, birth-year fixed effects, birth-state by birth-year fixed effects, birth-county trend, individual controls, and family controls. Individual controls include dummies for race and ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies.

Appendix A

In this appendix, we examine the robustness of the results to functional form based on alternative measures of $Exposure$. First, we replace $Exposure$ with $log(Exposure)$ and replicate the main results in column 1 of Appendix Table A-1. Second, we replace the continuous $Exposure$ variable with a dummy variable that indicates the $Exposure$ being above median. We report these results in the second column of Appendix Table A-1. Third, instead of using the share of childhood ages (up to age 3) that the individual was exposed to the post-screening era, we use the standardized number of screenings prior to age 3 in the county of birth. Therefore, the independent variable measures change in outcome as a result of a one standard deviation rise in the number of screenings of the movie in the county of birth for all years leading to the year the individual turned age 3. Although the different natures of independent variables in this table make the results difficult to compare with the main results of the paper, the pattern in the estimated coefficients and their magnitude are consistent and comparable to the main results.

In the main results of the paper, we focused on exposure between ages 0-3. We chose this bandwidth based on the results of different ages of exposure reported in Figure 4. In Appendix Table A-2, we start with the bandwidth of ages 0-10 and slightly narrow it down across consecutive columns. We observe slight increases in coefficients as we focus more on earlier ages of exposure.

Appendix Table A-1 - Replicating the Main Results Using Alternative Measures

	Outcome: Age-at-Death (Months)		
	(1)	(2)	(3)
Black × Log Exposure	-2.1707***		
	(.38663)		
Log Exposure	.14068		
	(.21371)		
Black × Exposure above Median		-6.51561***	
		(.99827)	
Exposure above Median		.46999	
		(.41623)	
Black × Standardized Exposure			-4.69893***
			(1.30838)
Standardized Exposure			.0974
			(.22312)
Black	-12.51688***	-7.62978***	-10.17279***
	(.65814)	(.30388)	(.45696)
Observations	1828934	5635434	5635434
R-squared	.24257	.35267	.35267
Mean DV	858.704	874.370	874.370

Notes. Standard errors, clustered on county, are in parentheses. All regressions include county fixed effects, birth-year fixed effects, birth-state by birth-year fixed effects, birth-county trend, individual controls, and family controls. Individual controls include dummies for ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies.
**** p<0.01, ** p<0.05, * p<0.1*

Appendix Table A-2 - Replicating the Main Results Using Other Exposure Ages

	Outcome: Age-at-Death (Months)				
	(1)	(2)	(3)	(4)	(5)
Black × Exposure at Ages 0-10	-5.36047***				
	(.97673)				
Exposure at Ages 0-10	.8182				
	(.82905)				
Black × Exposure at Ages 0-5		-6.48467***			
		(1.04186)			
Exposure at Ages 0-5		.01579			
		(.39204)			
Black × Exposure at Ages 0-2			-7.32855***		
			(1.10497)		
Exposure at Ages 0-2			.67688		
			(.54547)		
Black × Exposure at Ages 0-1				-7.29254***	
				(1.10096)	
Exposure at Ages 0-1				.55545	
				(.45468)	
Black × Exposure at Age 0					-7.7038***
					(1.06078)
Exposure at Age 0					.31618
					(.40748)
Black	-6.92514***	-6.85993***	-6.90141***	-7.03234***	-7.03878***
	(.35886)	(.33256)	(.31979)	(.31669)	(.31454)
Observations	5019122	5019122	5019122	5019122	5019122
R-squared	.25392	.25393	.25394	.25393	.25394
Mean DV	891.091	891.091	891.091	891.091	891.091

Notes. Standard errors, clustered on county, are in parentheses. All regressions include county fixed effects, birth-year fixed effects, birth-state by birth-year fixed effects, birth-county trend, individual controls, and family controls. Individual controls include dummies for ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies.

**** p<0.01, ** p<0.05, * p<0.1*

Appendix B

The portion of treated courts in our final sample is also exposed to the infamous 1918-1919 Spanish flu during their early life. Although there is no empirical evidence that the spread of the movie and the spread of the disease might correlate, one may argue for its potential confounding influence in unobservable ways (e.g., City size that attracts both the screening of the movie and a higher concentration of the disease). We extract city-level influenza mortality rate due to the Spanish flu for the years 1918-1919 from Beach et al. (2022). We then merge this with our final sample based on the city of birth (which is inferred using the same method as county of birth, explained in section 3). The merged sample is considerably smaller in size than our final sample. In column 1 of Appendix Table B-1, we replicate the main results of the paper using this sample. The interaction term is only 12% smaller than the main results and remains economically and statistically significant, suggesting a 5.7-month reduction in longevity.

In column 2, we add city-level influenza mortality rate due to the Spanish flu for the years 1918–1919 interacted with birth year fixed effects to flexibly control for the influence of the disease across cohorts. We observe almost identical coefficients to those of column 1. In columns 3-4, we replicate the results for the subsample of below-median and above-median influenza mortality rates. Although we observe a smaller interaction coefficient in the subsample of above-median 1918-1919 influenza rate, the estimated effects are quite comparable to those of column 1.

Appendix Table B-1 - Examining Potential Confounding Influence of the Spanish Flu

	Outcome: Age-at-Death (Months)			
	Full-Sample	Full-Sample Adding City-Level 1918-1919 Influenza Rate	Below Median 1918-1919 Flue Rate	Above Median 1918-1919 Flue Rate
	(1)	(2)	(3)	(4)
Black × Exposure	-5.73431***	-5.73193***	-5.46272***	-4.4933***
	(1.30253)	(1.30335)	(2.02707)	(1.19635)
Exposure	1.60381*	1.60844*	5.26271***	-1.08922
	(.88984)	(.88941)	(1.67461)	(.67926)
Black	-10.55489***	-10.55514***	-12.8467***	-9.23739***
	(.76019)	(.75946)	(1.19084)	(.86252)
Observations	1978085	1978085	988867	988784
R-squared	.34315	.34316	.34668	.31942
Mean DV	874.861	874.861	859.148	890.576

Notes. Standard errors, clustered on county, are in parentheses. All regressions include county fixed effects, birth-year fixed effects, birth-state by birth-year fixed effects, birth-county trend, individual controls, and family controls. Individual controls include dummies for ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies.

**** p<0.01, ** p<0.05, * p<0.1*

Appendix C

The main analysis of the paper is based on DMF data that covers the deaths of male individuals only. However, the trajectory of outcomes after exposure could be different for males and females. In this appendix, we use alternative data that contains deaths of both genders. The Berkeley Unified Numident Mortality Database (so-called BUNMD) covers deaths of both males and females. Also, it reports county of birth which is a necessary variable in our research setting. The primary disadvantage of BUNMD is that the mortality information covers primarily the years of 1988-2007. Therefore, we lose many death years compared with the DMF data. Another disadvantage of the BUNMD data is that it cannot be linked to the 1940 census.

In Appendix Table C-1, we replicate the main results of the paper with the BUNMD data for males and females in panels A and B, respectively. The interaction terms suggest considerably larger effects among male individuals. Among Blacks, full exposure is associated with a 4.7 and 2.1 months reduction in longevity of males and females, respectively. The fact that the coefficient of males is smaller than the main results of the paper suggests that the right truncation of death records likely underestimates the true effects. The fact that the impacts are considerably larger among males is in line with the literature on the *fragile male hypothesis* and studies that show larger impacts of early life exposures on later life outcomes of male children.(Clark et al., 2021; Dratva et al., 2016).

Appendix Table C-1 - Replicating the Main Results Using BUNMD Data

	Outcome: Age-at-Death (Months)		
	(1)	(2)	(3)
Panel A. Males			
Black × Exposure	-4.79027***	-4.34615***	-4.30851***
	(.63277)	(.65719)	(.6571)
Exposure	2.86382***	-.06573	.43488
	(1.03969)	(.50902)	(.51891)
Black	-13.77712***	-14.01646***	-14.0201***
	(.32383)	(.31582)	(.31595)
Observations	7354545	7354545	7354545
R-squared	.22322	.22367	.22405
Mean DV	917.178	917.178	917.178
Panel B. Females			
Black × Exposure	-2.11914***	-2.2076***	-2.08741***
	(.32256)	(.32404)	(.32768)
Exposure	-.51158	-.96976***	-.32016
	(.41629)	(.23183)	(.23916)
Black	-9.75393***	-9.77148***	-9.78983***
	(.20003)	(.20294)	(.2018)
Observations	7883577	7883577	7883577
R-squared	.32936	.32965	.32995
Mean DV	957.980	957.980	957.980
Birth-County FE	✓	✓	✓
Birth-Year FE	✓	✓	✓
Birth-State-by-Birth-Year FE		✓	✓
Birth-County Trend			✓

Notes. Standard errors, clustered on county, are in parentheses.
**** p<0.01, ** p<0.05, * p<0.1*

Appendix D

One concern in the main results of the paper is the confounding influence of data linking. If the sample of linked individuals is different from the sample of individuals that weren't matched with their 1940 records in a way that correlates with their early life exposure to the movie screening, then the results might pick up on these differences rather than the effects of exposure to the movie. We empirically test this by merging the final sample observations with the 1940 census cohorts of 1910-1920 and examining whether the exposure measures can explain the successful linking, conditional on fixed effects and covariates. The results are reported in Appendix Table D-1. We observe that exposure among Blacks is associated with a reduction in the probability of being in the linked sample. The respective change implied by the coefficient is about 8.6% with respect to the mean of the outcome. Although this is a small change, the coefficient is statistically significant. One explanation for the observed reduction in the likelihood of merging is that black individuals in showing counties have significantly lower life expectancy and are less likely to live up to the year 1975, the year that the DMF data started.

Appendix Table D-1 - Exploring Endogenous Merging Concern

	Outcome: Successful DMF-Census Merging			
	(1)	(2)	(3)	(4)
Black × Exposure	-.01511***	-.01394***	-.0143***	-.01302**
	(.0046)	(.00515)	(.00513)	(.00517)
Exposure	.00905	.00913	.00892	.00871
	(.01331)	(.01525)	(.0161)	(.01612)
Black	-.03423***	-.03492***	-.03512***	-.03293***
	(.00155)	(.00164)	(.00165)	(.00168)
Observations	29253719	29253719	29253719	29253719
R-squared	.02829	.02981	.03023	.03135
Mean DV	0.154	0.154	0.154	0.154
Birth-County FE	✓	✓	✓	✓
Birth-Year FE	✓	✓	✓	✓
Birth-State-by-Birth-Year FE		✓	✓	✓
Birth-County Trend			✓	✓
Individual Control				✓

Notes. Standard errors, clustered on county, are in parentheses. Individual controls include dummies for ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies.
**** p<0.01, ** p<0.05, * p<0.1*

Appendix E

In the main analysis sample of the paper, we used several methods to infer the county of birth. In this appendix, we simply use the county of residence in 1940 as a proxy for the county of birth. We replicate the main results and report them in appendix table E. The interaction term drops by about 26%. This drop in the magnitude suggests that the measurement error in assigning the county of birth underestimates the true effects and highlights the relevance of our procedure for the county of birth inference.

Appendix Table E-1 - Replicating the Main Results Using 1940 County as A Proxy for County of Birth

	Outcome: Age-at-Death (Months)			
	(1)	(2)	(3)	(4)
Black × Exposure	-4.89744***	-4.56632***	-4.54123***	-4.54123***
	(.52681)	(.5583)	(.56011)	(.56011)
Exposure	.6423***	.22477	.28371	.28371
	(.22076)	(.21617)	(.2277)	(.2277)
Black	-7.91105***	-7.99812***	-7.97388***	-7.97388***
	(.30163)	(.30203)	(.30397)	(.30397)
Observations	5569763	5569763	5569763	5569763
R-squared	.33902	.33931	.33971	.33971
Mean DV	875.616	875.616	875.616	875.616
Birth-County FE	✓	✓	✓	✓
Birth-Year FE	✓	✓	✓	✓
Birth-State-by-Birth-Year FE		✓	✓	✓
Birth-County Trend			✓	✓
Individual/Family Control				✓

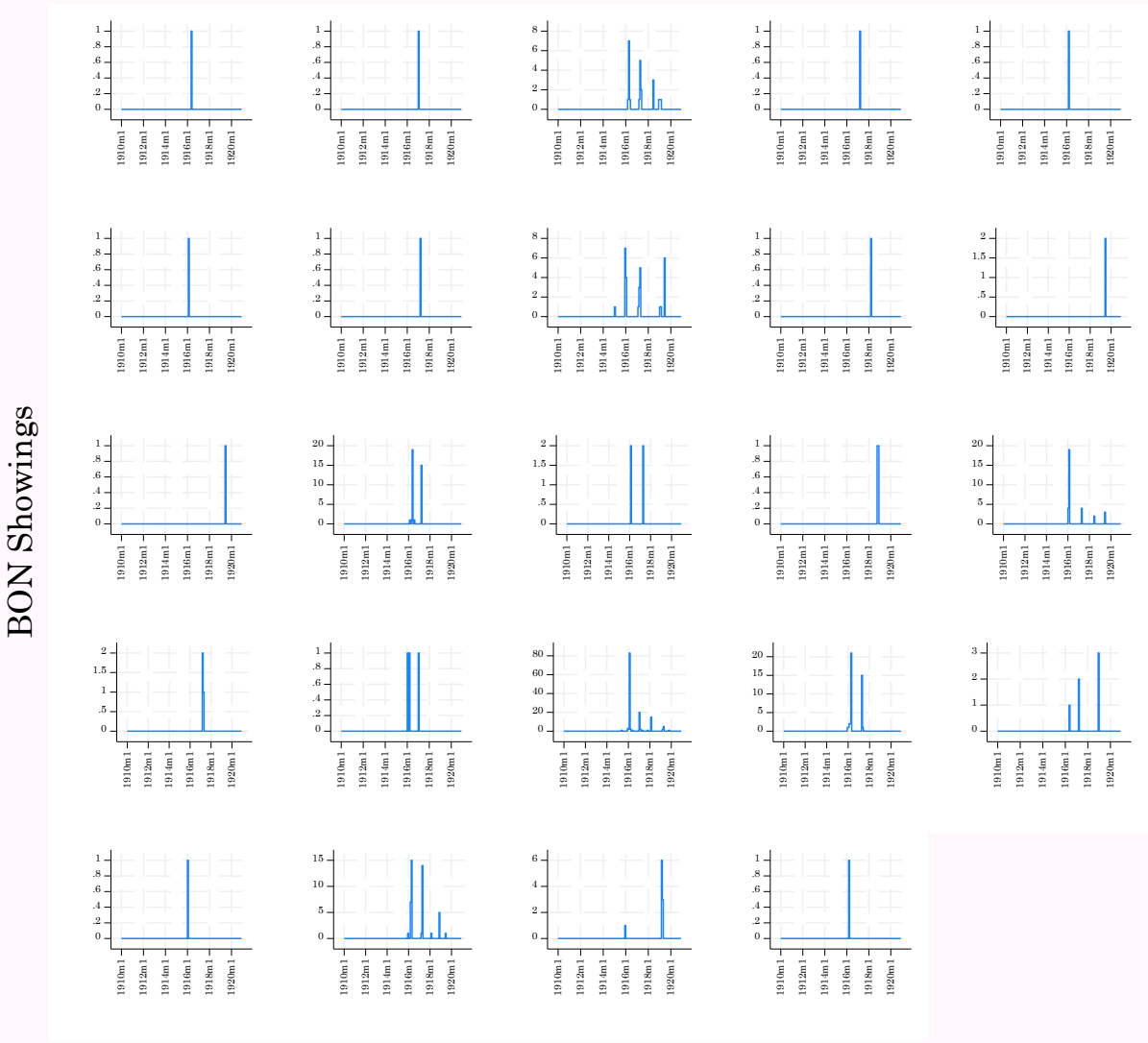
Notes. Standard errors, clustered on county, are in parentheses. All regressions include county fixed effects, birth-year fixed effects, birth-state by birth-year fixed effects, birth-county trend, individual controls, and family controls. Individual controls include dummies for ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies.

**** p<0.01, ** p<0.05, * p<0.1*

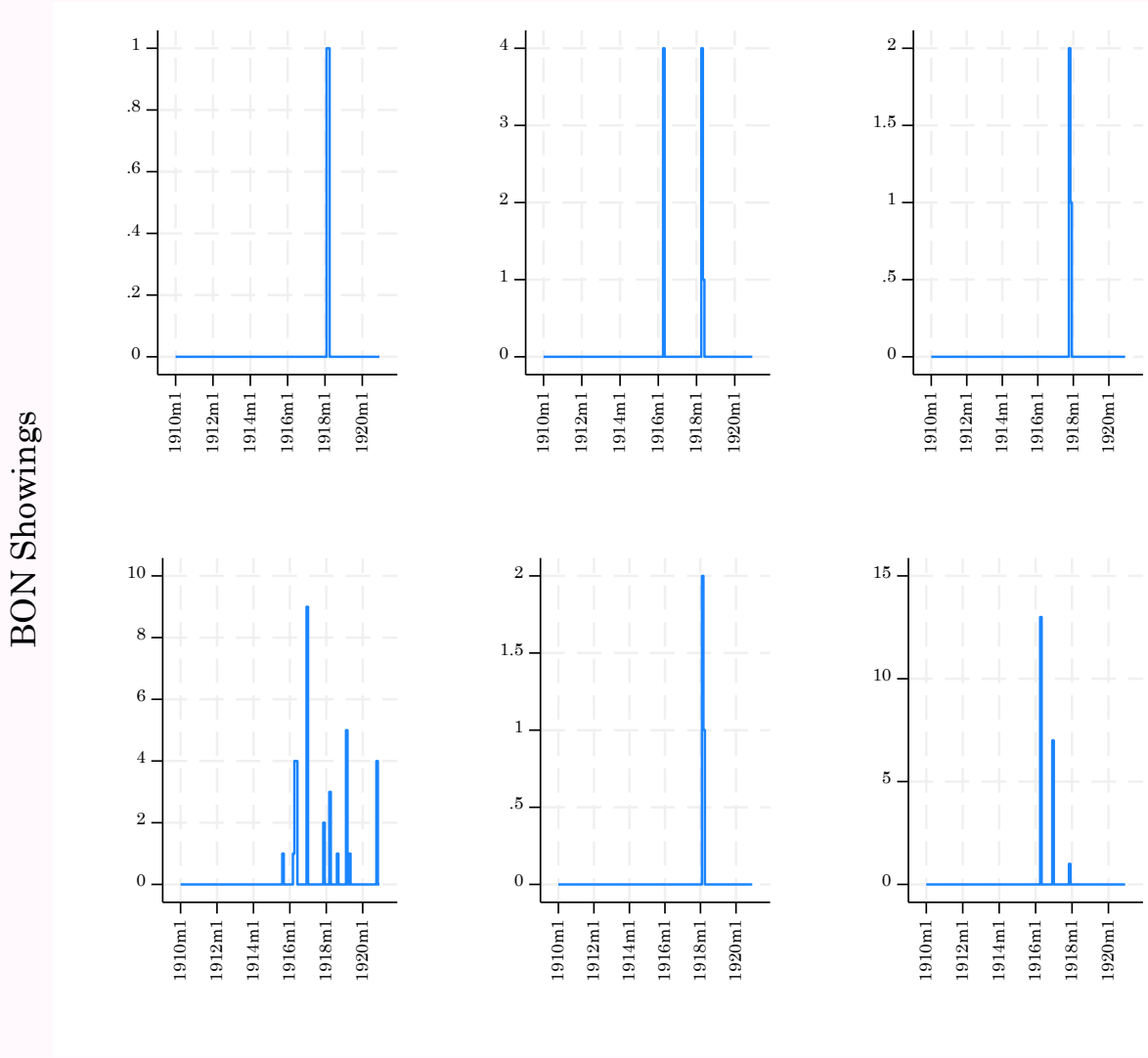
Appendix F

The following figures depict the time series of county level movie showing across the road-show counties.

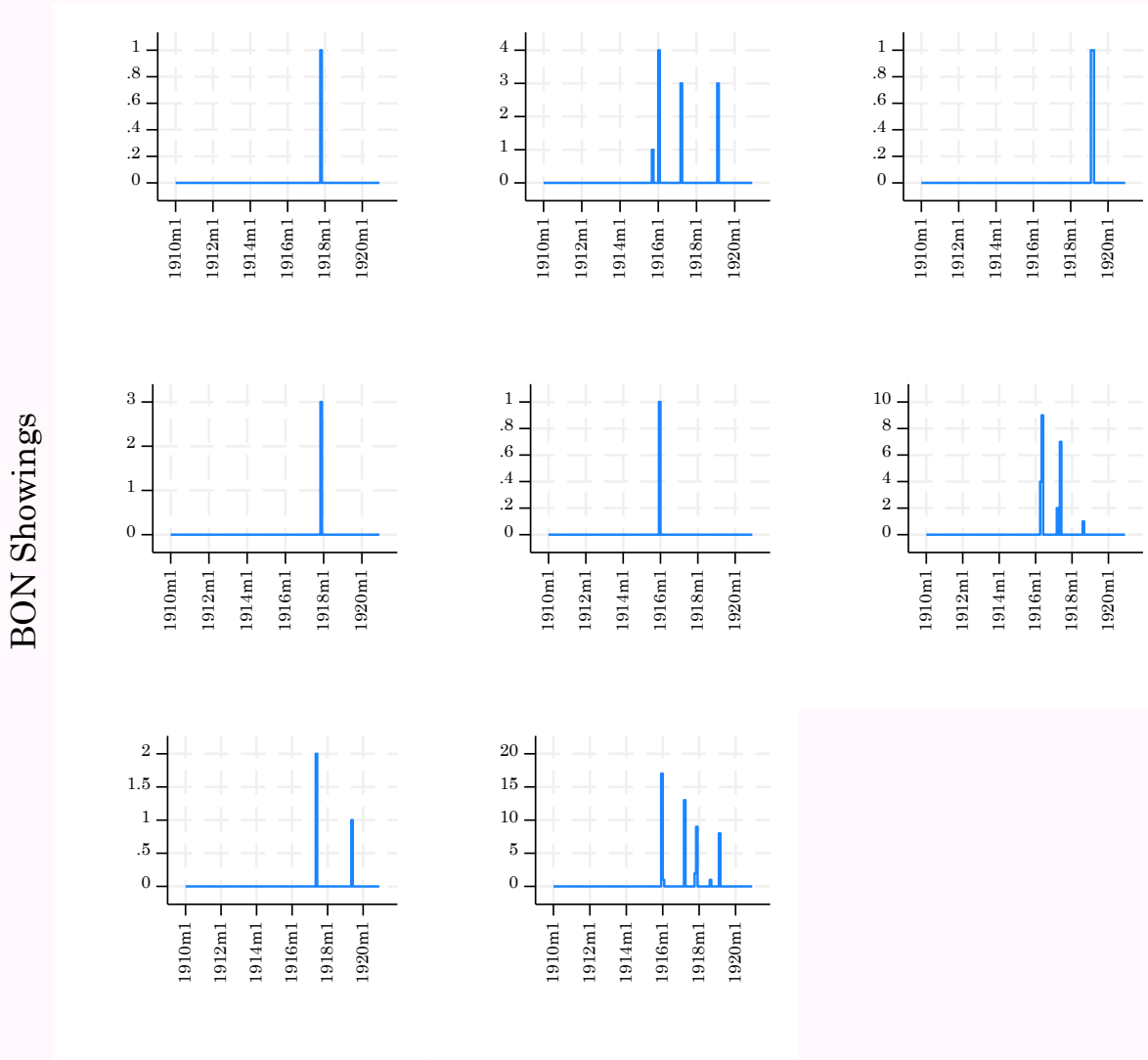
Alabama



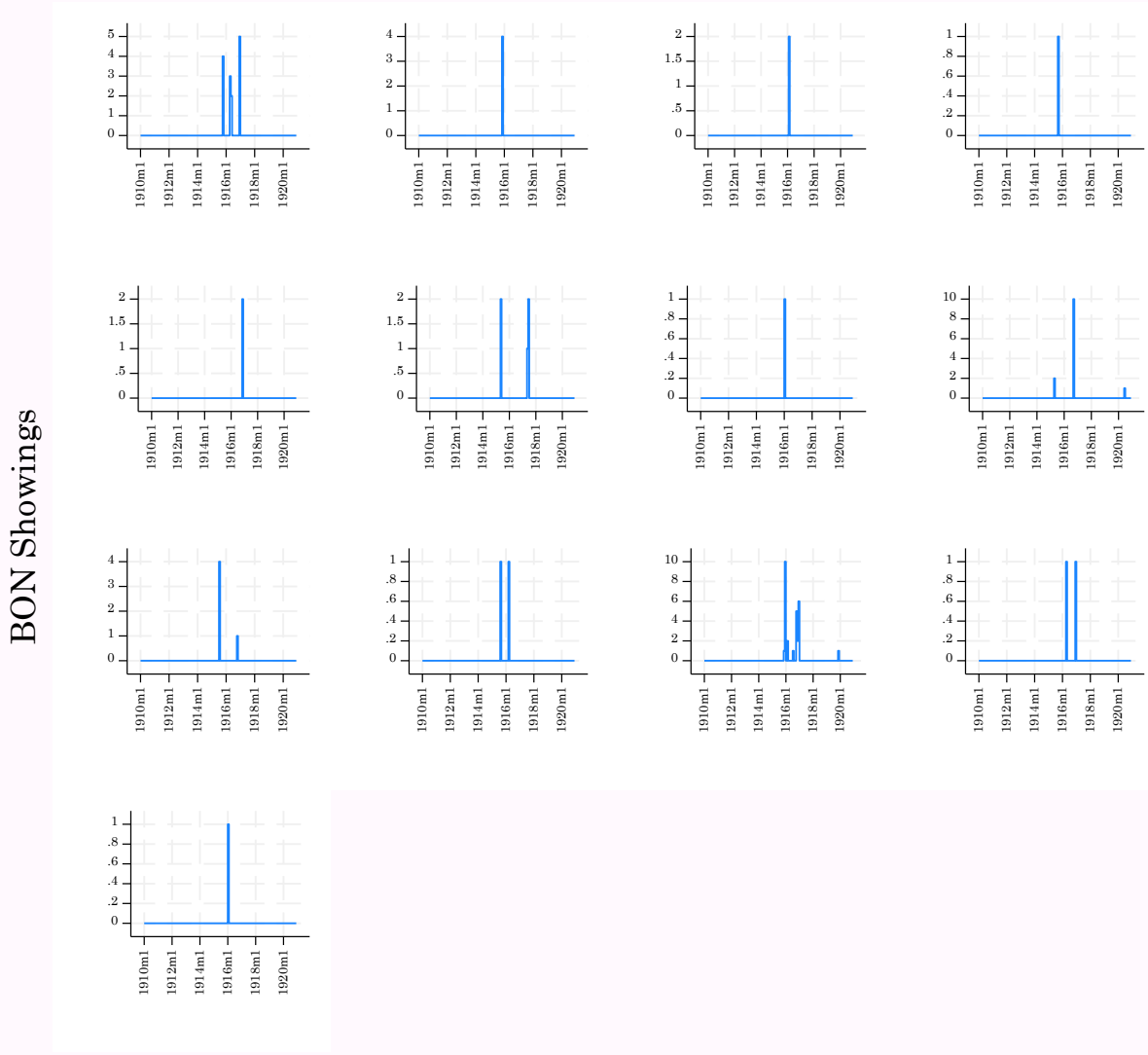
Arizona

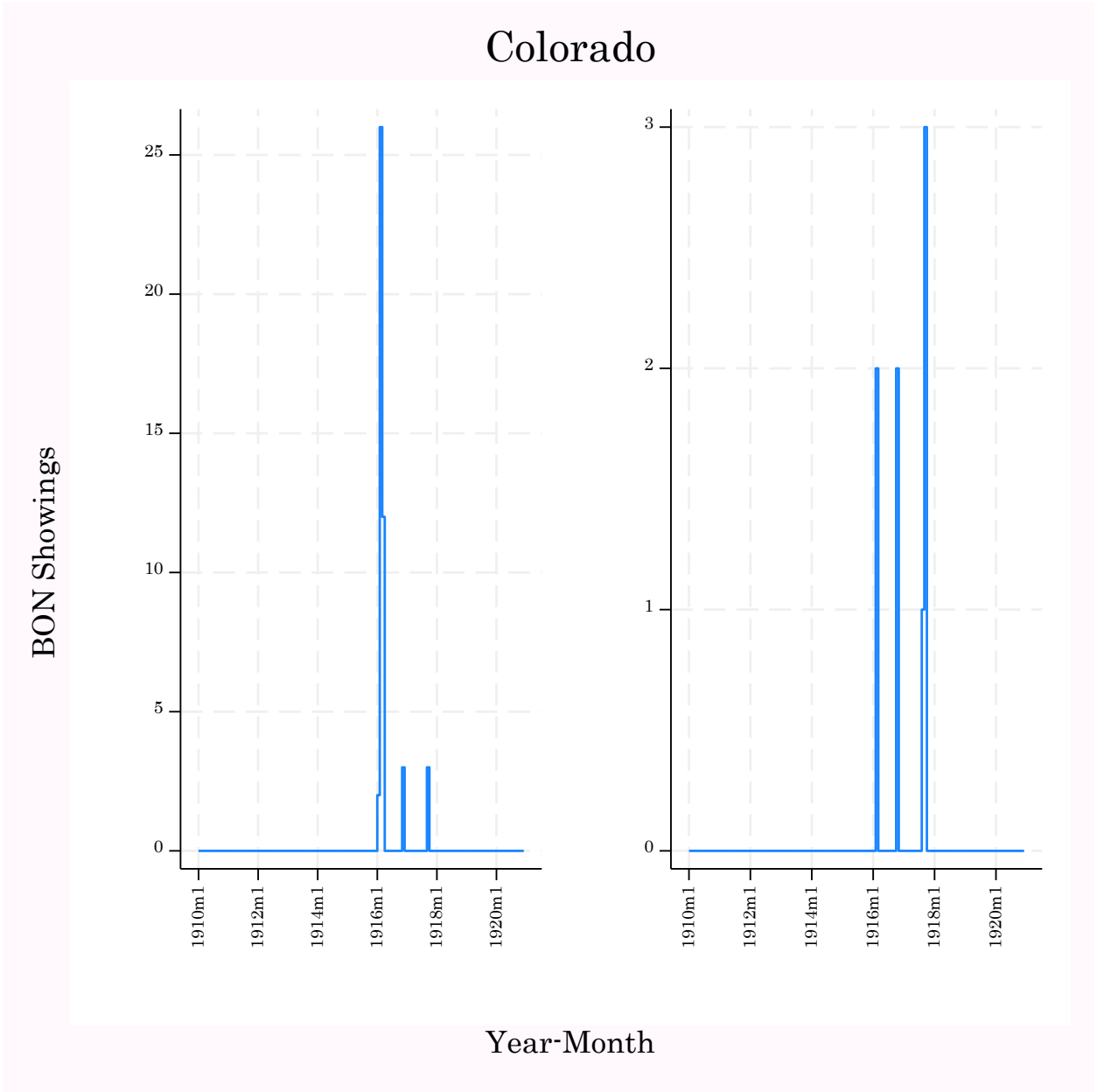

BON Showings
Year-Month
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1

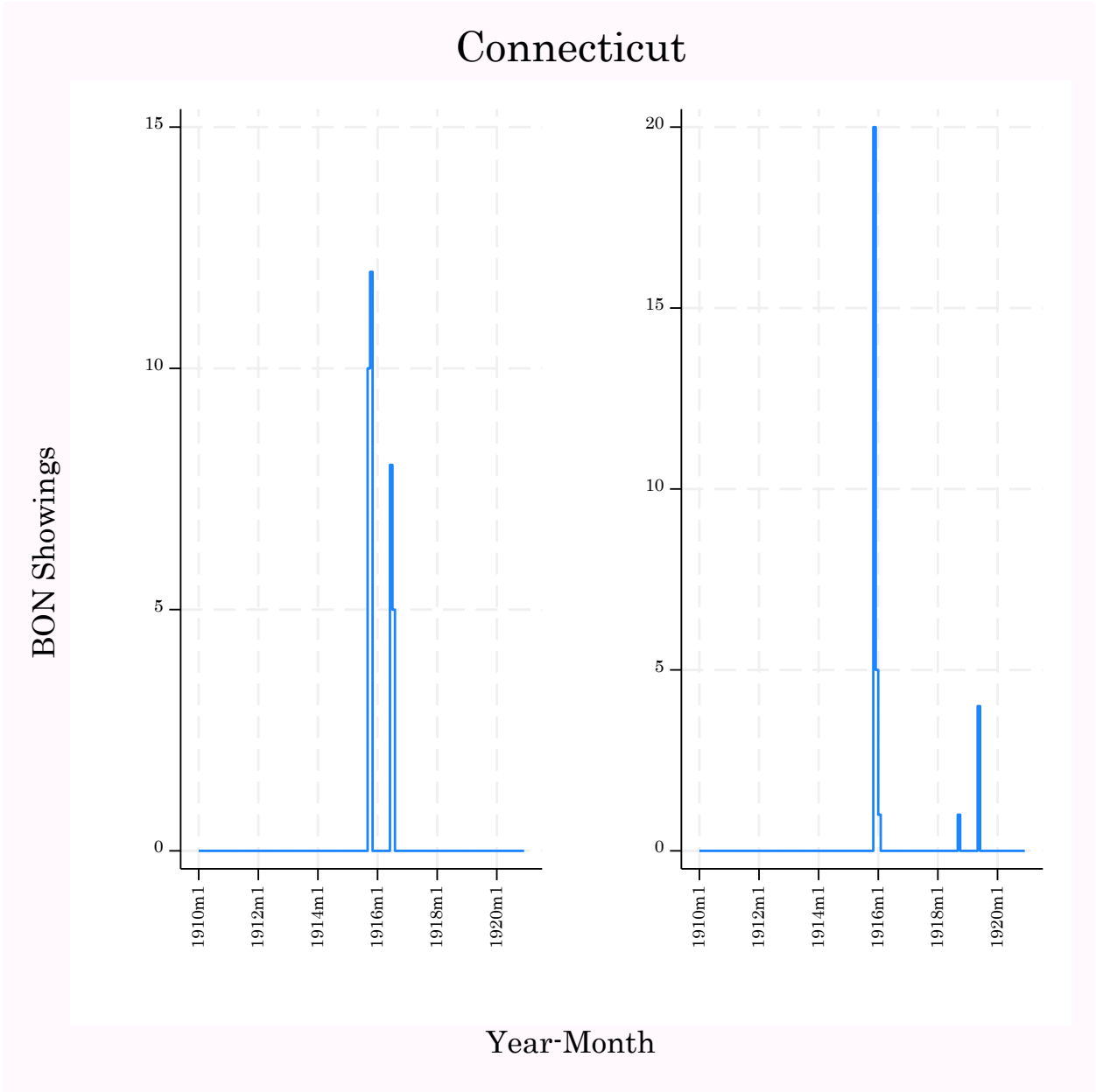
Arkansas

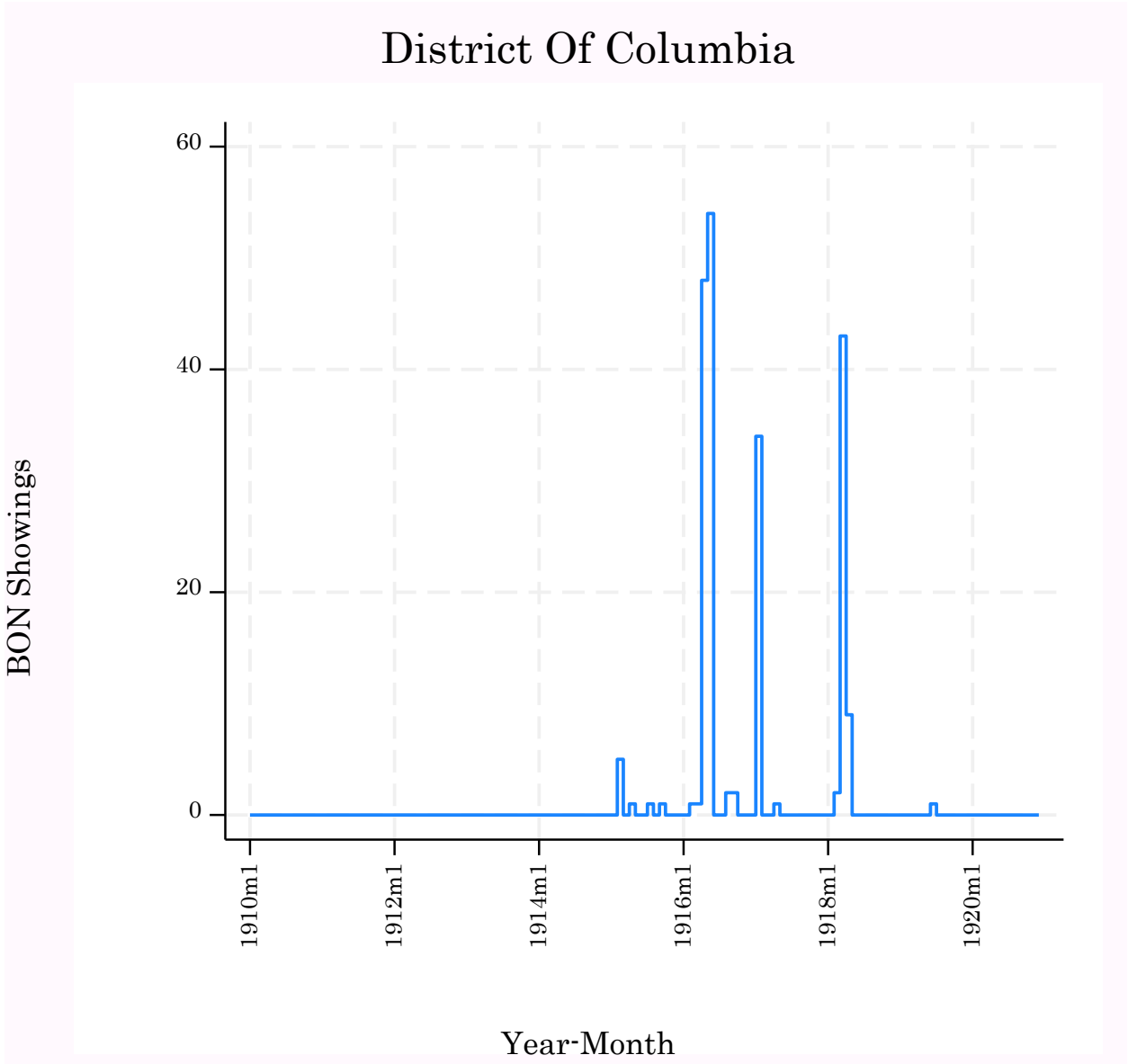

BON Showings
Year-Month
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1

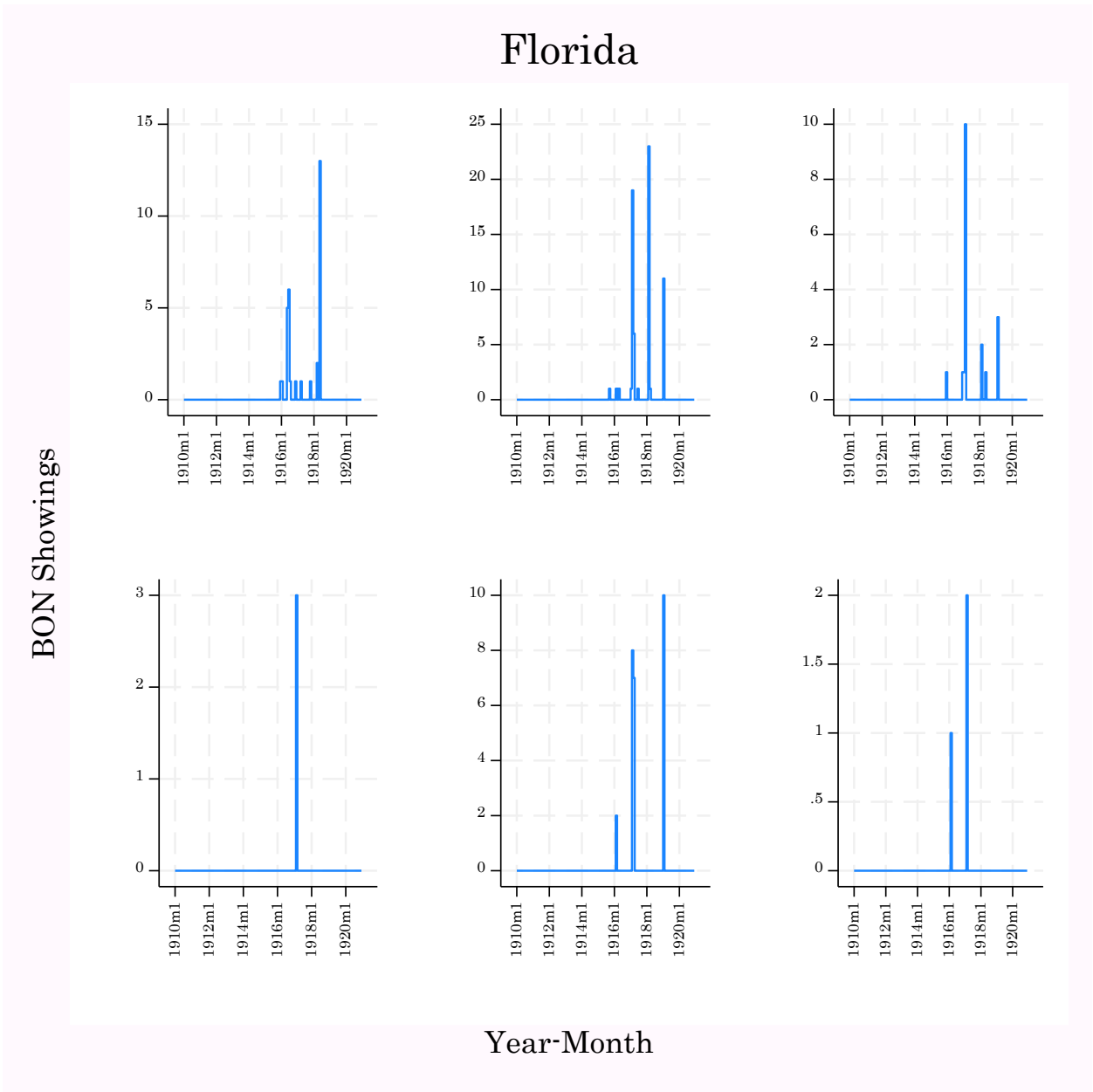
California

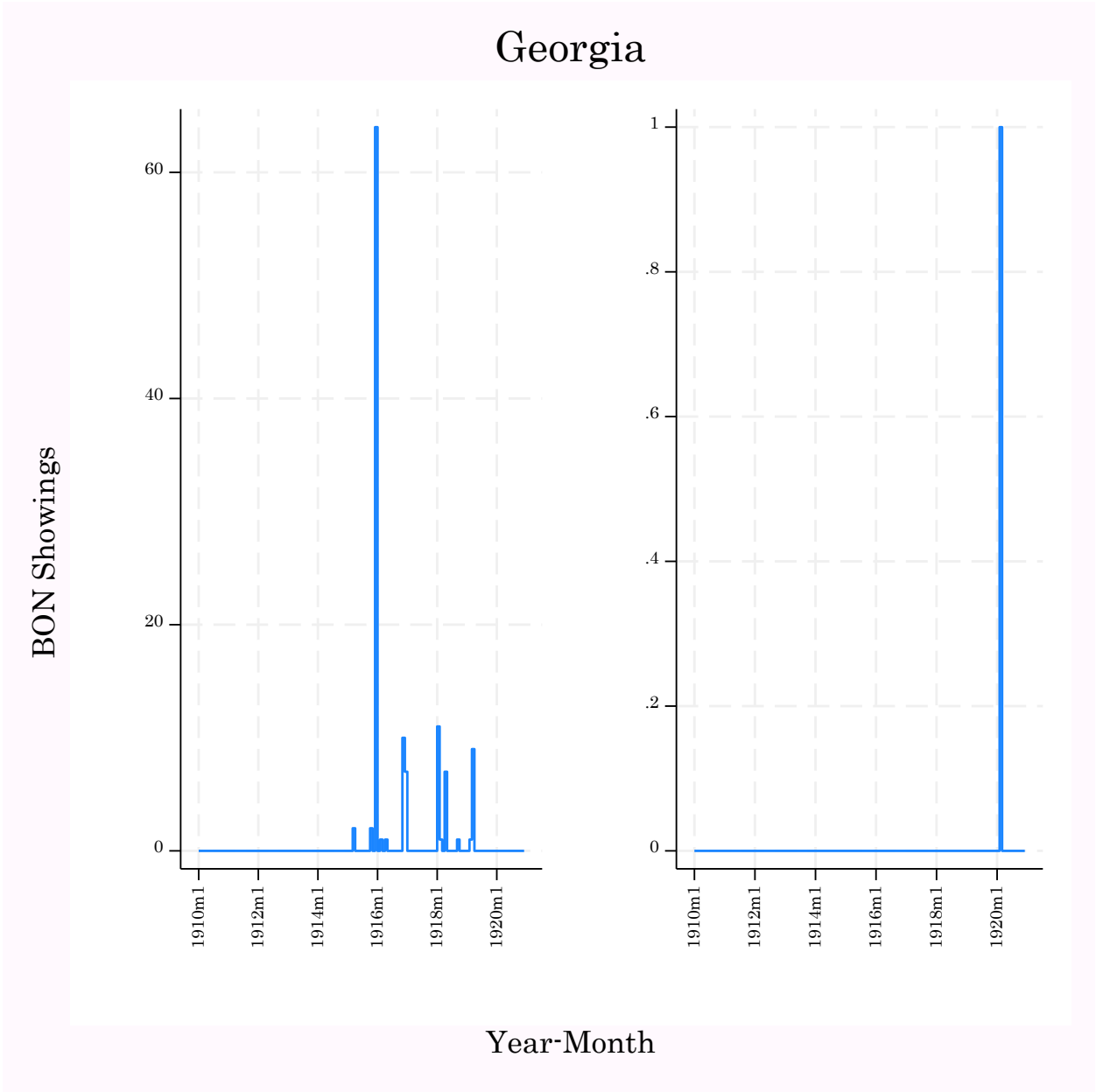


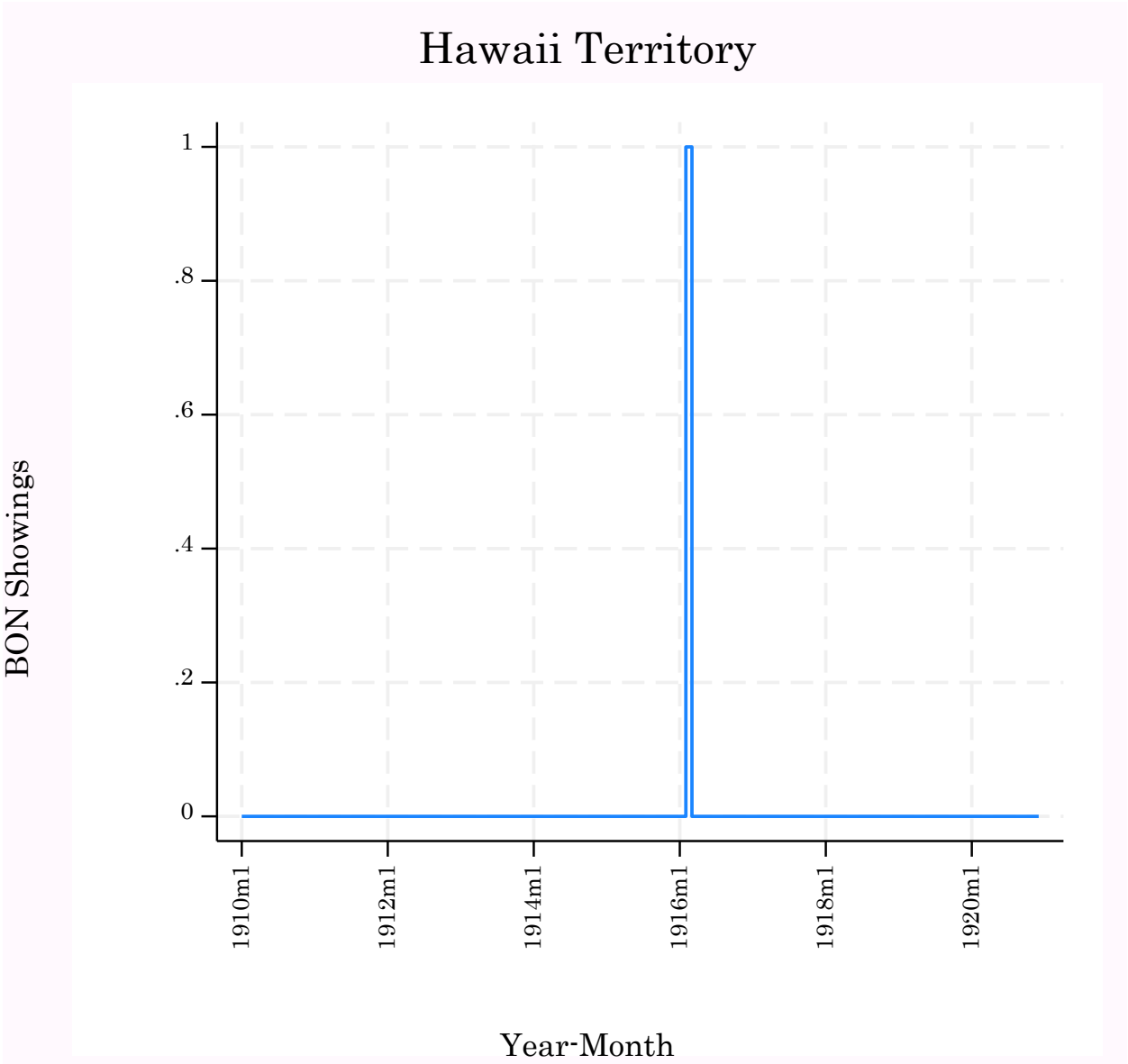

Colorado
BON Showings
25
20
15
10
5
0
3
2
1
0
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1
Year-Month


Connecticut
BON Showings
15
10
5
0
20
15
10
5
0
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1
Year-Month

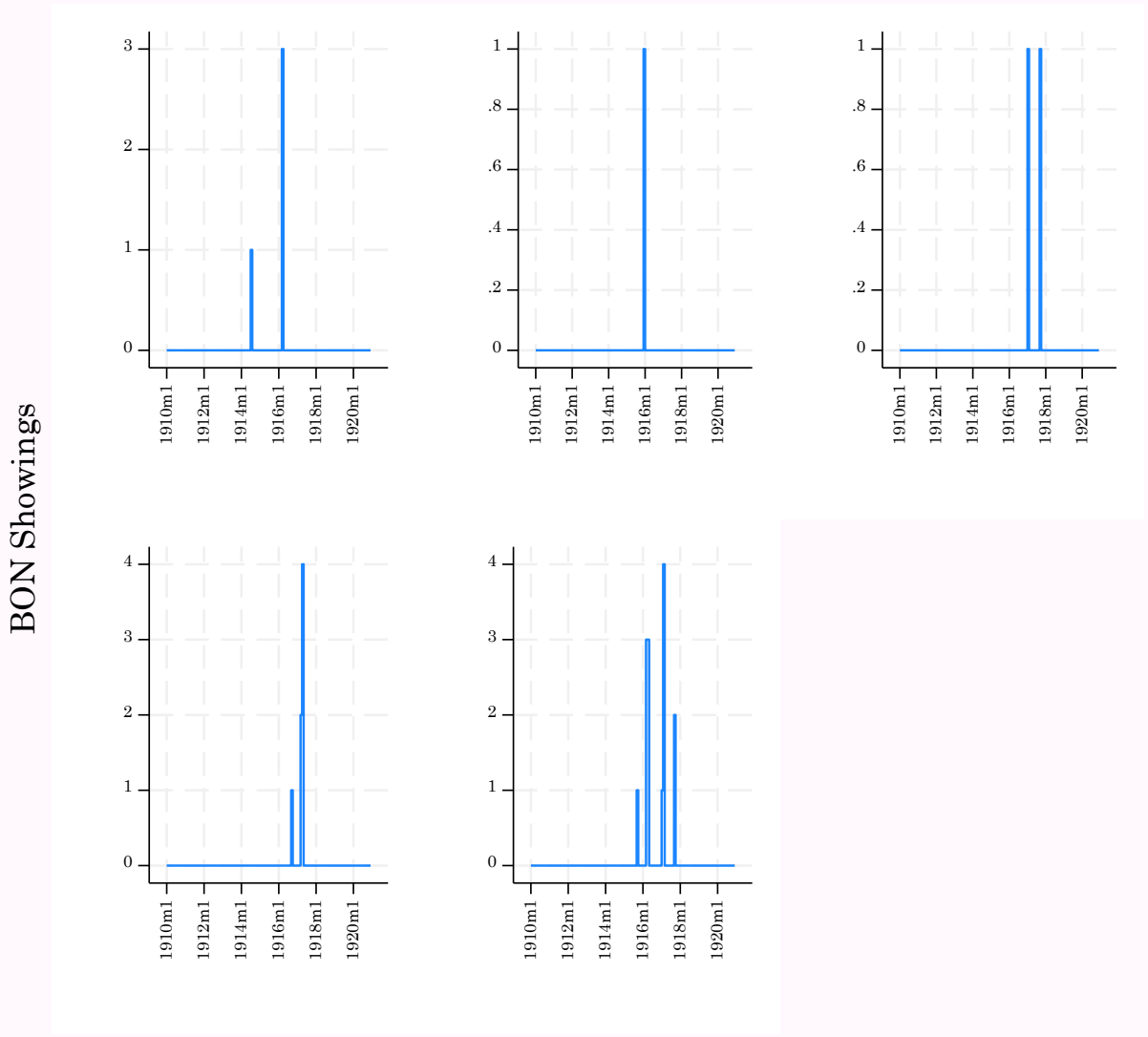

District Of Columbia
BON Showings
60
40
20
0
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1
Year-Month


Florida
BON Showings
Year-Month
15
10
5
0
25
20
15
10
5
0
10
8
6
4
2
0
3
2
1
0
10
8
6
4
2
0
2
1.5
1
.5
0
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1

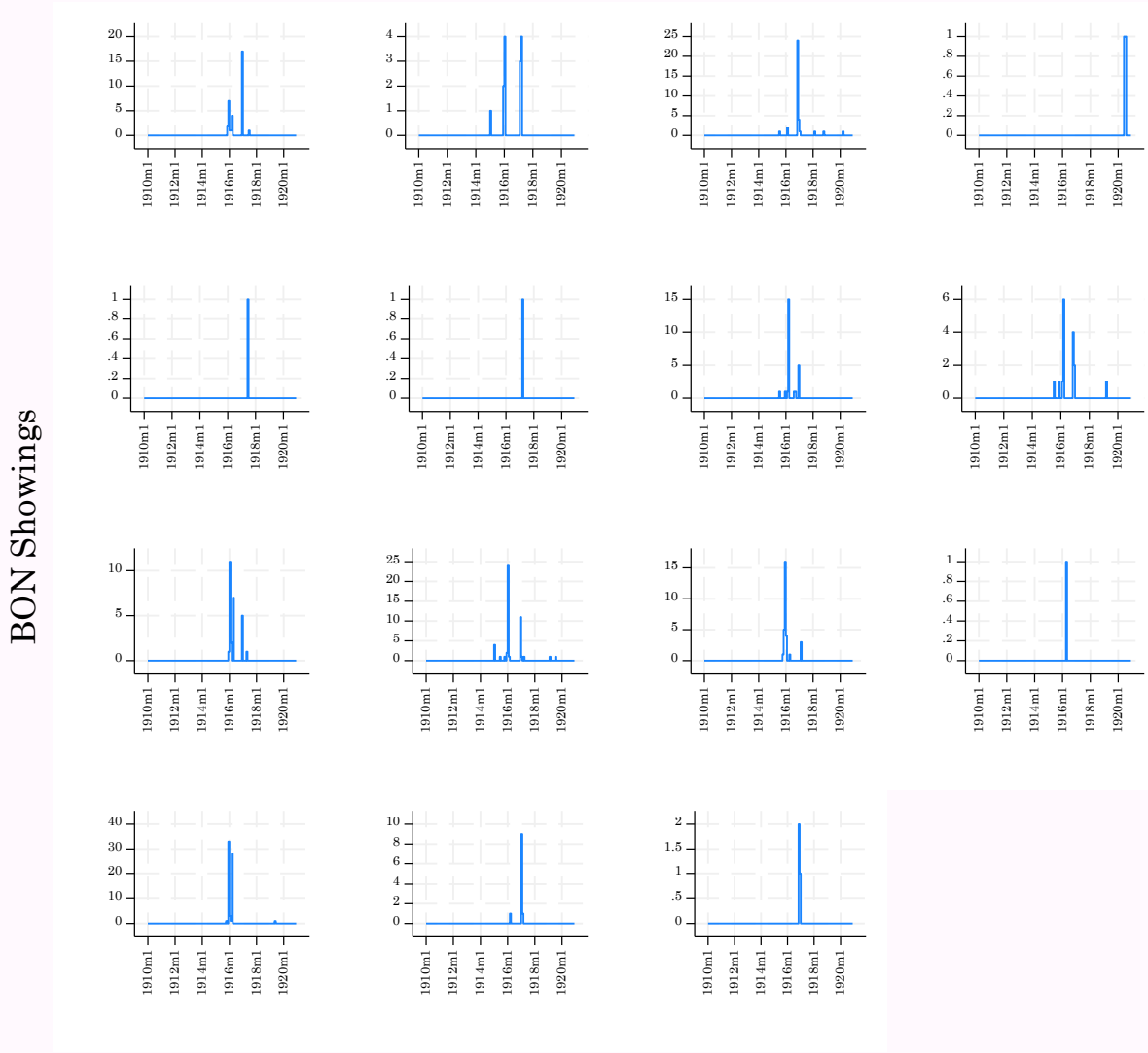

Georgia
BON Showings
60
40
20
0
1
.8
.6
.4
.2
0
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1
Year-Month


Hawaii Territory
BON Showings
1
.8
.6
.4
.2
0
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1
Year-Month

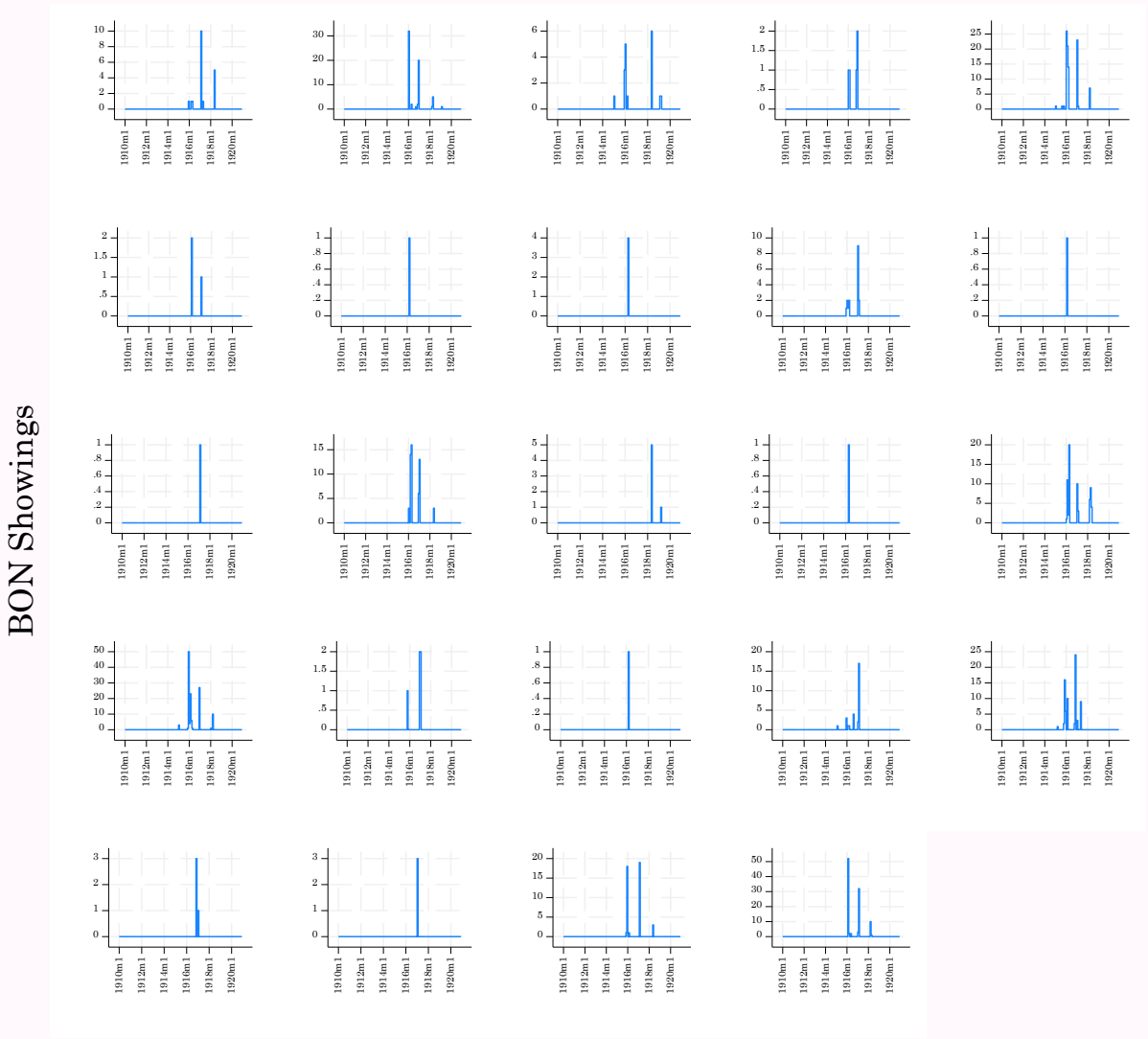
Idaho


BON Showings
Year-Month
0
1
2
3
.2
.4
.6
.8
4
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1

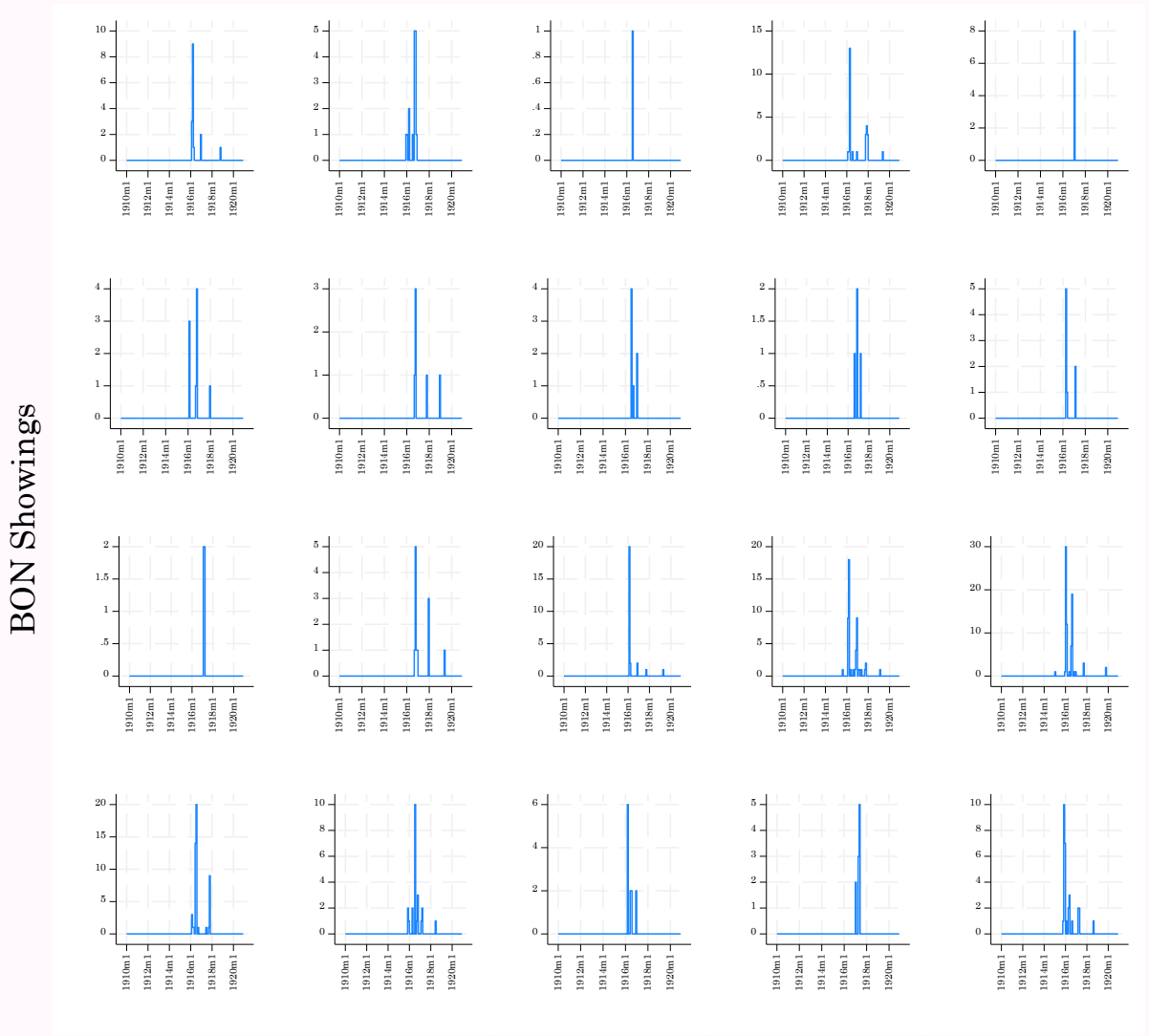
Illinois


BON Showings
Year-Month
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1

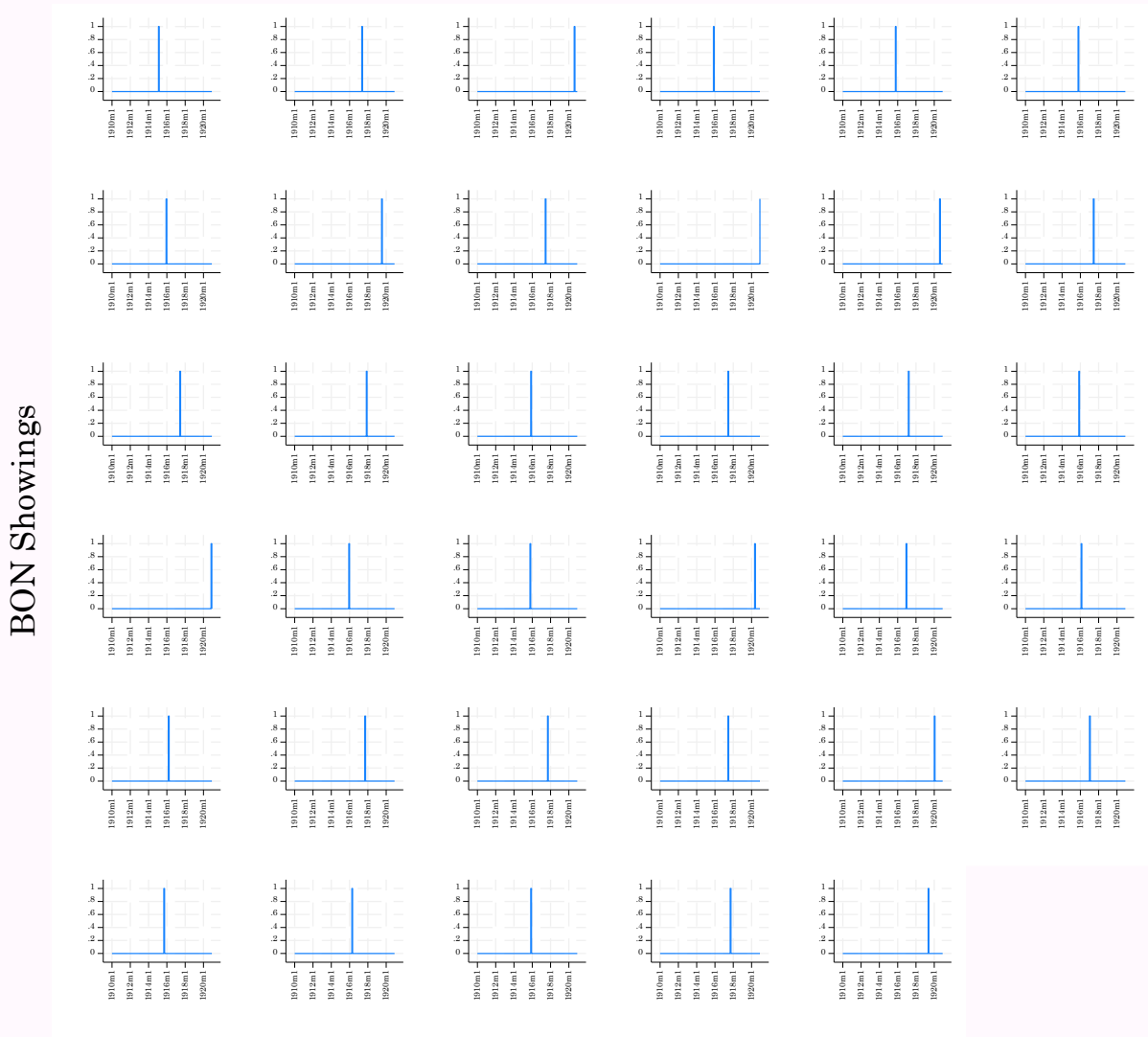
Indiana



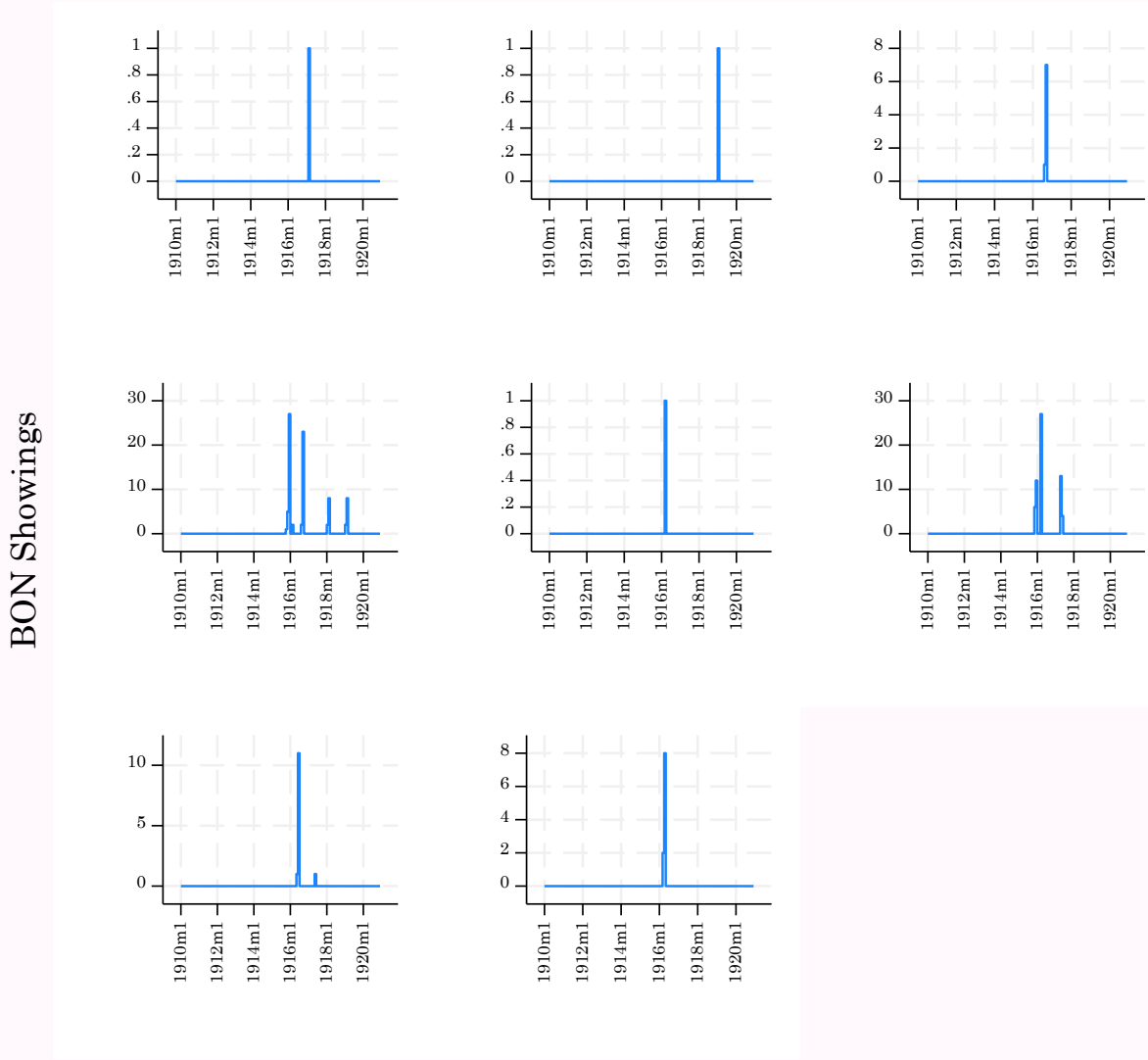
Iowa



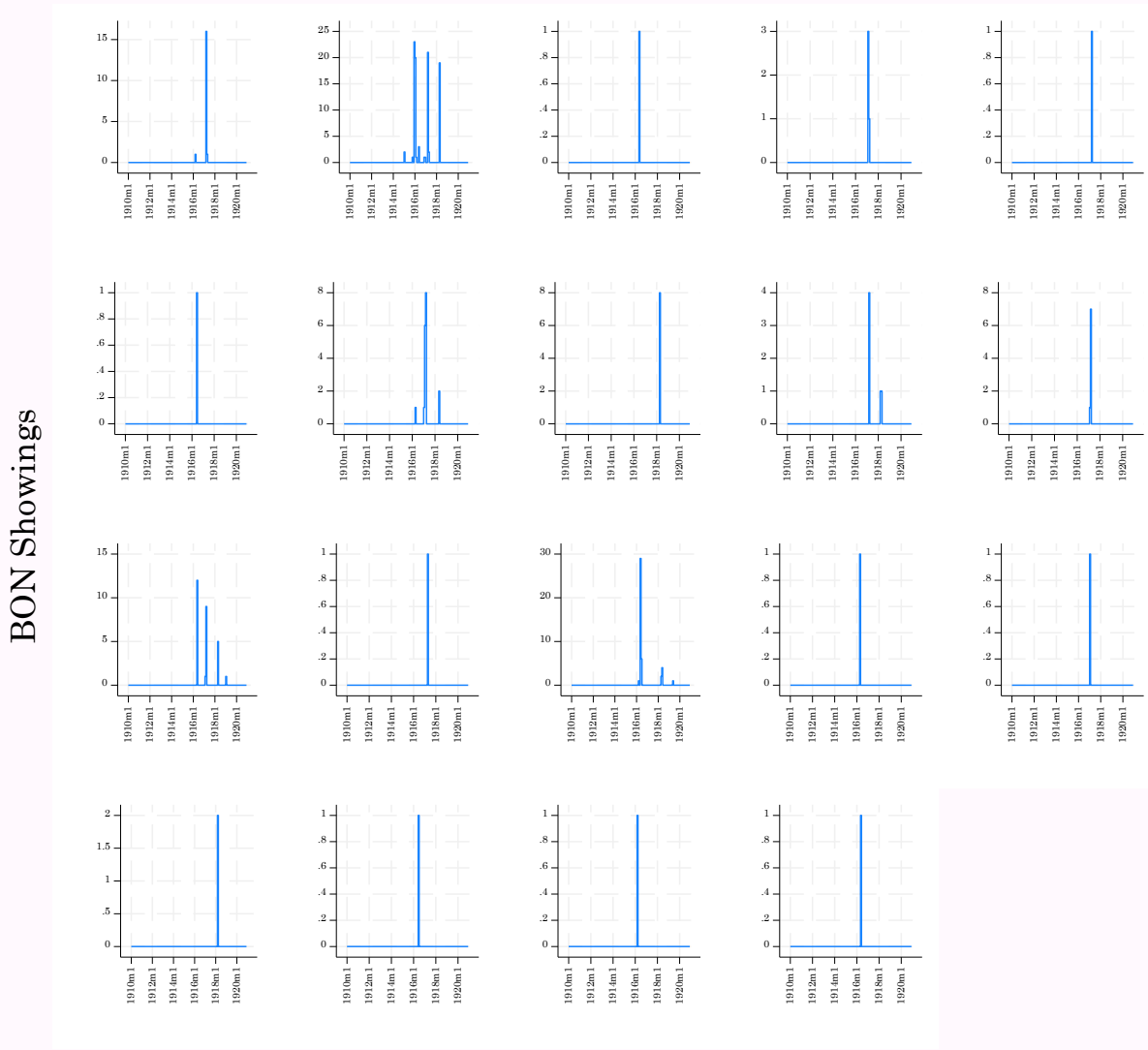
Kansas

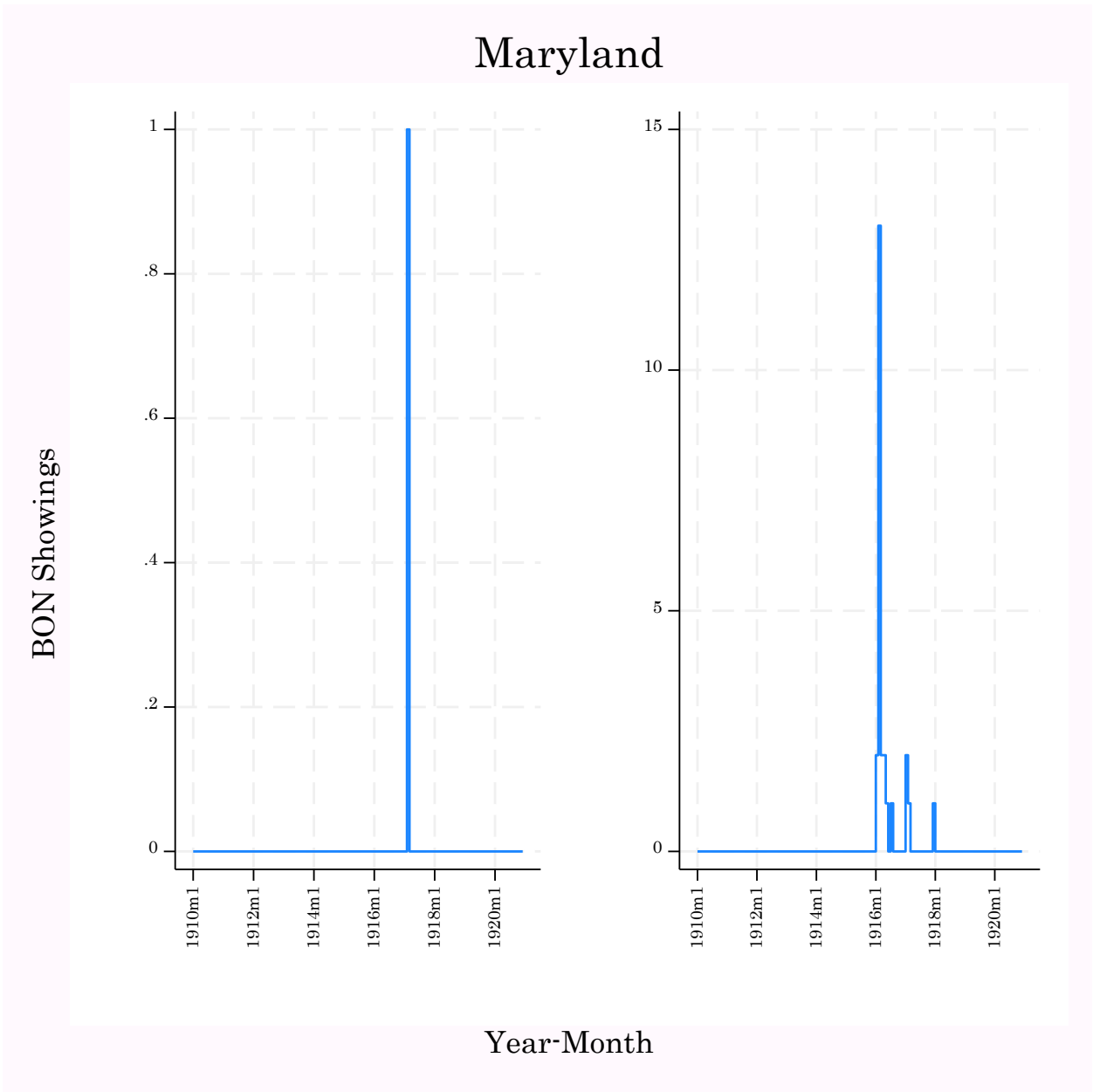


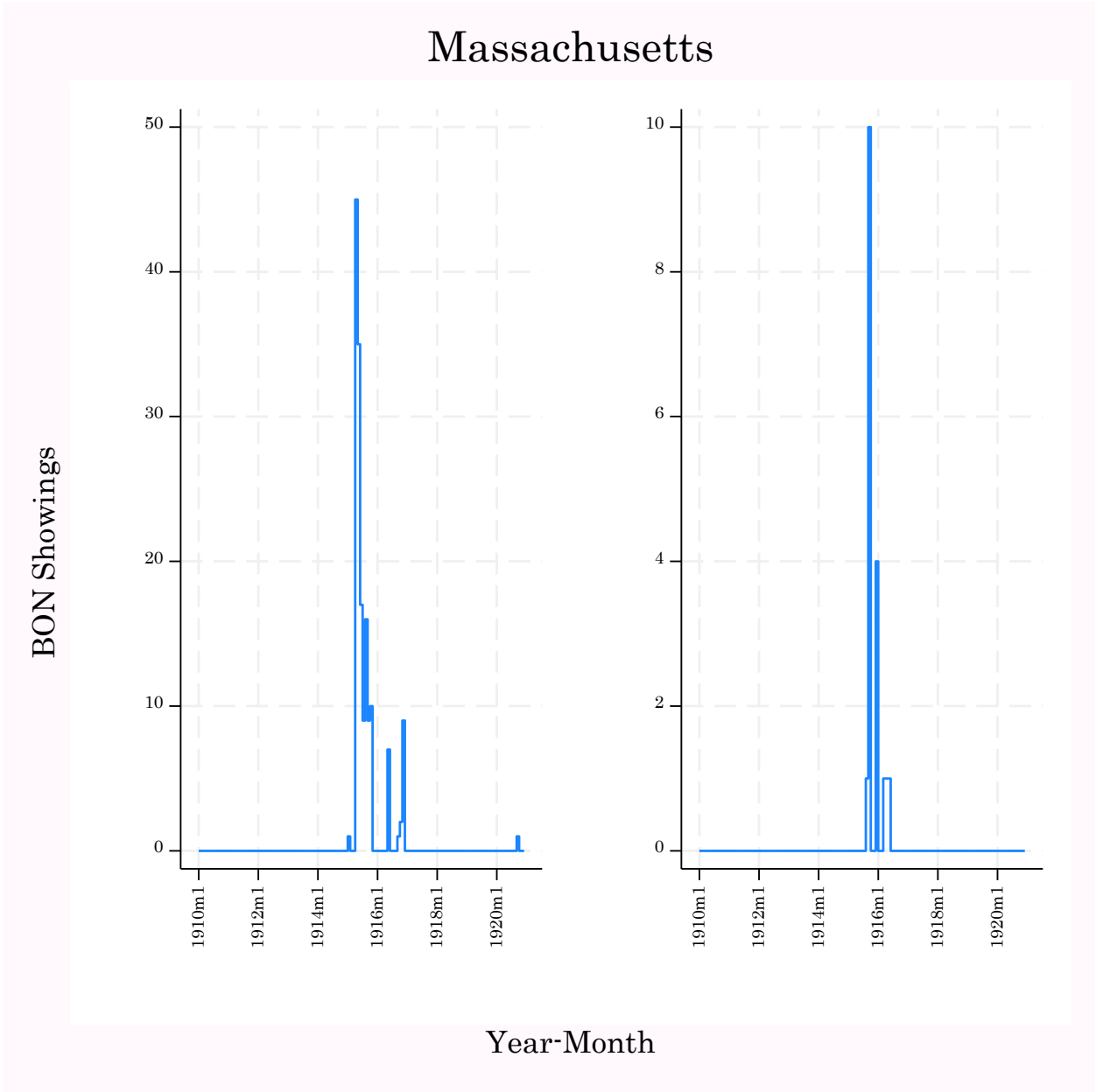
Kentucky

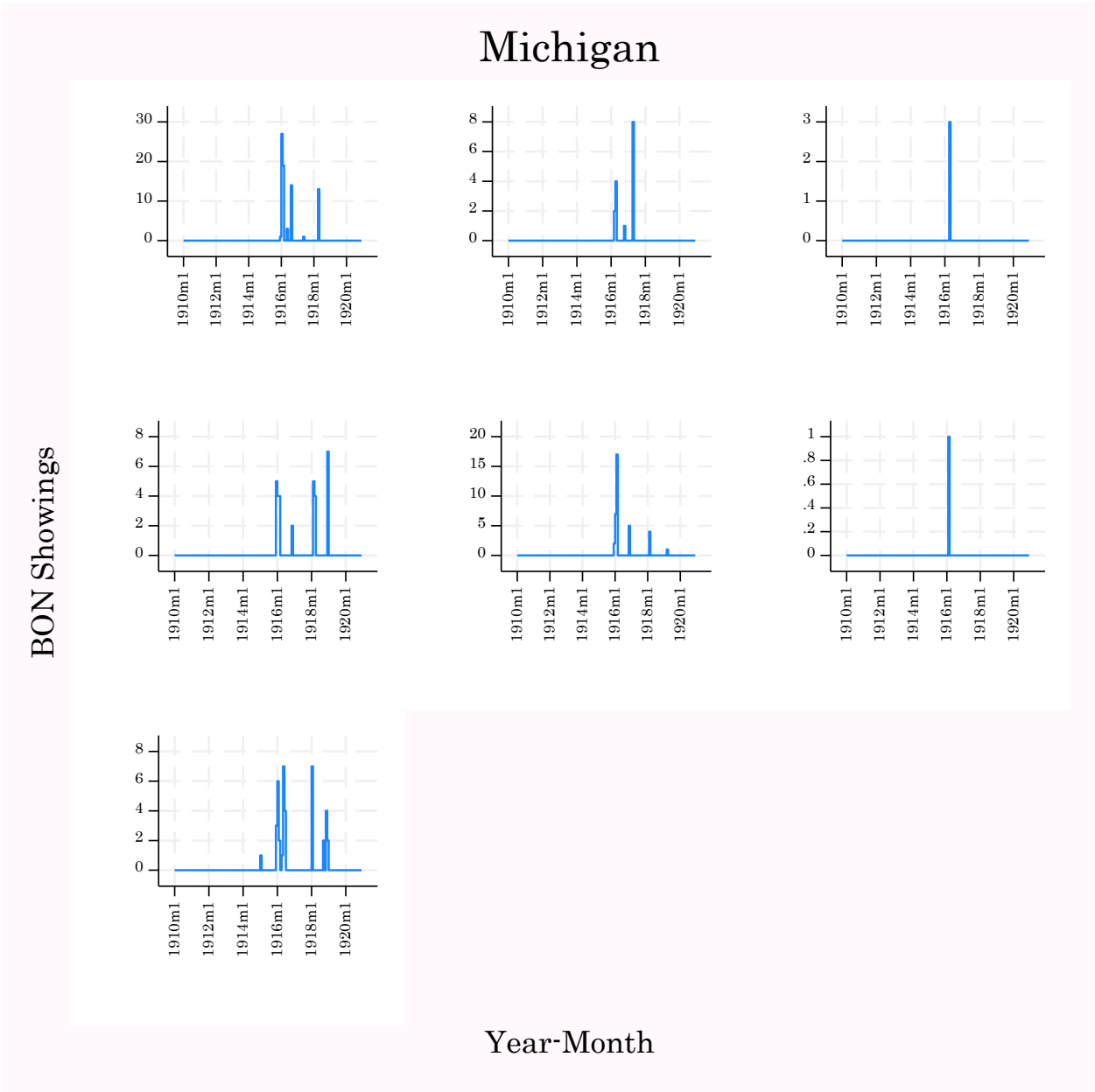

BON Showings
Year-Month
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1

Louisiana

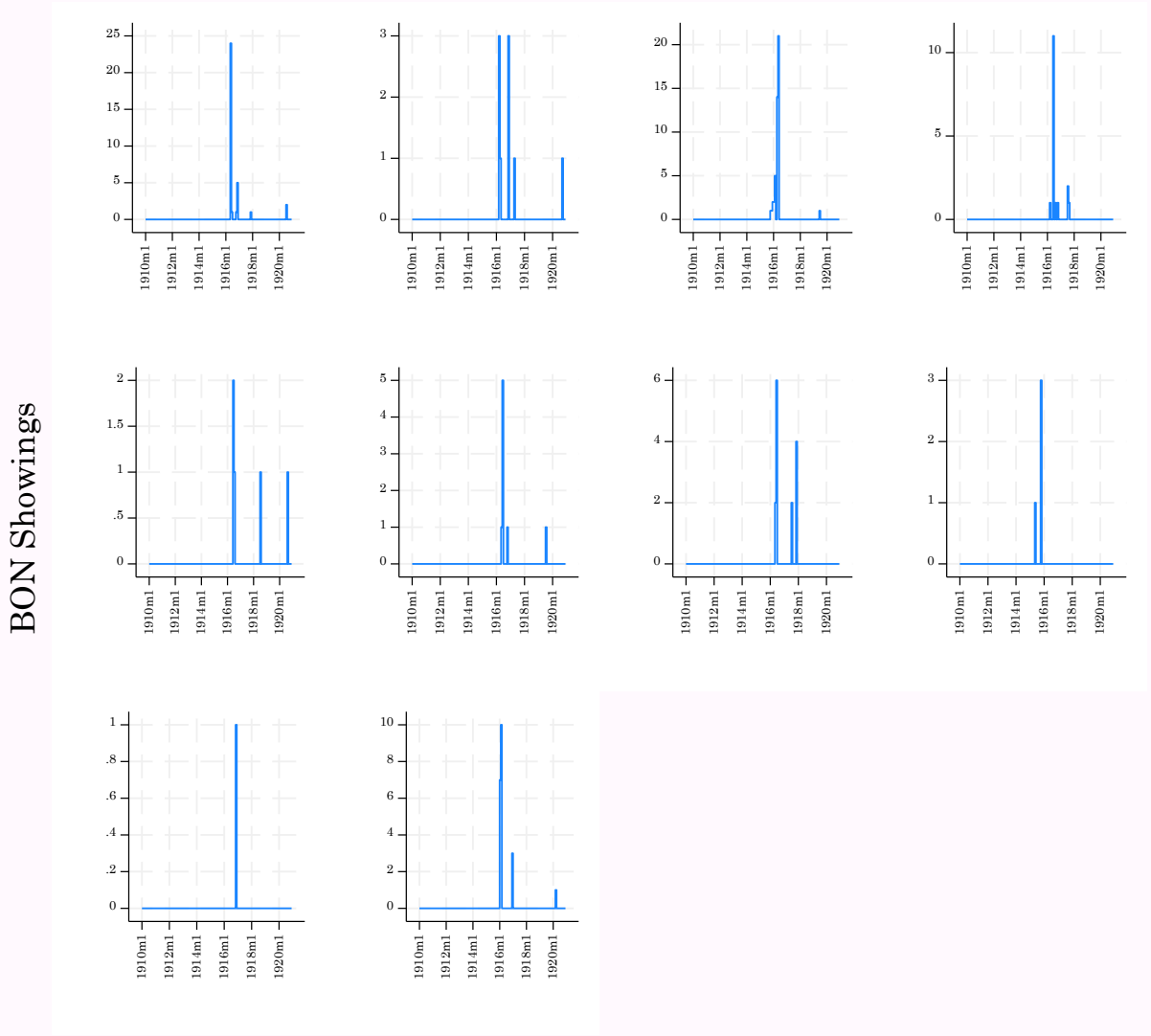



Maryland
BON Showings
1
.8
.6
.4
.2
0
15
10
5
0
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1
Year-Month

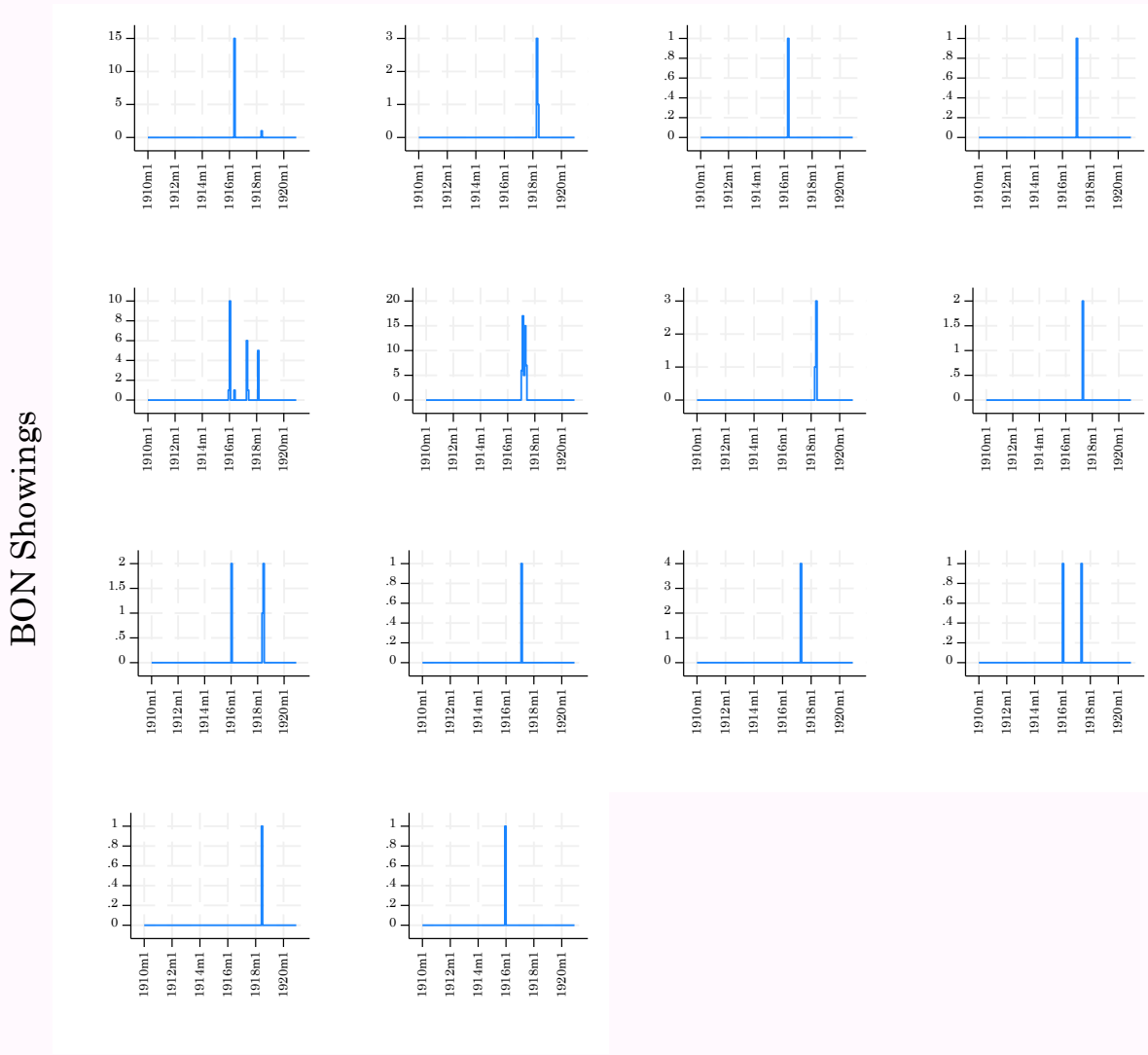

Massachusetts
BON Showings
50
40
30
20
10
0
10
8
6
4
2
0
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1
Year-Month


Michigan
BON Showings
Year-Month
30
20
10
0
8
6
4
2
0
3
2
1
0
8
6
4
2
0
20
15
10
5
0
1
.8
.6
.4
.2
0
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1

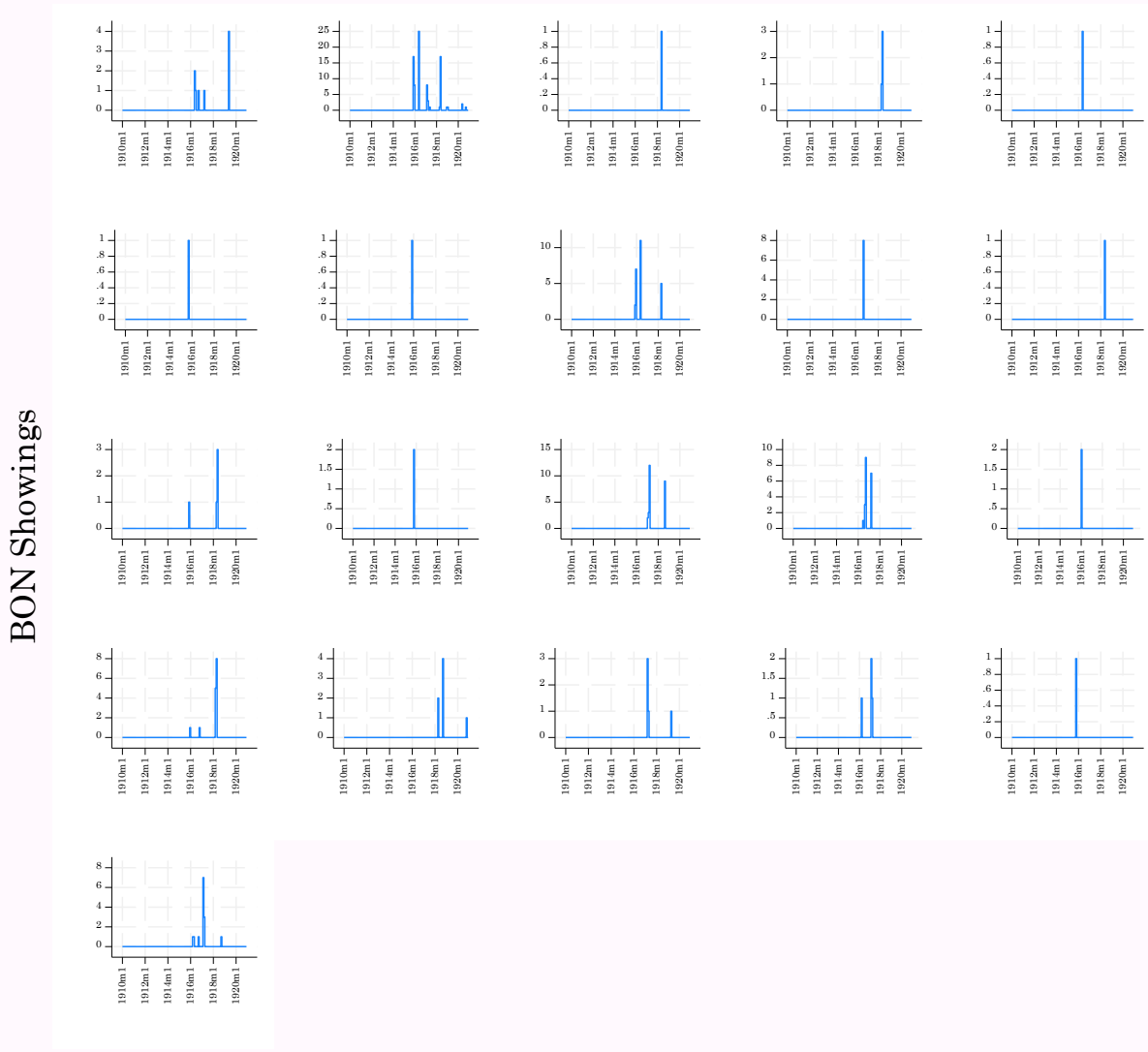
Minnesota


BON Showings
Year-Month
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1

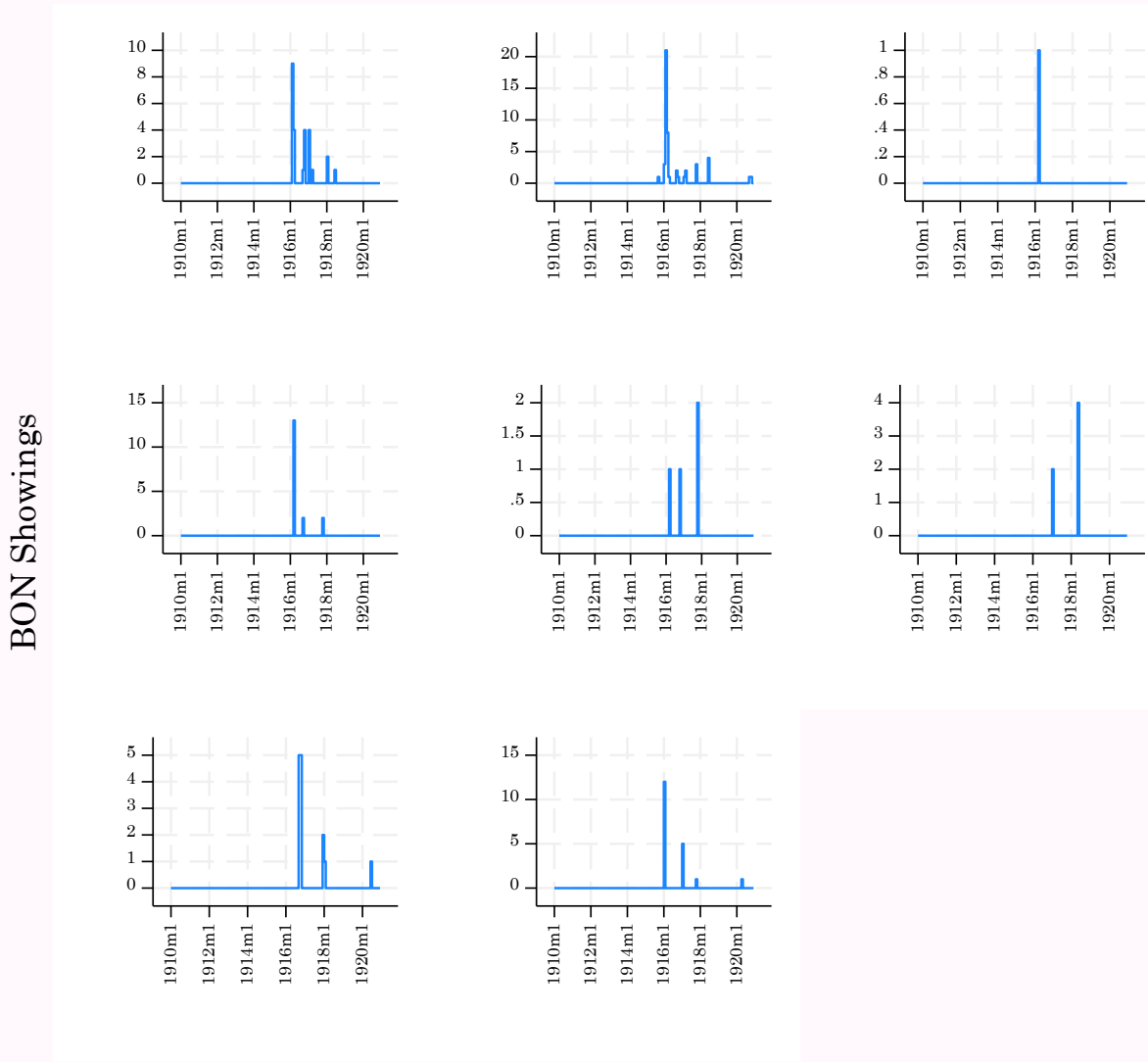
Mississippi



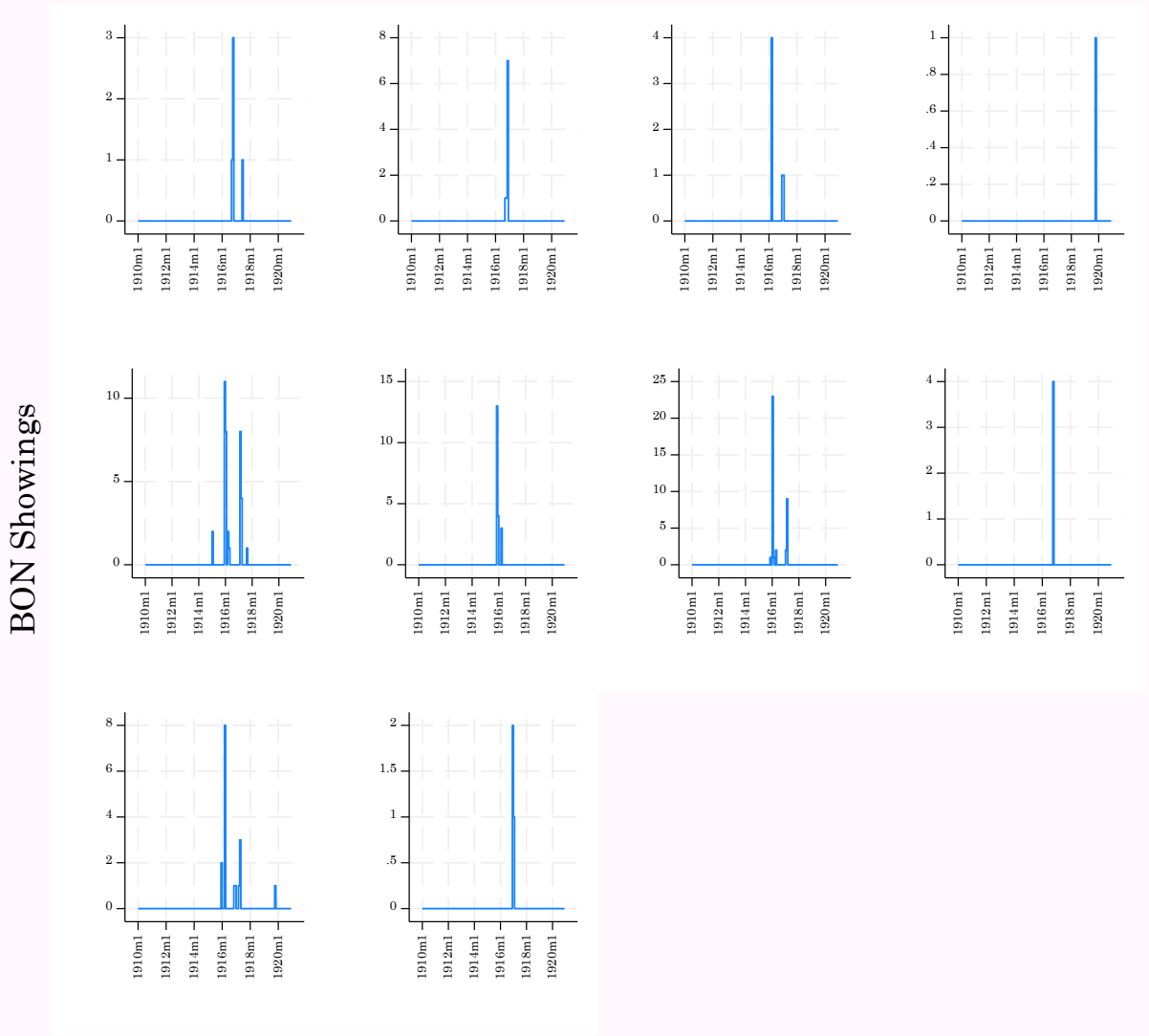
Missouri



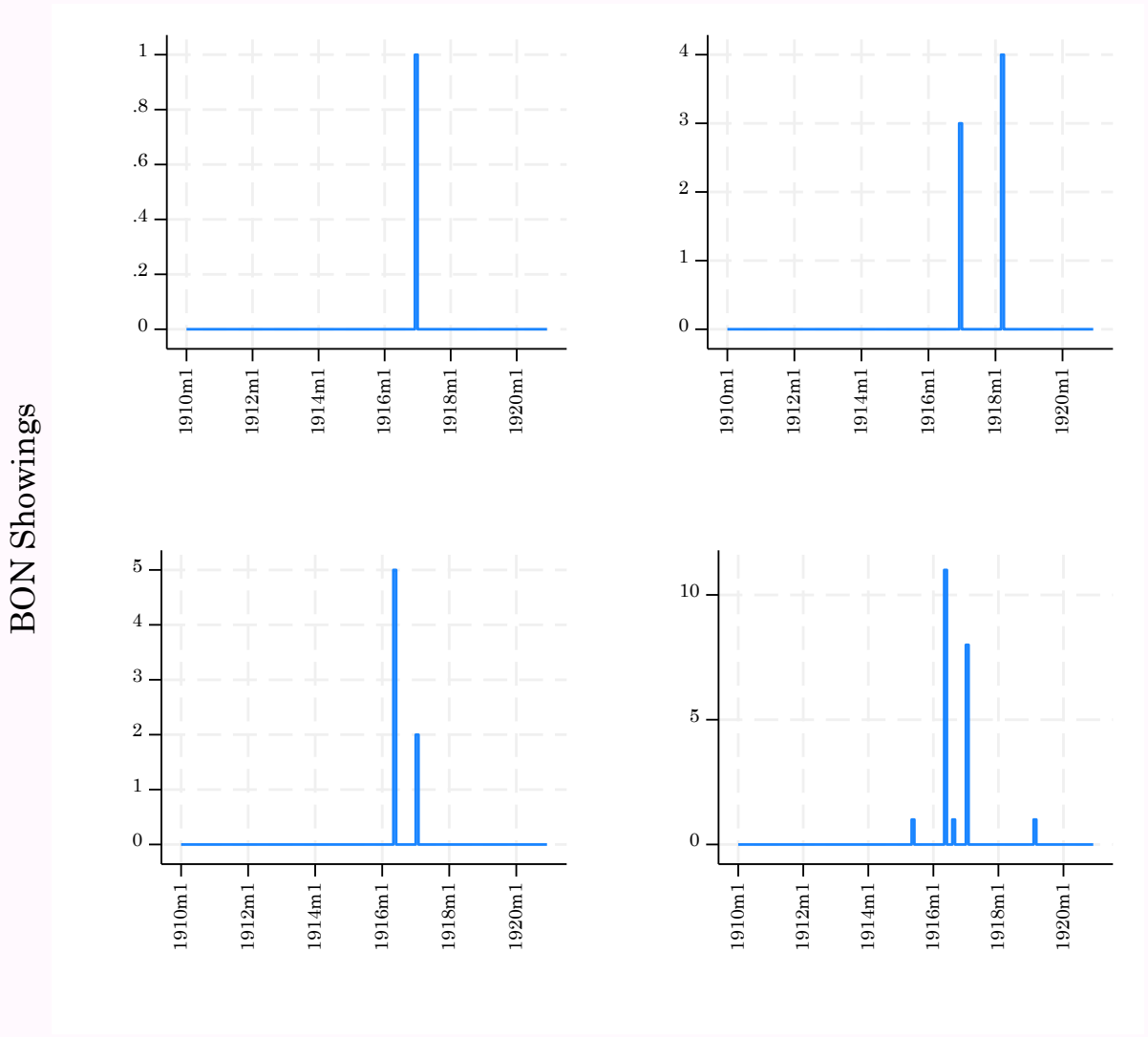
Montana

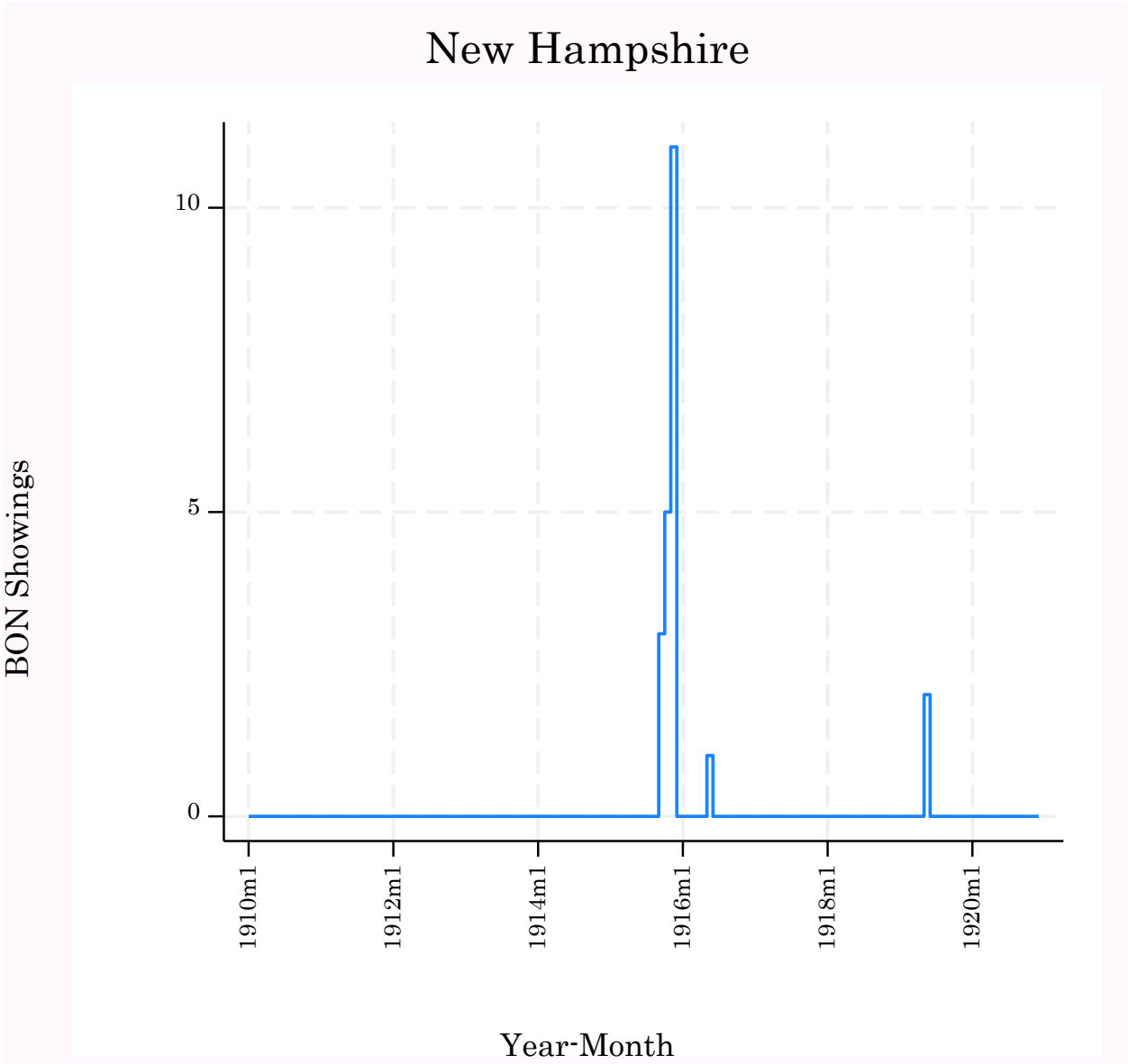

BON Showings
Year-Month
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1

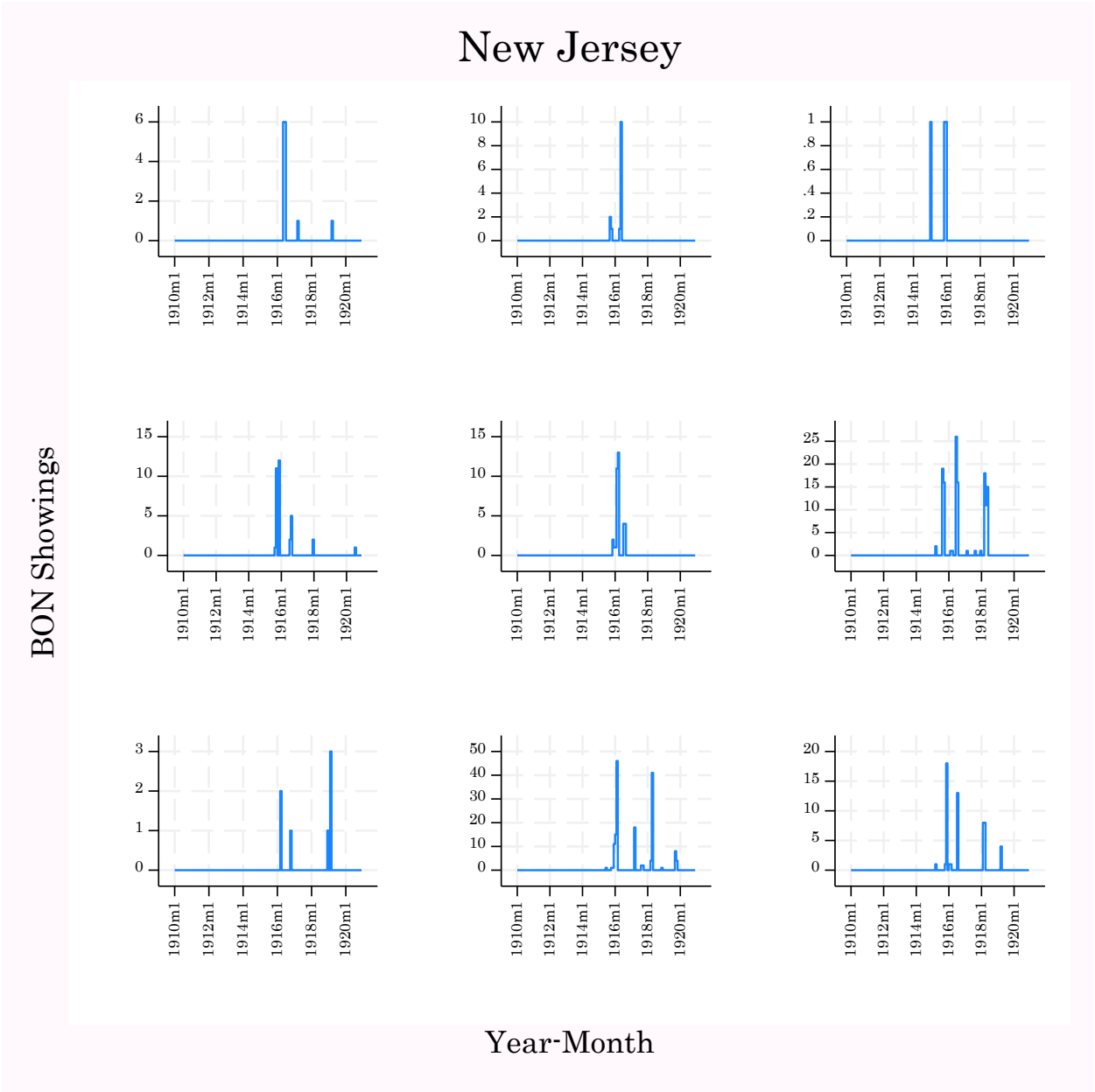
Nebraska


BON Showings
Year-Month
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1

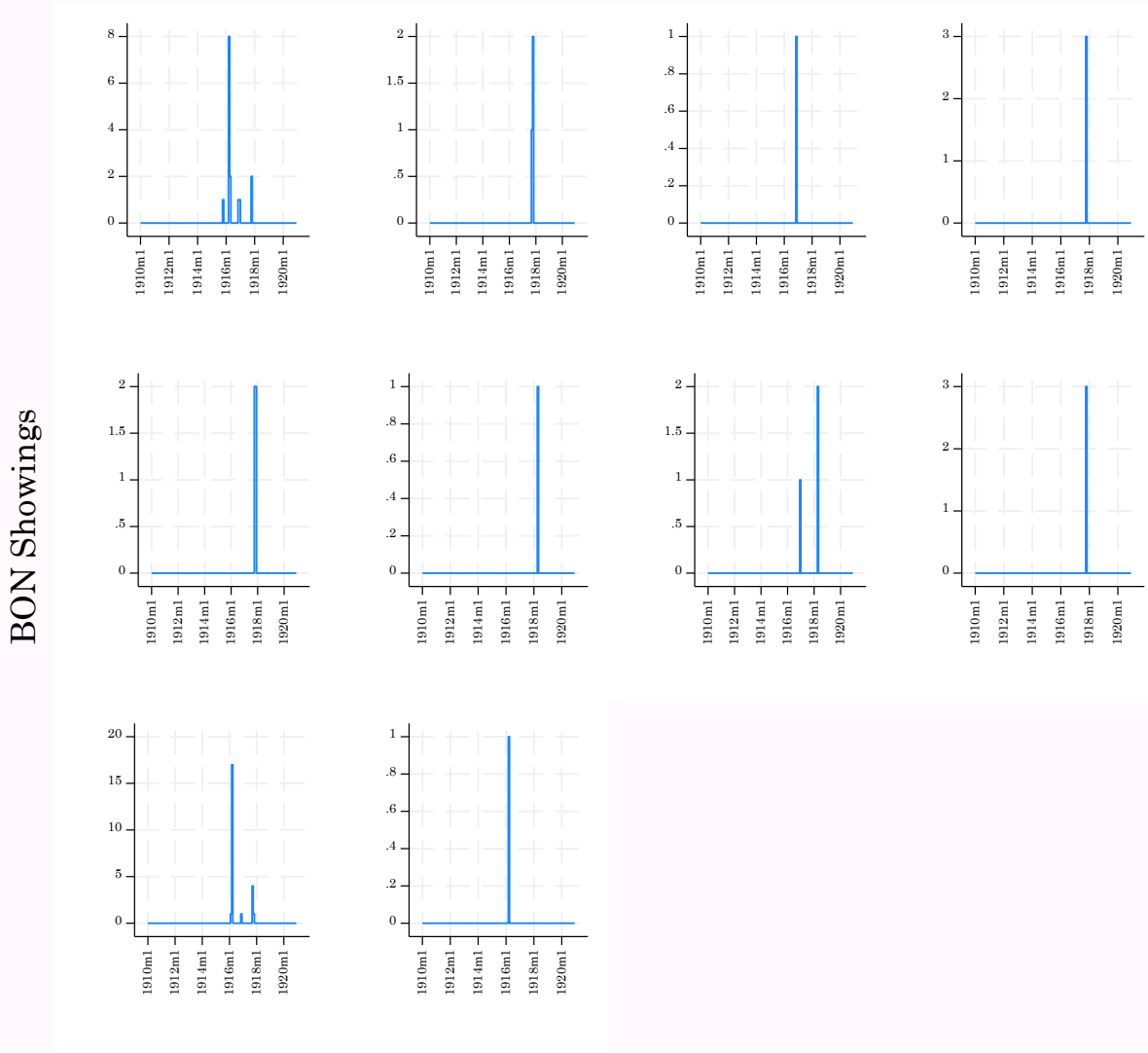
Nevada


BON Showings
1
.8
.6
.4
.2
0
4
3
2
1
0
5
4
3
2
1
0
10
5
0
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1
Year-Month

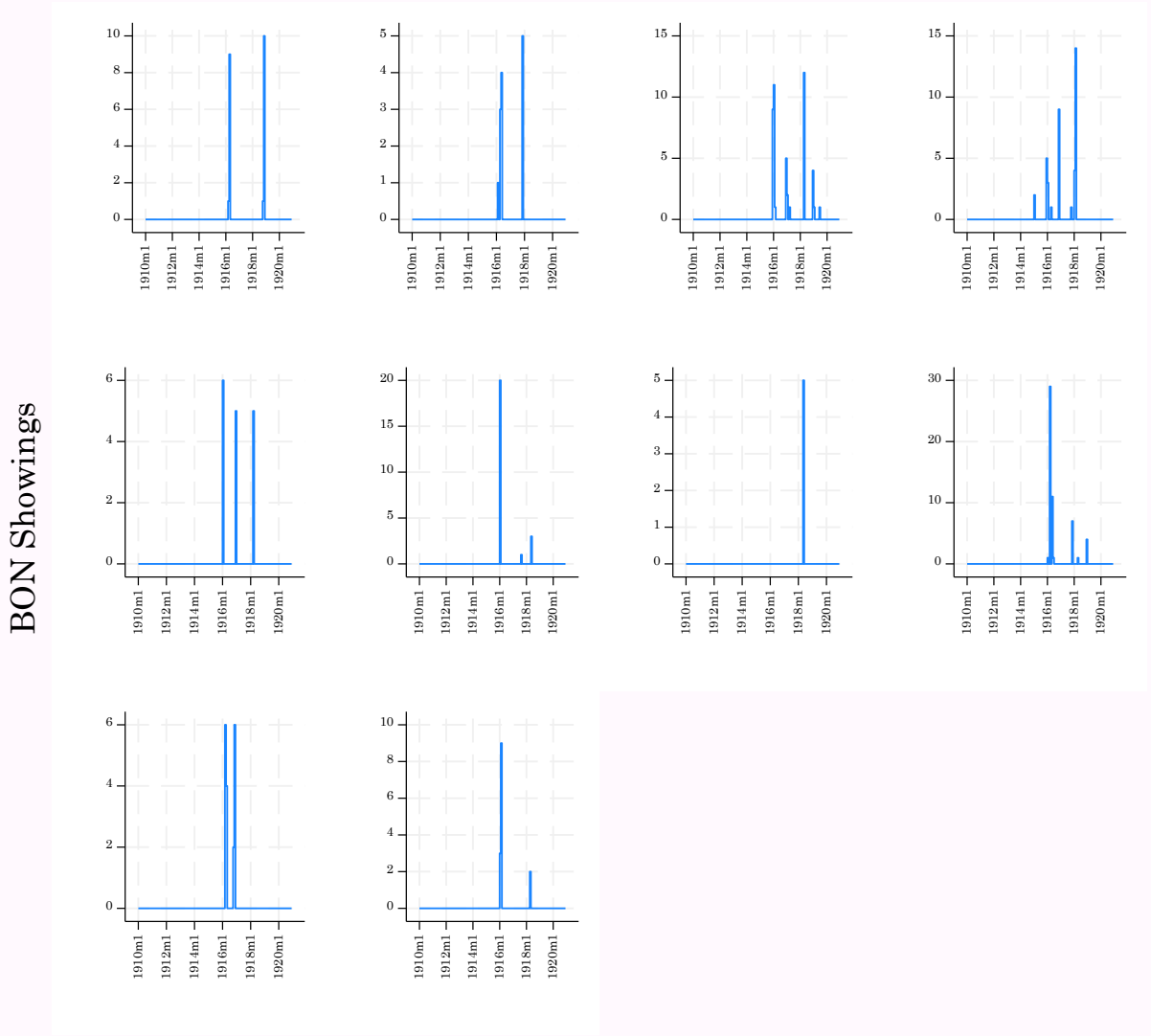

New Hampshire
BON Showings
10
5
0
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1
Year-Month


New Jersey
BON Showings
Year-Month
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1

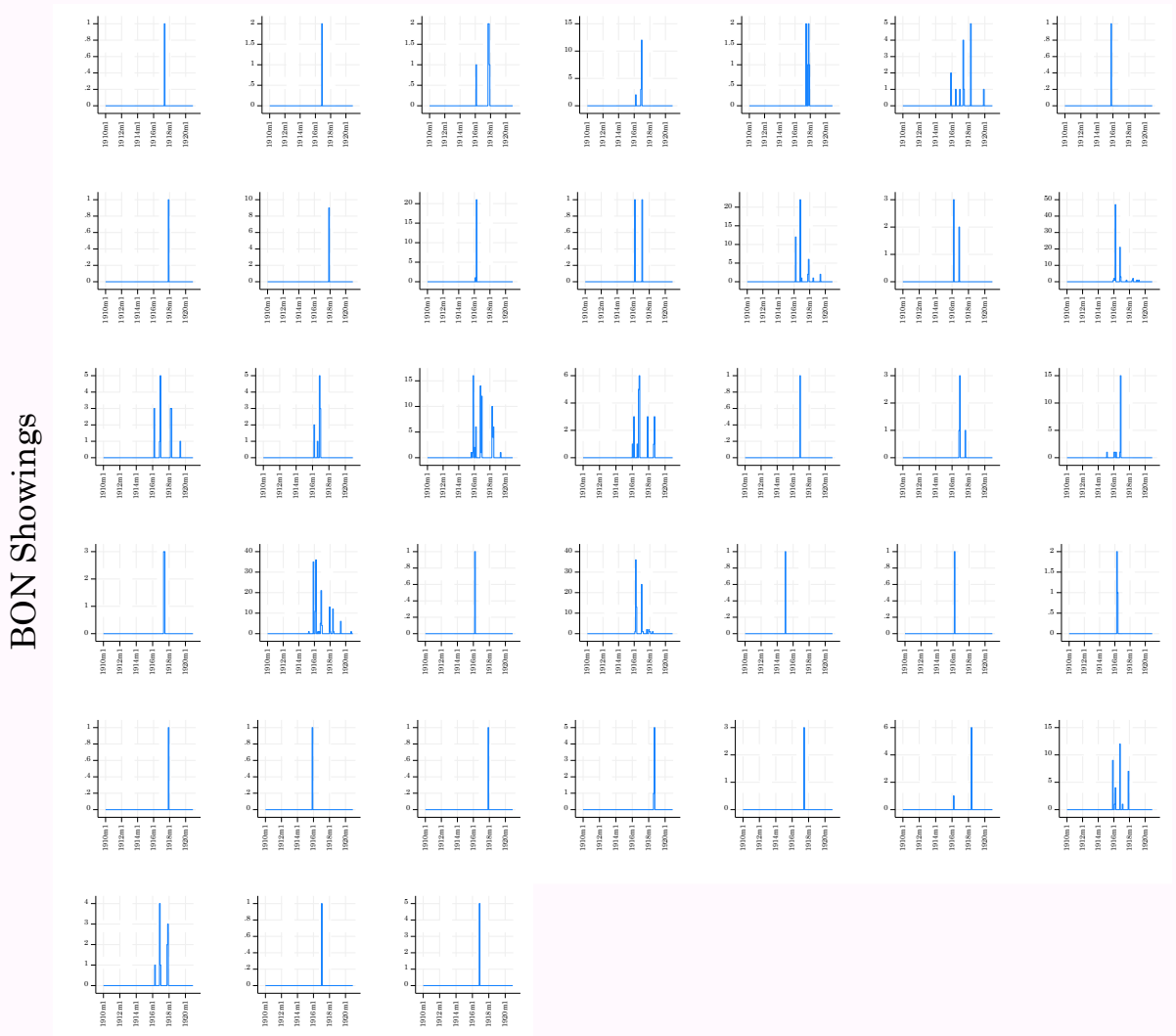
New Mexico


BON Showings
Year-Month
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1

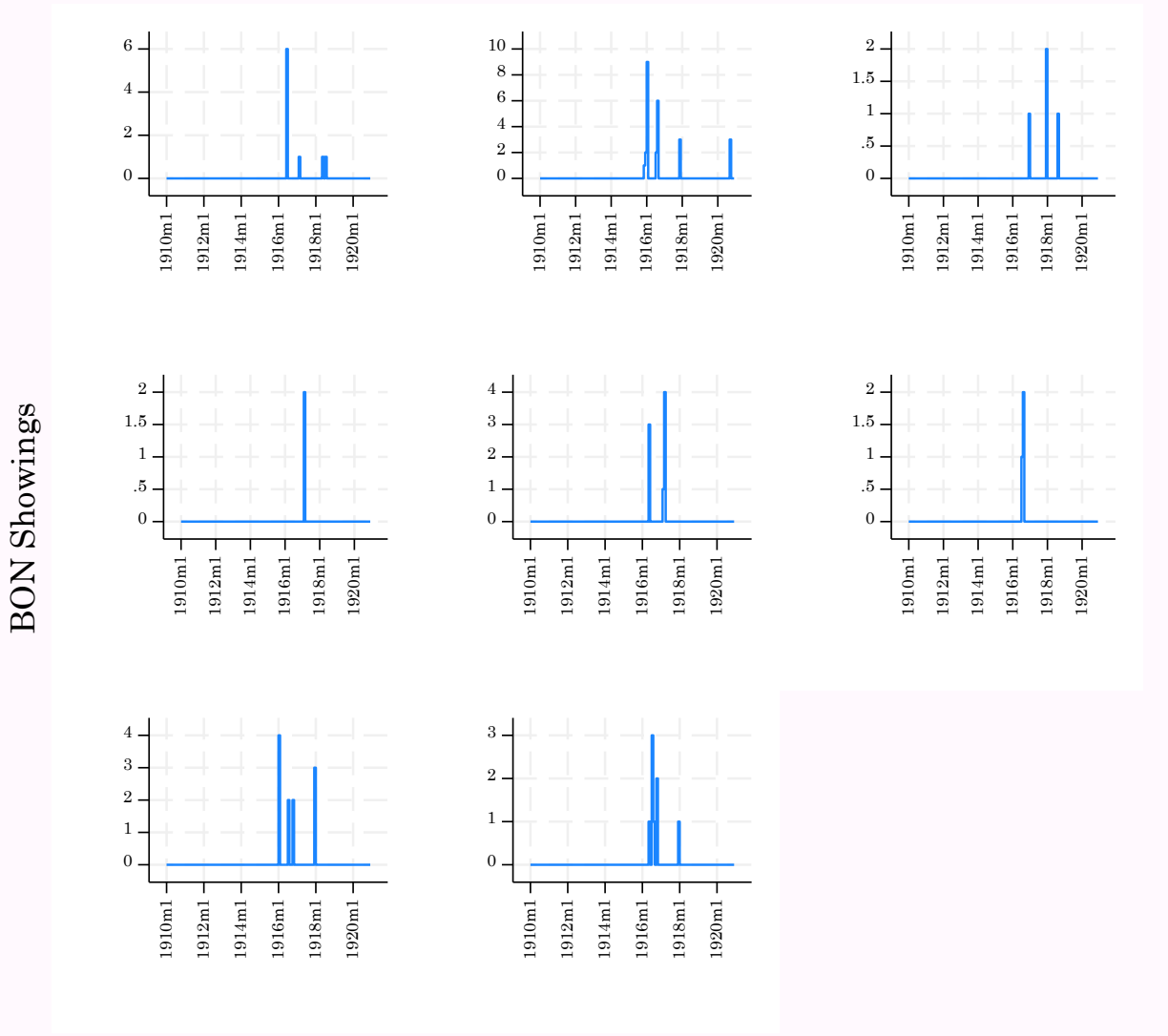
New York


BON Showings
Year-Month
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1

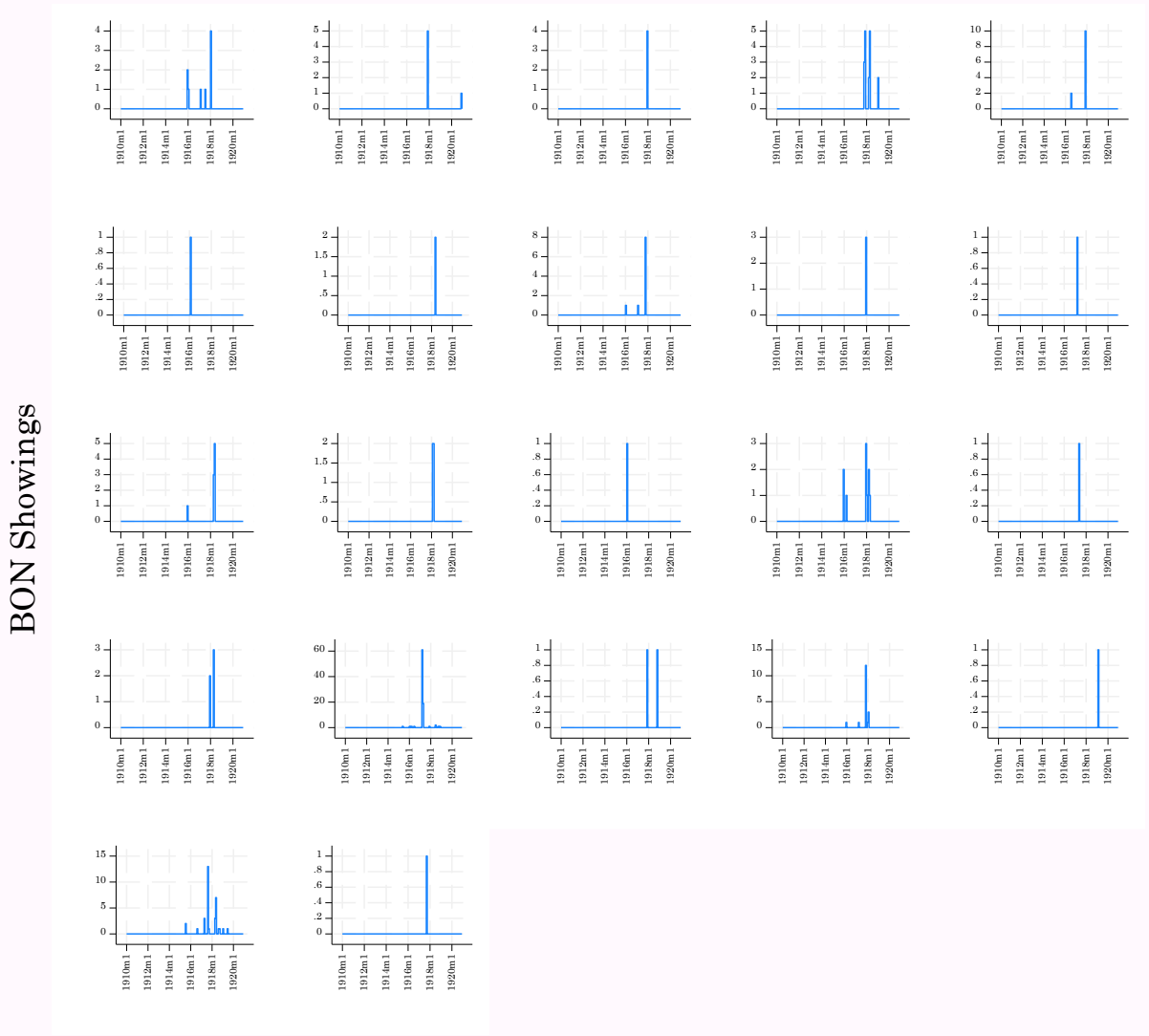
North Carolina



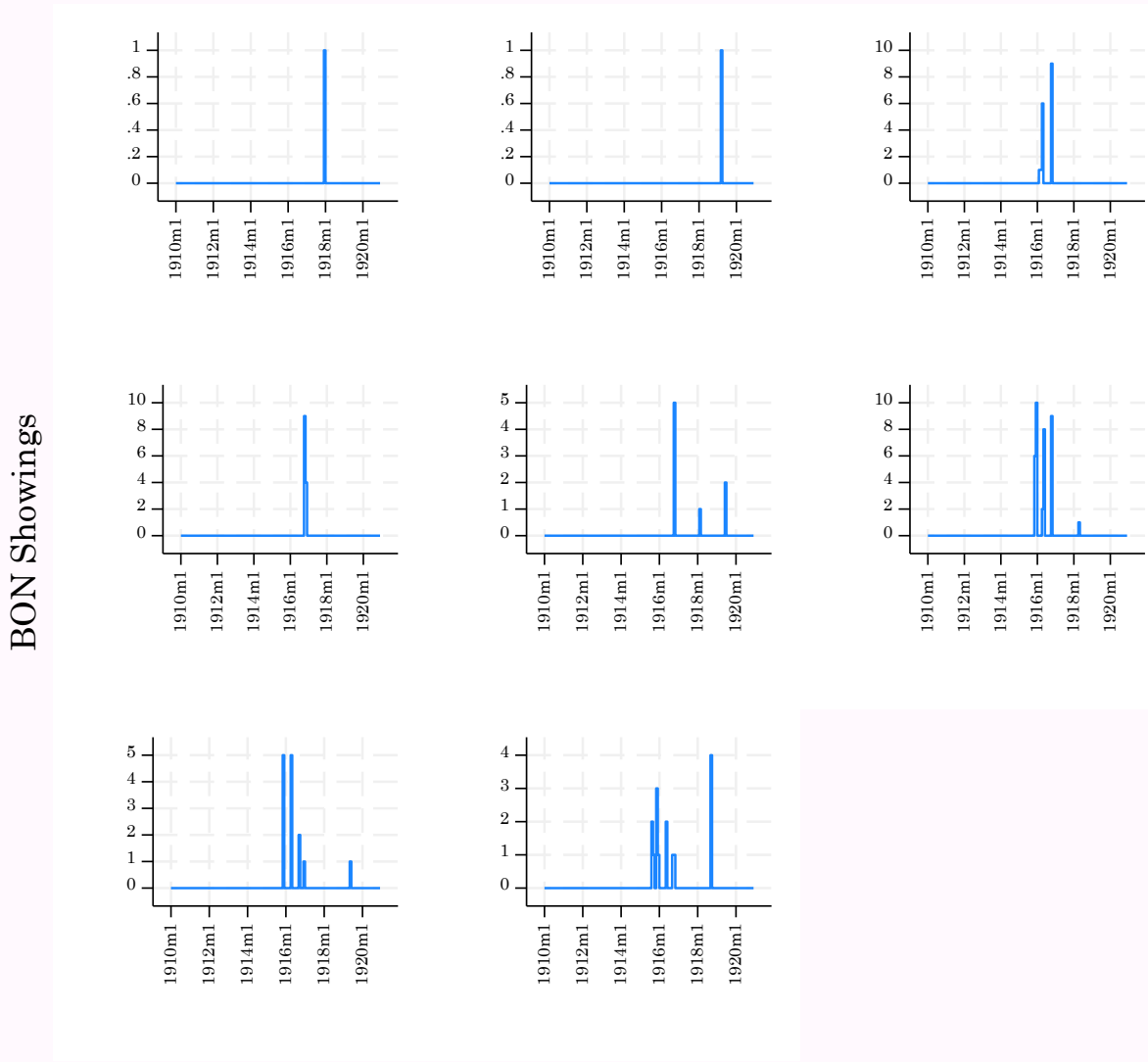
North Dakota



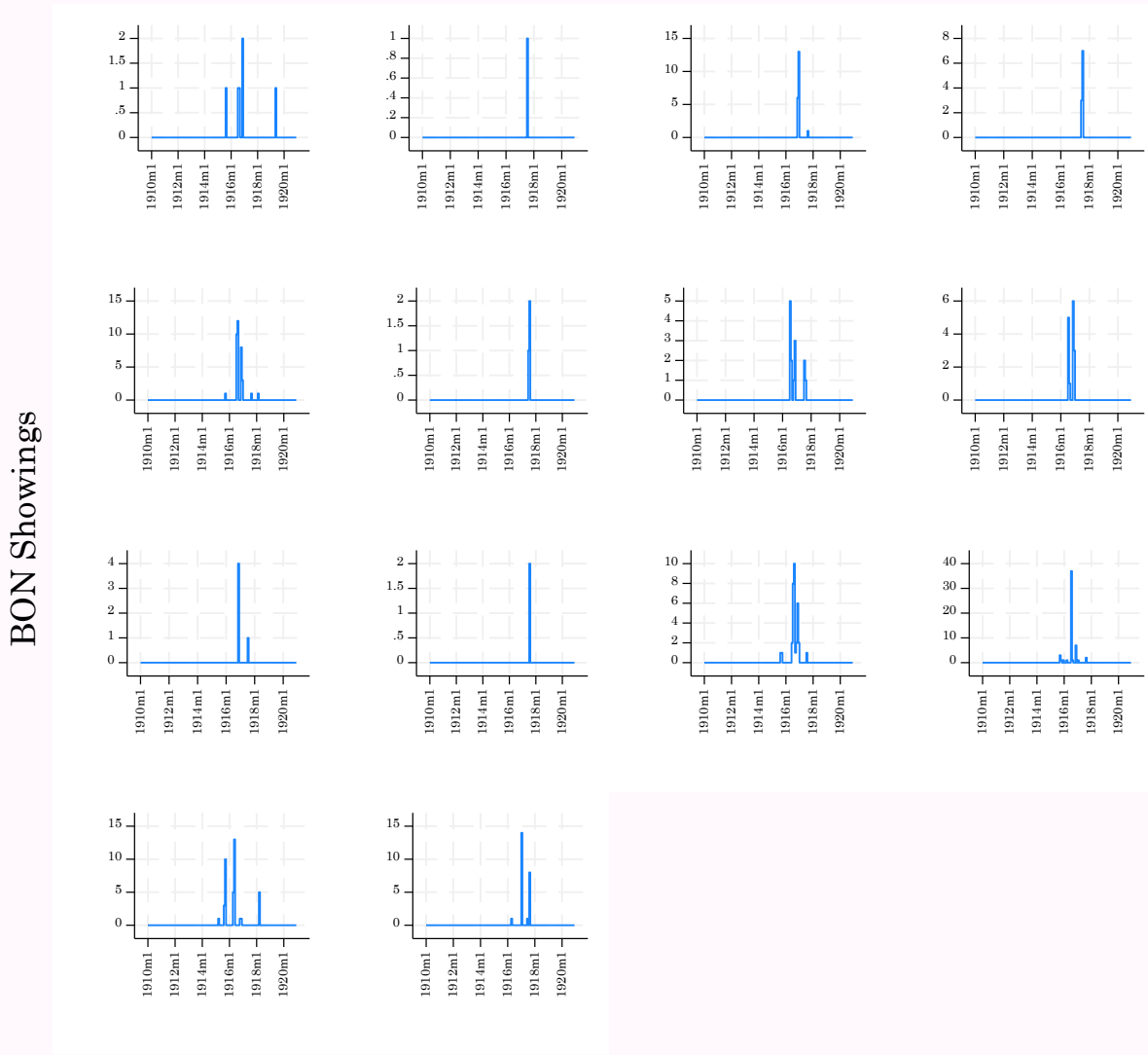
Ohio



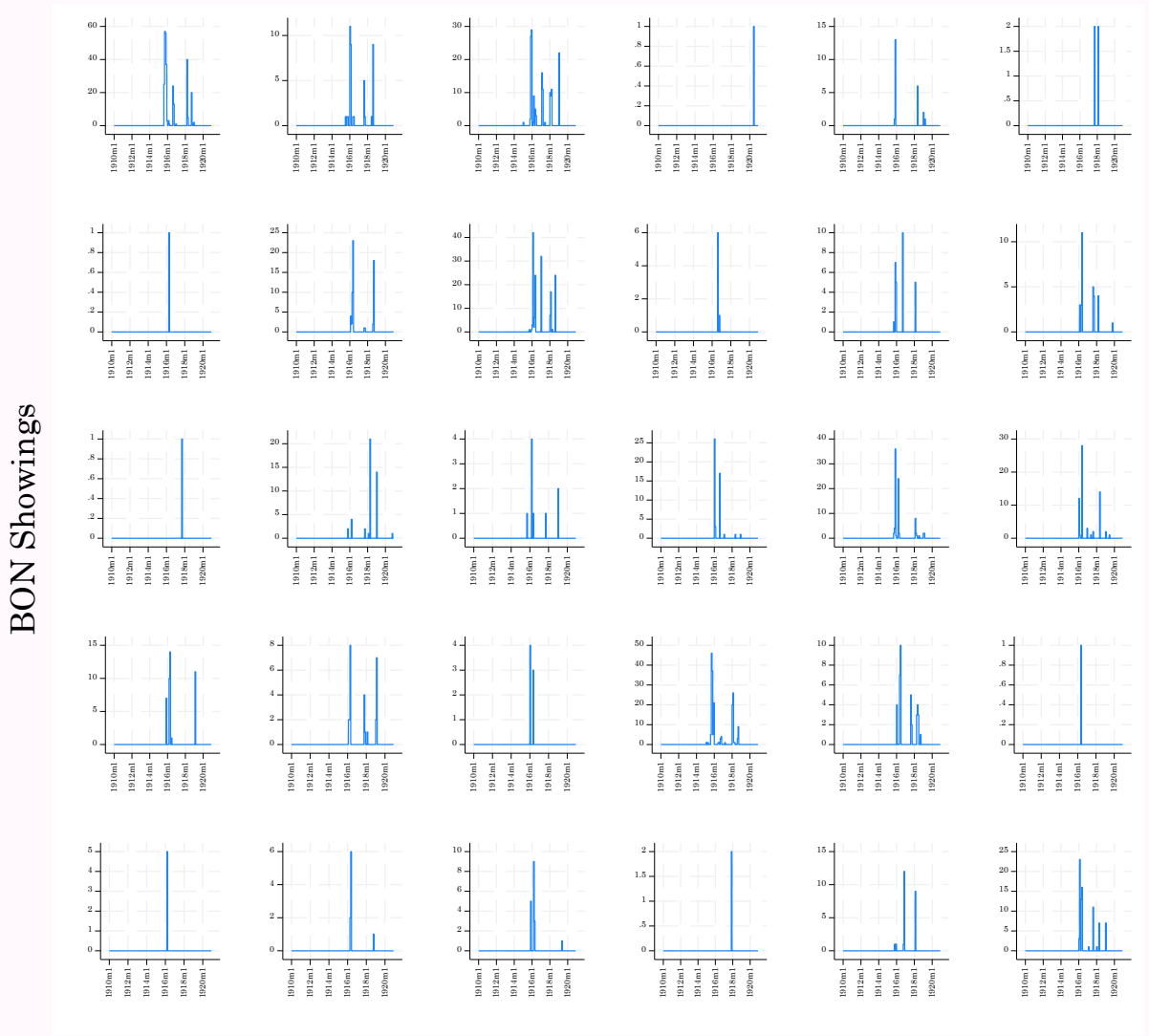
Oklahoma


BON Showings
Year-Month
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1

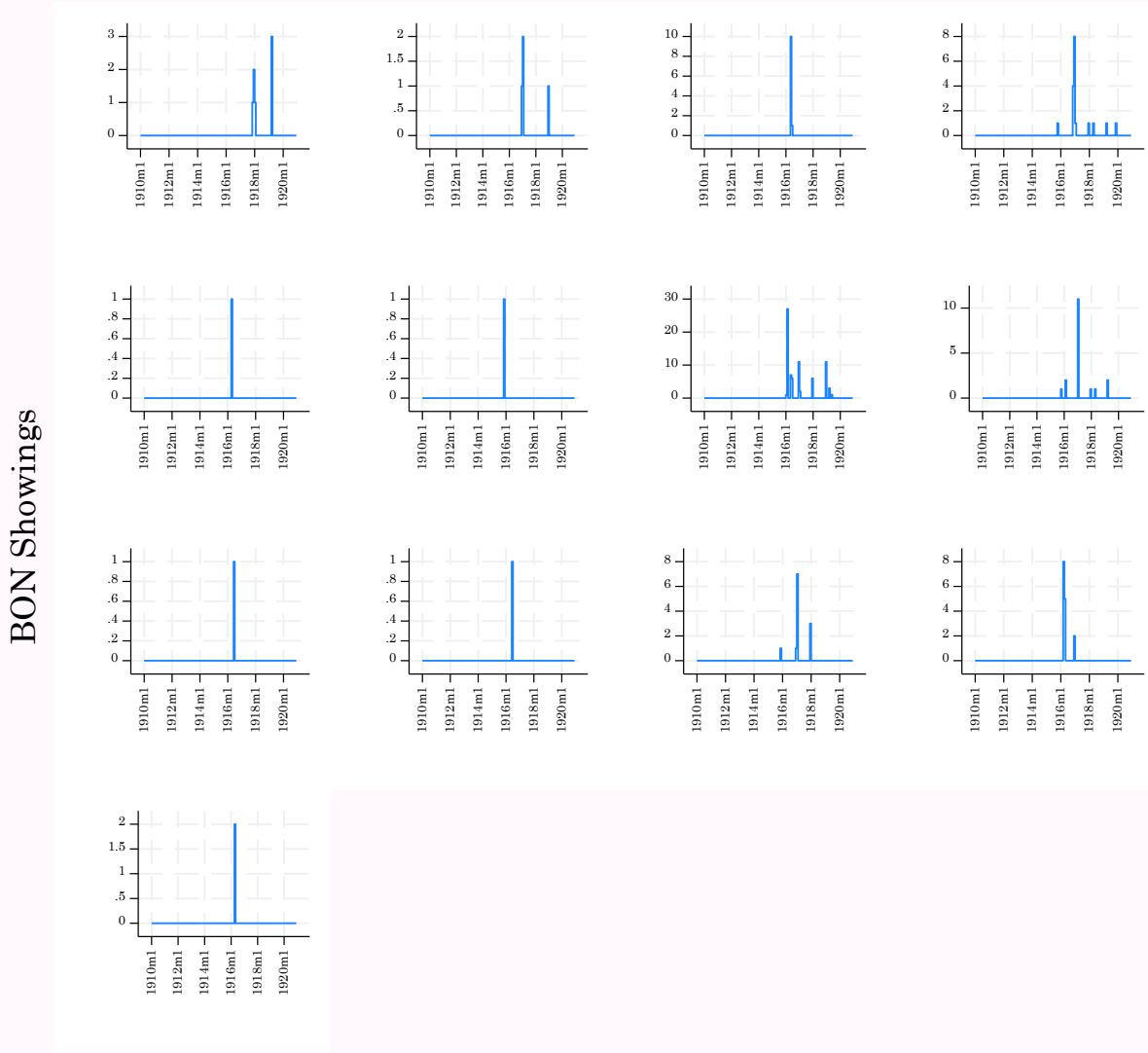
Oregon


BON Showings
Year-Month
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1

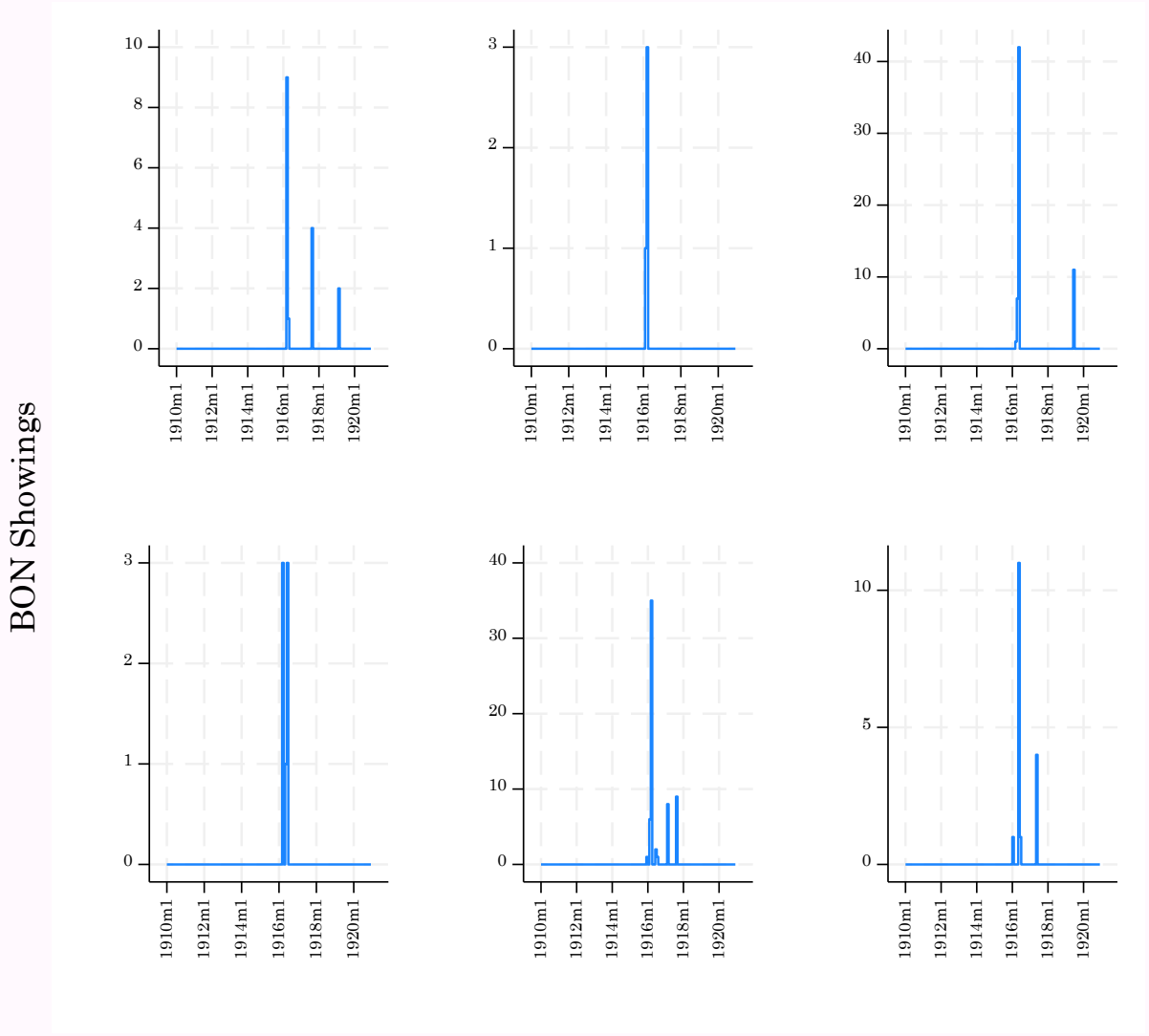
Pennsylvania

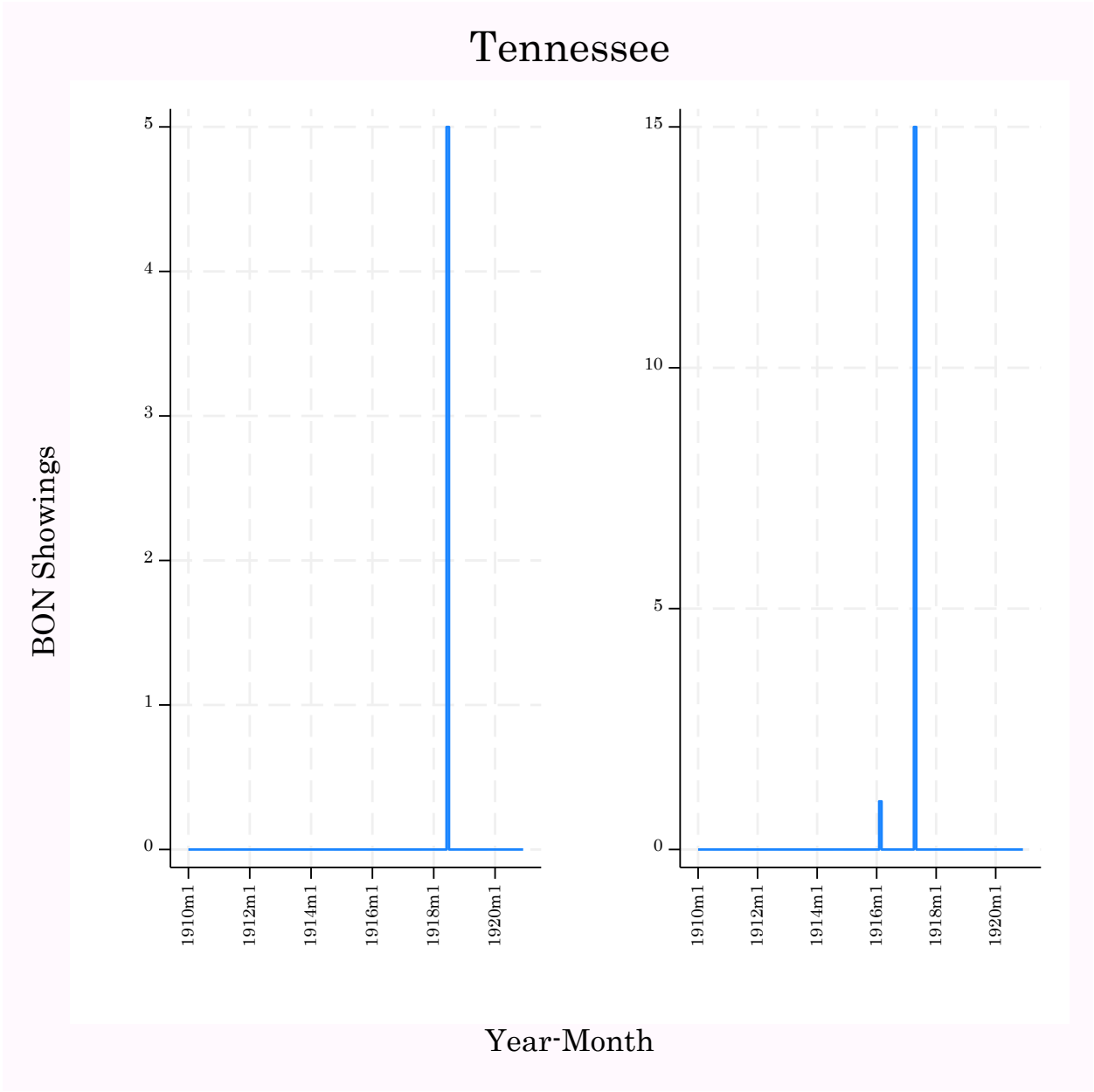


South Carolina

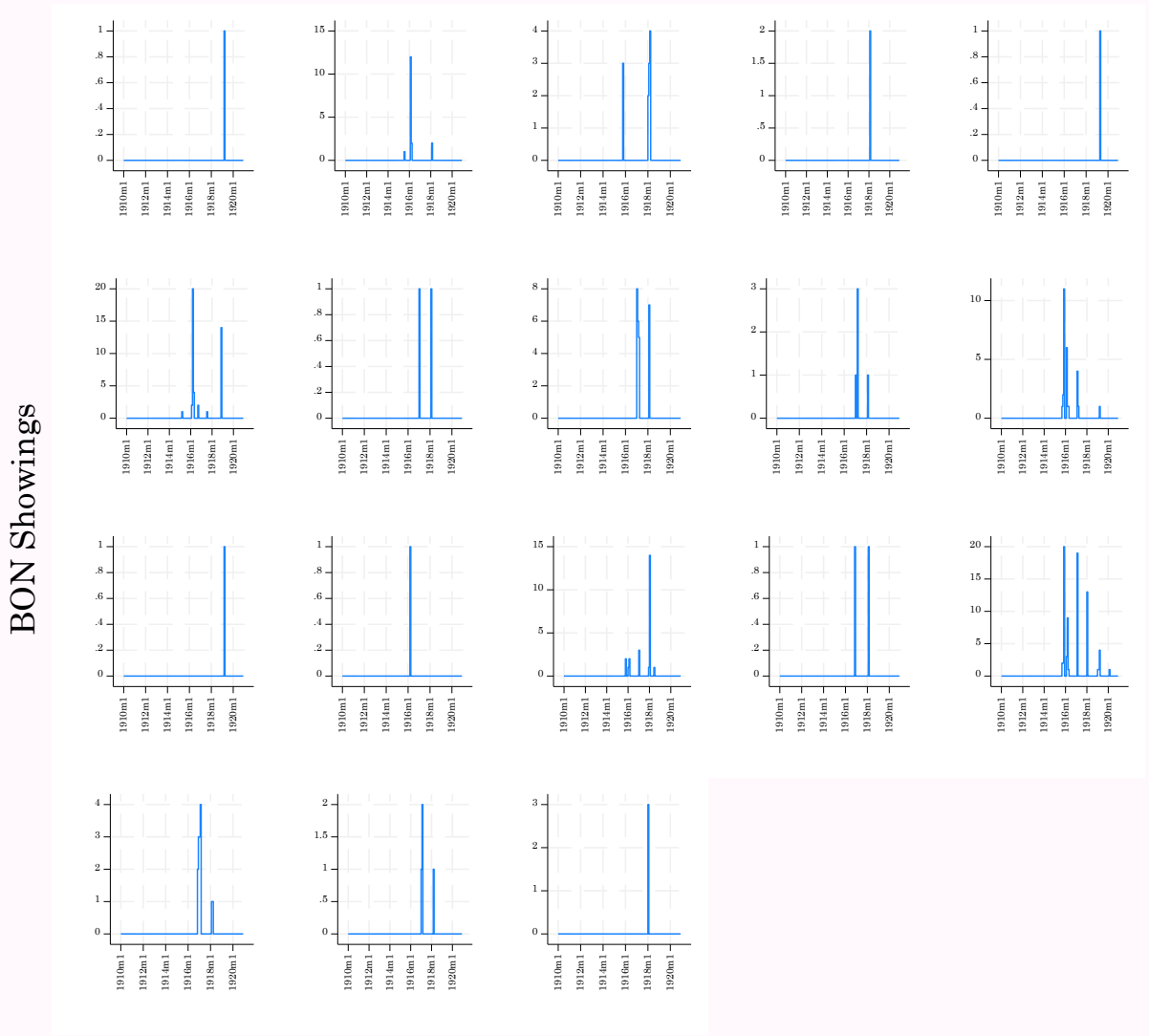


South Dakota

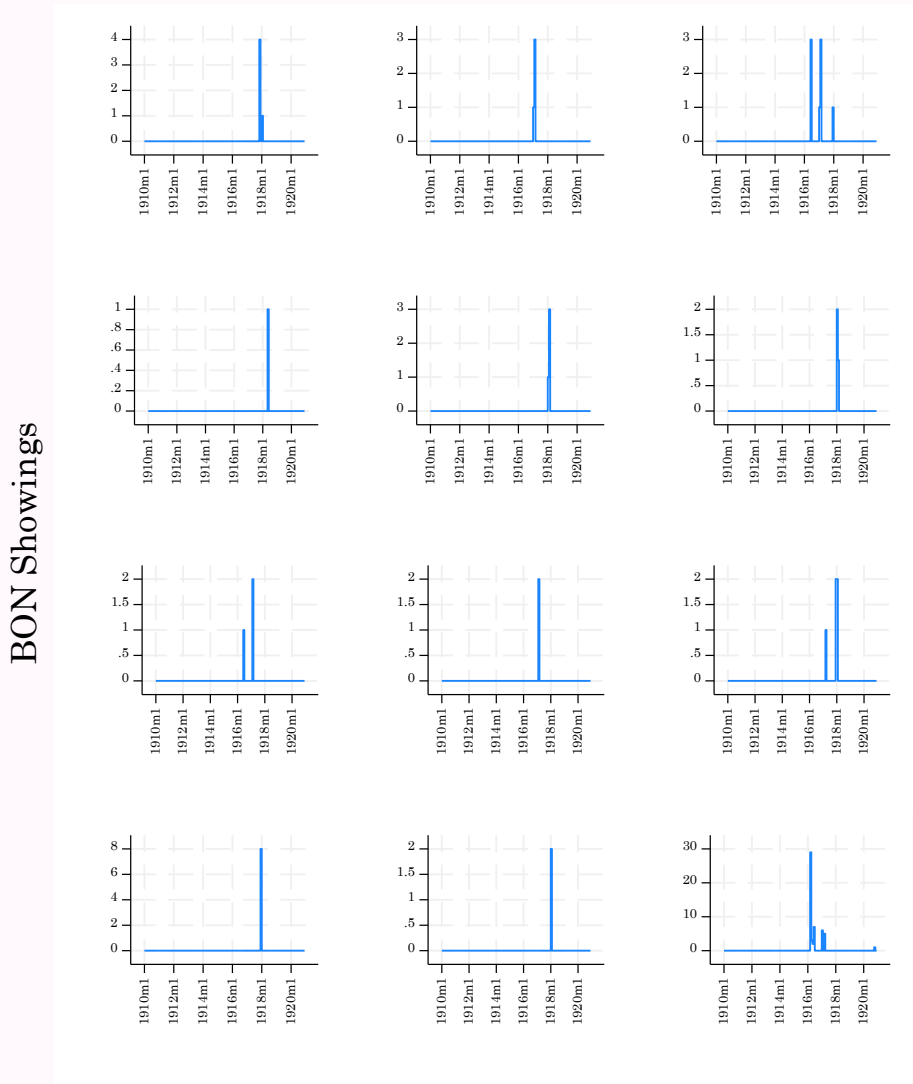


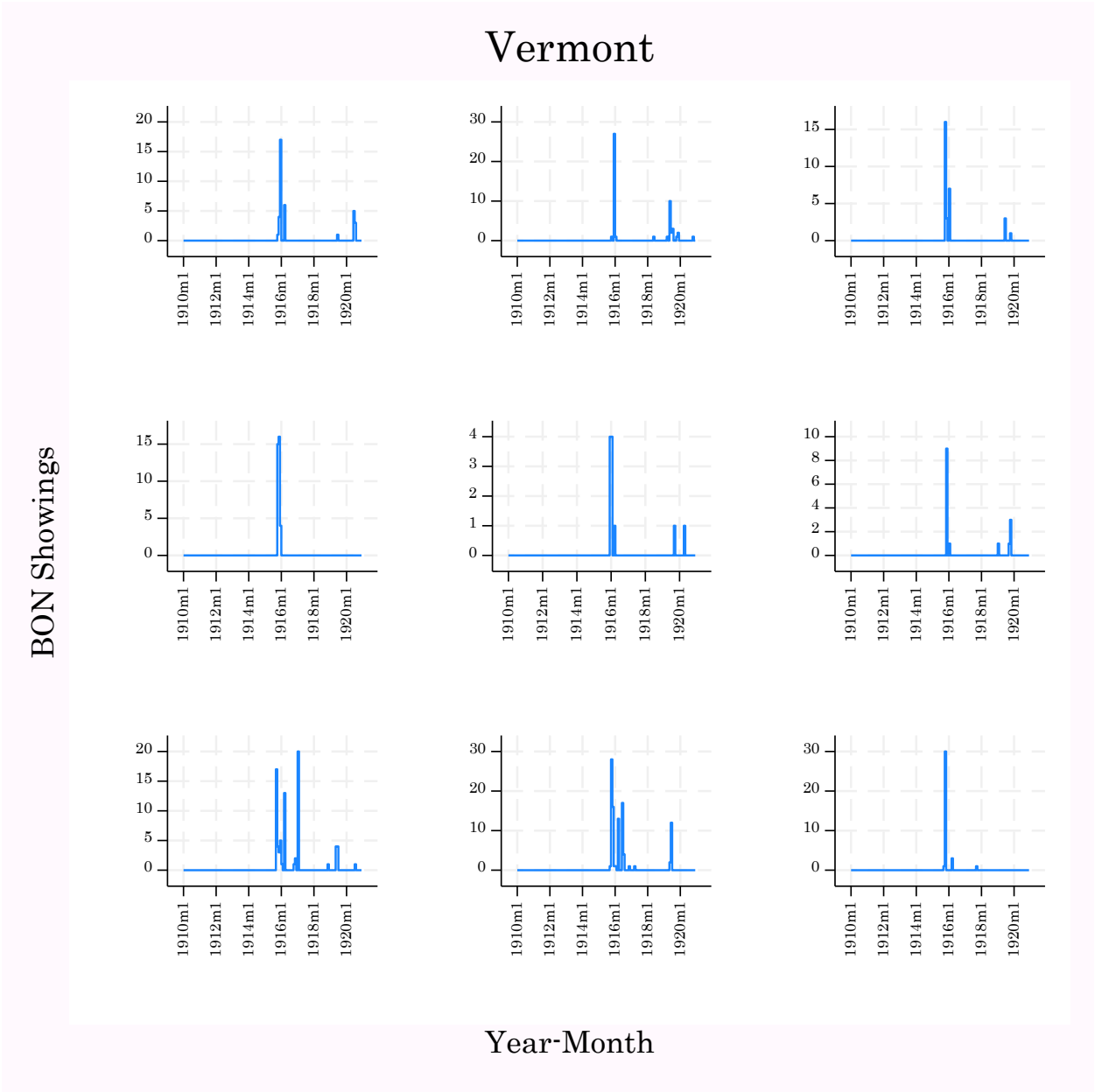

Tennessee
BON Showings
0
1
2
3
4
5
0
5
10
15
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1
Year-Month

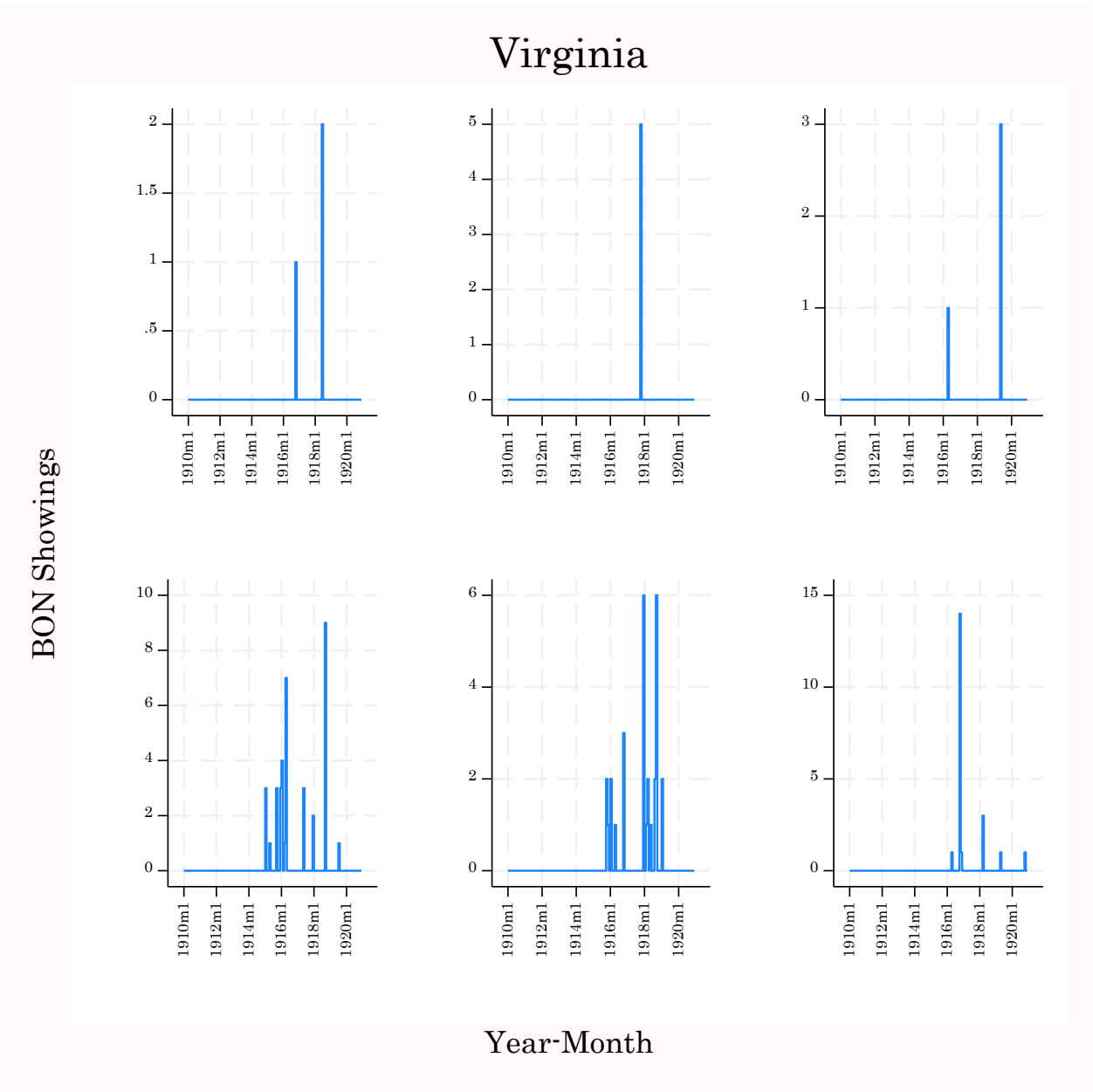
Texas



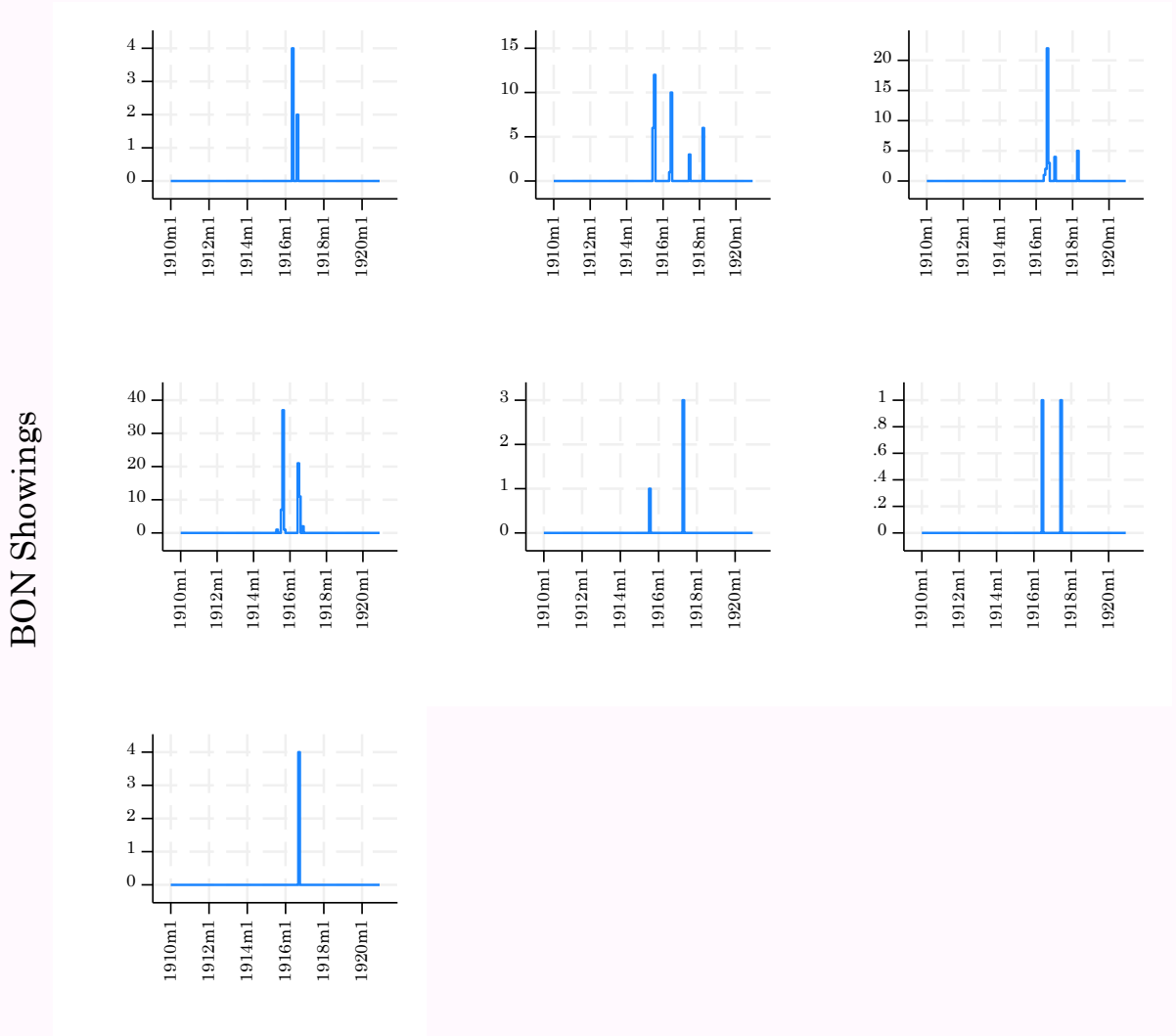
Utah


BON Showings
Year-Month
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1

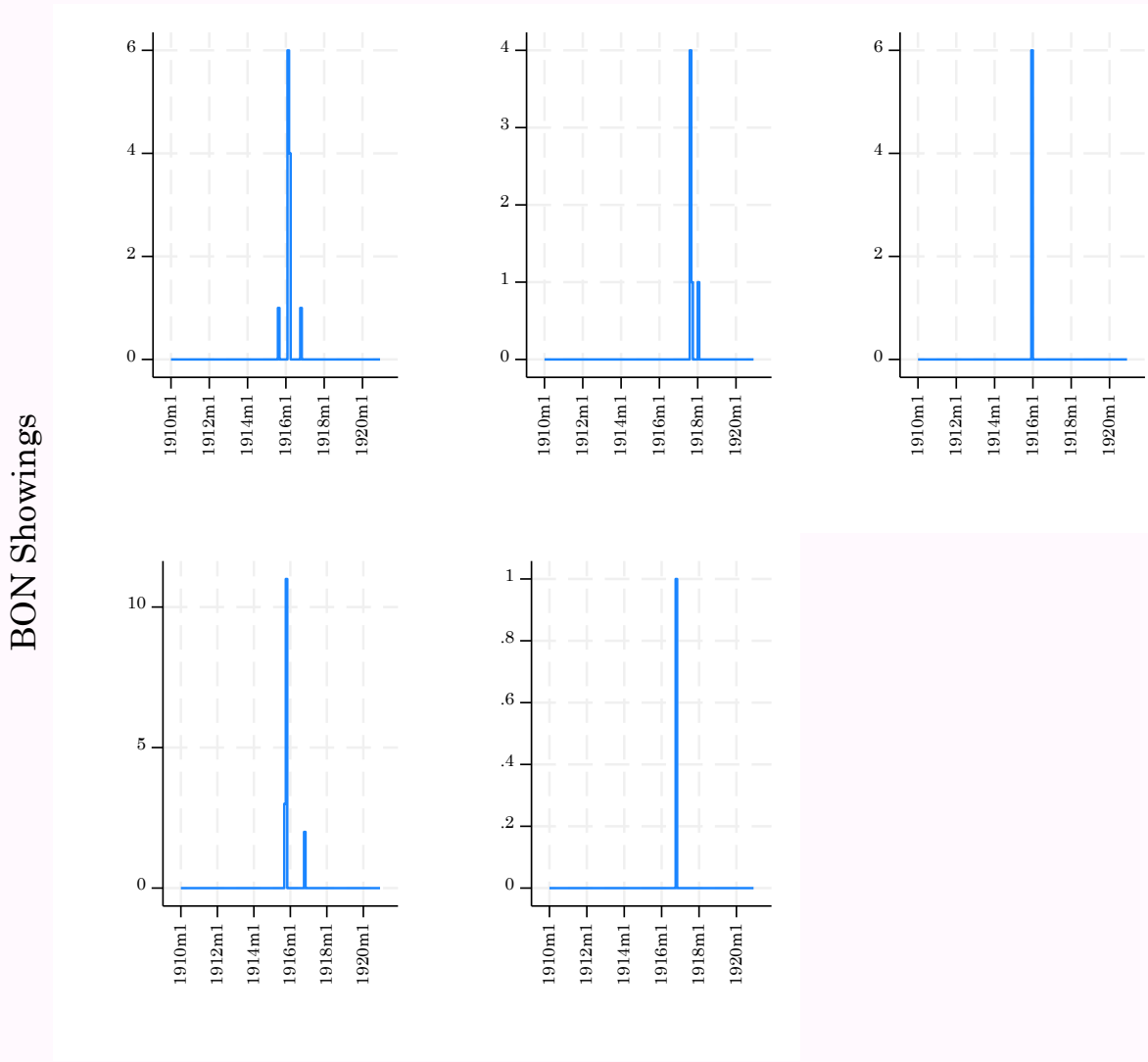

Vermont
BON Showings
Year-Month
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1


Virginia
BON Showings
Year-Month
0
.5
1
1.5
2
0
1
2
3
4
5
0
1
2
3
0
2
4
6
8
10
0
2
4
6
0
5
10
15
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1

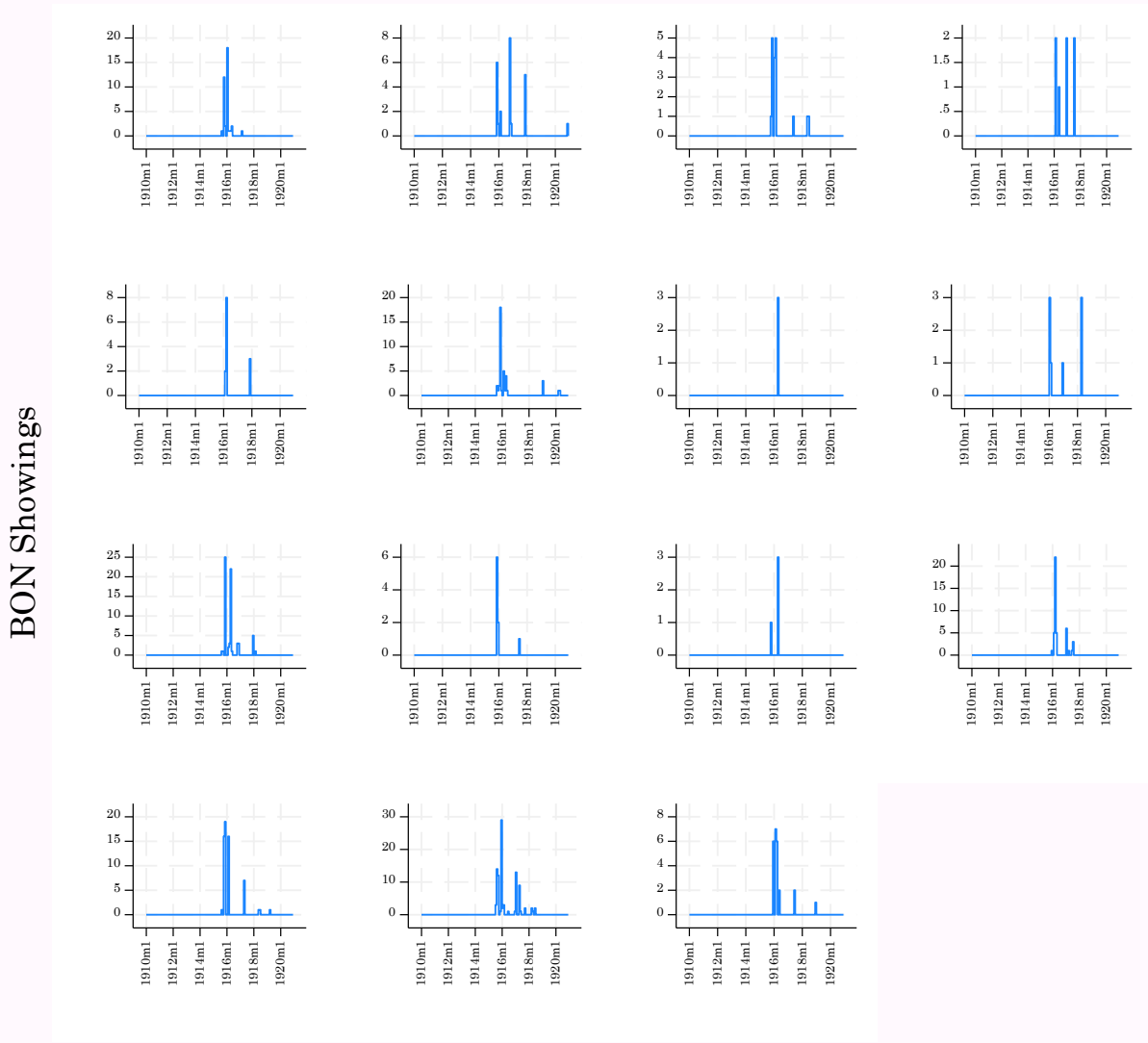
Washington

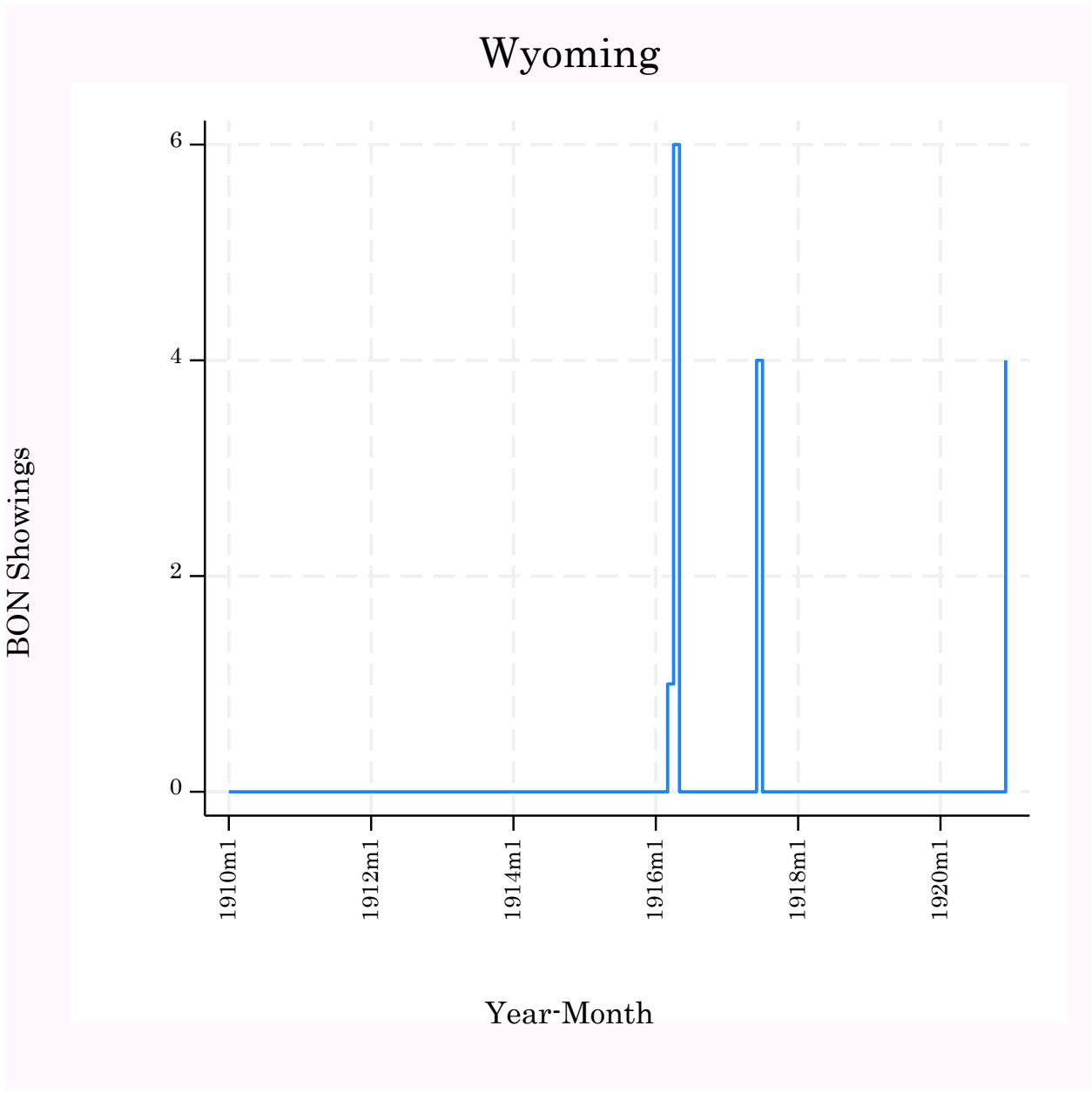

BON Showings
Year-Month
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1

West Virginia



Wisconsin




Wyoming
BON Showings
6
4
2
0
1910m1
1912m1
1914m1
1916m1
1918m1
1920m1
Year-Month