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Testing the economics of the net neutrality debate

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ABSTRACT

This paper examines the impacts of net neutrality rule changes in the United States in 2010, 2015, and 2017 on telecommunication industry investment levels. The paper tackles the issue with a comprehensive dataset with full time series coverage for all SEC-registered telecommunications firms from 2009 to 2018. The author tracks new capital expenditures incurred, which reflects new investment decisions made rather than old investment decisions materialized, with quarterly data and exact issuance dates. The paper uses a standard difference-in-difference model and a variety of robustness checks to examine causal impact effects of net neutrality rule changes. The paper finds net neutrality rule changes in the United States had no impact on telecommunication industry investment levels based on the data, outcome variable, and limiting assumptions used. This empirical question featured heavily as the key economic question in the 2015 Open Internet Order issuance and 2017 repeal process. However, research has failed to adequately address the question due to a short timeframe for research and methodological issues. The paper offers the only analysis with a full dataset and a sufficient time period of observation to properly examine effects. As such, it offers an incremental step forward in research assessing network neutrality empirically.

1. Introduction

This paper takes us once more unto net neutrality. More specifically, the author offers an assessment of the key economic argument featured on both sides of the debate during the issuance of the 2015 US Open internet Orders and its subsequent 2017 Federal Communications Commission's repeal – the impact of the US Title II ruling on telecommunication network infrastructure investment.

The author argues previous empirical assessments of this issue, from all sides, have largely been plagued by shortcomings. Prior to the 2017 repeal, empirical testing of the issue was essentially non-existent. Theory and modeling dominated the discussion, showing a range of possible outcomes. During the 2017 repeal process, empirical analyses lacked sufficient time and data and suffered from key methodological issues to draw robust conclusions. During this process, partisanship and industry agendas dominated. The result has been an economics and telecommunications literature set with little empirical clarity and no consensus on arguably the most important economic aspect of net neutrality (hereafter NN) policy.

The author attempts something novel in the current paper. The analysis returns to the investment issue armed with the luxuries of time and data to build on the empirical work already done. The paper offers the first assessment on the causal impacts of NN on industry investment using robust methods and a comprehensive dataset that avoids the flaws of previous research. The author uses quarterly data from the Securities and Exchange Commission (SEC) from Q1 2009 to Q3 2018 and examines capital expenditure as a proxy for industry network investment, following guidance of previous empirical studies from both sides of the debate. The author

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improves upon previous studies by using newly *incurred* capital expenditures tracked to exact days (dates), rather than *current* capital expenditures that reflect decisions often made years before the actual expenditure dates. Using this full time series coverage and more appropriate metric of investment, the analysis provides insight into the reaction of the telecommunications industry directly to NN rule changes. The range and quality of the data mean that the author has a time series with full coverage of the 2010 Open Internet Order, the vacating of the 2010 order in 2014, the 2015 Open Internet Order, and the 2017 vote to repeal as well as several periods of observation before and after each of these regulatory actions. This coverage provides, to the extent of the author's knowledge, the only empirical assessment of its kind on this issue.

The results of the paper are clear and should be both unsurprising and uncontroversial. The key finding is there were no impacts on telecommunication industry investment (as measured by new capital expenditures incurred) from the NN policy changes. Neither the 2010 or 2015 US NN rule changes (run as separate analyses) had any causal impact on telecommunications investment. The treatment indicator coefficients in the analyses were all insignificant. Furthermore, the scale of the coefficients (while not significant) only represent fractions of industry total investment. The results also stand up through multiple alternative specifications and robustness checks. This suggests that even if the current analysis is lacking sufficient controls to capture the true treatment effect – something the author views as unlikely given the quality of the model fit and robustness checks – the actual scale of impact would be very minimal. Perhaps most importantly, the results highlight the lack of nuance in previous studies and the importance of returning to fundamental theory on NN policy and its economic impacts.

The author begins by reviewing the core concepts, key timeline of policy actions, and the theoretical framework underlying the debate on NN and telecommunications investment in Section 2. The author then reviews the empirical literature, both before the 2017 repeal process and from that process onward, in Section 3. In Section 4, the author presents the paper's methodology including details on data, empirical strategy and model, and limitations. The author presents results in Section 5 and a discussion in Section 6. The author concludes in Section 7.

2. The theory of NN and network investment

2.1. Core concepts and timelines

The simplified principle of NN is that all internet traffic should be treated equally without preference or bias to a particular group or type of traffic.

Arguments in support of NN emphasize the prevention of anti-competitive practices and potential positive effects that prevent systematic advantages in markets based on resources. There are several key aspects to these arguments: 1) the prevention of discriminatory pricing arrangements and content delivery practices; 2) the prevention of capital barriers for new market entrants; 3) the prevention two-sided pricing (the practice of charging businesses for the ability to use an internet network while simultaneously charging consumers for the ability to access that business product/service); and 4) the prevention of multiple network access fees (Economides & Tag, 2012; Faulhaber, 2011; Greenstein, Martin, & Valletti, 2016; Wu, 2006).

Arguments opposing NN theorize harms to the telecommunications sector, particularly disincentives to investment and innovation.[1] A primary claim is that NN leads to a reduction in investment incentives among telecommunications firms. Additionally, opponents argued (in the US policy debate) that ISP networks have capacity issues due to the inability to differentiate content demand and supply. Finally, there are also arguments that consumers benefit without NN rules through improved package offerings that better fit demand preferences (Cheng, Bandyopadhyay, & Hong, 2011; Choi & Kim, 2010; Faulhaber, 2011; Litan and Singer, 2007).

In the US context, the NN debate has centered around three specific aspects: 1) Content should not be blocked – i.e. internet service providers (ISPs) should not forbid some content or groups while allowing others; 2) Content should not be throttled – i.e. the intentional slowing of internet service for some groups/platforms by ISPs; and, 3) Content should not be able to receive prioritization by paying fees (i.e. paid prioritization) – i.e. platforms and content providers should not be able to pay to have their content prioritized over that of others.[2] There are, of course, more nuances to the issue and there is ample theoretical work in law and economics describing them in detail. It is beyond the scope of the current paper to comprehensively rehash these and the author recommends Krämer, Wiewiorra, and Weinhardt (2013) for an extensive overview of the fundamental principles and key research.

Within this debate, there are a few key dates and actions on which empirical studies have focused. First, the FCC approved the 2010 Open Internet Order on December 21, 2010. This set of regulations laid out six conceptual NN principles and tenets. On January 14, 2014, the DC Circuit Court determined in its Verizon Communications v. FCC ruling that the FCC did not hold the enforcement authority for net neutrality rules, thus vacating the 2010 order. Verizon had challenged the 2010 order and the DC Circuit Court ruled that the order was not consistent with Title I authority since it presumed any paid prioritization violated its non-discrimination standard. On April 13, 2015, the FCC issued the final rule implementation for its 2015 Open Internet Order. This ruling reclassified broadband as a common carrier under Title II of 1934 Communications Act and effectively instituted NN rules in the United States.

[1] The author notes that the paper addresses only the former issue of investment and not the latter issue of innovation in order to maintain a realistic scope for a single paper. I discuss in more detail later.

[2] Additionally, there is a fourth component around zero rating or the practice of allowing certain content access without cost (such as in the case of free video streaming for certain apps on certain carriers). However, this fourth component was less contended in the debate since it was not part of the 2015 Open Internet Order.

Finally, the FCC under new leadership voted on December 14, 2017 to repeal the 2015 Open Internet Order policies with the repeal becoming effective on June 11, 2018. Advocates of NN are currently challenging this most recent action in court.

It is important to emphasizes here that the 2010 Open Internet Order and 2015 Open Internet Order represent different regulatory actions. The 2010 order did not prohibit paid prioritization, but rather applied a non-discrimination standard to them. Additionally, the 2010 order did not classify ISPs as telecommunication firms under Title II while the 2015 order did. Some researchers have argued that there would have been no expectation of ISP investment decline because of the unique aspects of the orders; however, other researchers have indeed argued that the 2010 Open Internet Order resulted in investment decline (Ford, 2017a, for example). The author notes these dates to provide clear guidance on its analysis later and notes the paper conducts analysis for both the 2010 and 2015 order to ensure comprehensiveness. However, the author argues the main interest should be the 2015 order.

2.2. A framework for understanding NN and ISP investment

From an economics perspective, the policy debate on US NN rules has largely focused on the network investment incentive of ISPs under NN – i.e. does NN impact ISP investment in network infrastructure? Critics of NN (led by ISPs and their stakeholders) in the US policy debate argue that the rules caused ISPs to decrease their investment. Proponents of NN (led by CSPs and their stakeholders) in the US policy debate argue that NN rules did not cause ISPs to decrease investment and that they led to increased innovation by CSPs. Both sides have arguably oversimplified the issue by largely ignoring the range of potential outcomes predicted by modeling (for full treatments, see: Choi & Kim, 2010; Krämer et al., 2013; Reggiani & Valletti, 2016).

Krämer et al. (2013) succinctly explain the underlying theoretical element that has been used to propel the NN debate on ISP investment. They note there is a potential incentive for ISPs to generate additional revenue by charging content service providers (CSPs) fees for data/content delivery to customers beyond basic connection access fees. One part of this incentive relates to the need by ISPs to build additional network capacity through infrastructure investment in order to serve increasing data volumes and another part relates to optimizing internet content delivery and revenue. Reggiani and Valletti (2016) note this question reflects the primary argument against NN rules – that ISPs need appropriate remuneration for use of their network infrastructure (by CSPs) to ensure optimal investment levels to continuously improve network capacity.

The relative strength of the investment incentive, however, depends on more than just the presence of NN rules. Theoretical modeling has shown that it depends on market conditions in various segments of the internet. Choi and Kim (2010) discuss a variety of considerations, both short-term and long-term. Under both NN and non-NN (or discriminatory) regimes, there is a potential incentive for ISPs to expand network capacity and both regimes produce capacity expansion that leads to greater revenue for ISPs. Thus, ISPs preference on the presence of NN rules depends on a potential tradeoff between extra revenue generated by data delivery (prioritization) fees from CSPs and end-user access fees. Under NN, capacity expansion increases delivery speeds of content uniformly, producing uniformly higher access fees. Under a non-NN scenario, content delivery speeds increase asymmetrically, which results in differentiated access fees. The authors note that because of the differentiation in the non-NN regime, it is impossible to determine conclusively in modeling which regime produces greater revenue for ISPs. In other words, the investment incentive of ISPs depends on how much CSPs are willing to pay for content prioritization and if that premium fee compensates for potentially lower fees for non-prioritized content. Indeed, because of this ambiguous interplay, Choi and Kim (2010) show in their modeling that network investment incentives for ISPs may even be higher under NN than under a non-NN regime. Specifically, Section 5 of their paper provides a full mathematical treatment of long-term ISP network investment incentive.

A further complication specifically in the case of the 2015 Open Internet Order was the reclassification of ISPs as telecommunications carriers. That change in classification added uncertainty by opening them to potential future price regulation application to telecommunications firms. Such price regulation could have significantly impacted ISPs' ability to produce extra revenue through differentiated access fees and, thus, directly impacted the investment incentive.

Conversely (and briefly), this same type of ambiguity applies to the other side of the NN debate on CSP innovation and content production – another potential driver of ISP network investment. Van Schewick (2007) succinctly notes that NN causes innovation to shift from the core (among ISPs) to the edge (among CSPs). If the increase in innovation and content production by CSPs sufficiently stimulates user demand, it may also sufficiently incentivize and compensate ISPs for additional network investment. Reggiani and Valletti (2016), however, note that NN does not ensure greater amounts of online content while a non-NN regime may also induce both increased content by CSPs and network investment by ISPs.

The modeling work on NN shows a range of equilibria for ISP investment and CSP innovation dependent on prices and revenues. Conveniently, Krämer et al. (2013) even provide a comprehensive framework for the NN policy decision process to account for variations in market conditions (see their Fig. 4 on page 808). Unfortunately, the two sides of the NN policy debate in the US largely left out this interplay of factors, effectively turning the economics portion of the debate into a binary issue – NN rules decreased network investment by ISPs or they didn't. Another factor complicating this issue is timing – there are no clear or accepted guidelines for when network investment would have started to change (if at all).

The unstated assumption in the US NN debate has been that market conditions are sufficient for the ISP investment incentive to be higher under a non-NN regime and lower in a NN regime; thus, the 2015 Open Internet Order (supposedly) distorted the telecommunications markets enough to cause reduced ISP investment in network infrastructure. Some have also extended that argument to the 2010 order. As shown in the next section, empirical studies have generally followed that unstated assumption, which puts the current paper in something of an empirical-theoretical tension.

The current paper also focuses on this investment issue, but must clarify the assumptions of the empirical work that it hopes to build upon and, therefore, the implications of its results. In doing so, the author attempts two things. First, I hope to better link the

theory on NN and network investment than in previous empirical work. This is crucial for understanding the scale of implications of the older and current empirical findings. Second, hopes to address the data and methodological issues of the empirical work to date specifically on the investment issue. This is crucial for moving the empirical debate forward by putting to rest some of the contention specifically on impacts to telecom investment.[3]

However, the author also notes that it is addressing only one empirical aspect of the NN debate, albeit the primary one of previous economics research, and herein lies the tension. I choose to address several of the empirical issues found in the NN literature, but by focusing on them I also leave out the broader theoretical issue of whether or not ISP investment is even the correct empirical question to be asking. The literature up to now has certainly fixated on ISP network investment, but has been flawed. The current paper helps to address some of the flaws in the empirical analyses – an incremental advancement –, but more work should be dedicated going forward to asking new questions.

3. Review of empirical works

3.1. Pre-2017 repeal literature

Theory and hypotheses without empirical observation have formed the foundation of critical arguments on both sides of the NN debate (Brennan, 2017). Greenstein et al. (2016) provide the most up-to-date review on it including the arguments of proponents and opponents. Krämer et al. (2013) introduce the debate for newcomers along with a detailed explanation of the potential positive and negative impacts of NN regimes and non-NN regimes. Additionally, Faulhaber (2011) and Schuett (2010) both offer reviews specifically on the economics of NN. They are all key starting points for any researcher seeking a balanced consideration.

There are a few other general features to highlight about the literature. First, much of the theoretical literature dates well before the 2015 and 2017 rule changes. Second, there is a lack of empirical evidence specifically on telecom investment prior to the 2017 repeal ruling process. Finally, there was only limited additional empirical work added to the literature during and since the 2017 repeal process.

Outside the telecommunications investment debate, there are a range of economic issues related to NN with some supporting research. From a theoretical perspective, Hylton (2017) investigates welfare gains and wealth transfers among internet subindustries. Yoo (2017) analyzes equilibria effects of product/service differentiation. Delp and Mayo (2017) examine theoretical questions around the definitions of market competition. Brennan (2017) examines the NN debate in the run up to the 2015 Open Internet Order and argues that non-economic issues and concerns should have been given more weight prior to the 2015 implementation. Connolly, Lee, and Tan (2017) examine the issue in the context of the urban-rural digital (broadband access) divide. Farrell (2017) looks at fore-markets and after-markets in the NN debate. And Katz (2017) offers a balanced examination on the economic logic of NN rules.

However, the author finds only two works specifically empirically examining the telecommunications investment question prior to the 2017 repeal process. The first is from Nurski (2012), which focuses on the United Kingdom market and which finds evidence that fast lanes increased consumer surplus, industry revenues, and advertising revenues – i.e. that a lack of NN was beneficial. The second is from Hazlett and Wright (2017) and offers a rebuttal against theorized economic benefits of NN, though it is a review and not exclusively concerned with infrastructure investment.

3.2. Literature from and since the 2017 repeal

Moving into the 2017 repeal process, the empirical literature grows. However, the research shows a range of methodological issues. It also remains relatively limited – particularly studies with analysis beyond back-of-the-envelope aggregations – and provides no clear answer on the investment question. Indeed, the research debate arguably become more contested during this period. These critiques apply to work on all sides.

There are a few examples that help clearly exemplify the contested and heated nature of the debate in the 2017 repeal process. The FCC (2017) noted a lack of evidence of purported harms from the 2015 Open Internet Order in its report, “Claims That the Open Internet Order Impaired Investment Lack Any Sound Theoretical or Factual Basis.” While this research document was certainly not speaking for the entire FCC and had no cited author, it demonstrated that even within the FCC there were starkly different opinions on how the Title II ruling impacted telecom investment. In response to the anonymous FCC document, Faulhaber, Singer, and Urschel (2017), in turn, criticized the FCC for its lack of rigorous economic analysis on NN. And finally, for good measure, Winseck and Pooley (2017) criticized Faulhaber et al. for the bias in their analysis. Again, these are merely examples, but they help demonstrate the politicization of the research in conjunction with the differing political viewpoints of the policy decisions. This is key context to keep in mind while discussing the empirical research of this period.

The starting point cutting through the politics are the annual Broadband Capital Expenditure measurements issued by USTelecom (an industry trade body for telecom providers). The data contained in these reports are seen by many as the standard for measuring telecommunication industry investment. They show a decline in U.S. Broadband Provider Capital Expenditure in 2015 and 2016 with

[3] The result is that the author intentionally leaves out two other key components of expected empirical outcomes related to advertising prices and innovation, as already mentioned. (see Choi & Kim, 2010; Krämer et al., 2013; Reggiani & Valletti, 2016). The author focuses on the same simplified empirical question investigated by previous studies, but buttressed by a comprehensive dataset and clearer contextualization of what the paper's results actually show.

a rebounding in 2017 – falling from $78 billion in 2014 to $77.5 billion in 2015, $74.8 billion in 2016, and back up to $76.3 billion in 2017. They are important for three primary reasons. First, they show that aggregate volumes of broadband investment did fall in 2015 and 2016 before rebounding in 2017, which follows the expectations of NN critics that the rules would cause lower telecom investment. Second, the research explicitly states that it does not conduct causal impact analysis – i.e. does not draw conclusions on the impact of NN on industry investment. Third, the research helps to establish the use of *Capital Expenditures* as an appropriate metric for study of telecommunications network investment for the NN debate.[4] (Brogan, 2016, 2017, 2018).

Research finding evidence of harms from NN seems to begin with a blog post analysis from Singer (2017), which finds evidence of a reduction in telecom investment after the 2015 Title II ruling. However, Singer's analysis relies on simple year-on-year 6-month period comparisons and only for a small set of companies. Furthermore, the author's figures show that a single company – AT&T – accounts for 93% of the investment drop and, even then, only after a debatable (though not necessarily incorrect) methodological decision to remove certain capital expenditures. Ford (2017a) examined telecom investment with a difference-in-difference causal impact model and synthetic control for the 2010 Open Internet Order. The author found evidence of a reduction in telecom investment after 2010, but employs a questionable counterfactual approach that fails to account for spillovers by isolating policy jurisdictional scope and which lacks sufficient controls. Ford (2017b) addresses part of that counterfactual issue in a further analysis, but both pieces only examine 2010 while arguing the results directly apply to the 2015 Open Internet Order. The Free State Foundation (2017) has a blog post looking broadband capital investment and finds evidence of a reduction in investment, but it examines only 13 companies and again offers no econometric analysis.

On the other side, the group Free Press (2016) analyzed the impacts the 2015 Open Internet Order and found that network investment, revenues and profits, subscriptions all continued their growth after the 2015 Title II ruling. The organization also provides a report (Free Press, 2017) using SEC filings showing that ISP capital expenditures among publicly-traded ISPs were, in fact, up by 5.3% in the two-year period following the ruling compared to the two-year period preceding the ruling. Their analyses suffer from the same issues as Singer (2017) and Free State Foundation (2017) – they are arguably more thorough, but fail to conduct statistical analysis and make some questionable (though again not necessarily incorrect) methodological decisions around the inclusion of certain capital expenditures. Hooton (2017) attempts a comprehensive set of analyses to test whether either the 2010 or 2015 Open Internet Orders had causal impacts on telecom investment and related metrics. The paper uses multiple data sources and techniques, finding no statistical evidence of investment decline and circumstantial evidence of sustained investment post-2015. This work is notable in its systematic approach to the issue, but there are clear (and noted by the author) issues with the quality of some data employed. Crandall (2017) analyzed stock market reactions to the 2015 rules and found negative impacts on equity in just three telecom companies with those impacts being short-lived. The author also found no impacts on media companies. Finally, as mentioned, the FCC (2017) conducted a review of studies and data on telecom infrastructure investment and found the claims of decreased investment as a result of NN to be unsubstantiated – both theoretically and in the data.

The author's overall conclusion from the current literature is that there is a limited number of empirical analyses, each defined by flaws to some degree. This observation is not intended as all-encompassing critique of these previous works – indeed they each have added value to the discussion and the authors conducted them under less than ideal circumstances. Rather, it is an observation on the current state of the debate in the research literature intended to guide new research, including this paper.

These previous works also, in a way, attest to two key empirical issues. First, they show the difficulty of testing the causal impact of a single national policy change on an industry's decision-making. While this is not an impossible endeavor, the lack of quality data and the insufficient passing of time following the 2015 regulatory actions (that were being examined in the 2017 regulatory debate) hamper the previous analyses. Indeed, many of the authors on both sides explicitly recognize this challenge, but fail to actually address it. For example, Ford (2017a) exclaims numerous factors that influence telecom investment decisions, but then proceeds to immediately ignore the challenge while Hooton (2017) only illustrates (rather than addresses) the inability to properly account for confounding factors given the data and techniques available. Second, the previous empirical literature also largely ignores one half of the debate – the issue of innovation and the various equilibria that may arise based on the equilibrium between innovation and investment – and focuses instead almost exclusively on ISP investment. While the current paper follows this precedent to specifically address the empirical shortcomings in data and observation, the author emphasizes the perspective is only partial. Further research specifically on net neutrality's impacts on innovation – both among edge and ISP companies – would be a valuable contribution going forward.

Given the rapid timeframe for developing and filing during the 2017 repeal process (a key motivator for much of the cited empirical work), it is unsurprising that questions remain. The author hopes the current paper can help address at least some of those remaining questions, specifically around the investment reaction of ISPs, with the benefits of a longer timeframe and fuller dataset.

4. Methodology

4.1. Data

The author employs quarterly data from the SEC's “Financial Statement Data Sets” for every quarter from Q1 2009 through Q3 2018. The data provide information from financial statements for every company reporting them to the SEC (SEC, n.d.). There are 8577 unique companies with over 24 million observations over the time period of analysis.

[4] See Section 4.5 for further discussion of this.

Within the data, the author tracks figures for *Capital Expenditures Incurred But Not Yet Paid* for each company as the measure for telecommunications investment, its dependent variable. The author chose this particular metric since it measures new investment obligations assumed in the current period rather than actualized, previous obligations captured by capital expenditures paid. This paper's metric offers a previously unexamined and more accurate method for tracking reactionary investment decisions to NN rule changes than current capital expenditures. There are 8913 unique observations across all firms and time periods for capital expenditures incurred with 270 specifically for telecommunications firms.

A key element of the NN investment debate has been how ISPs reacted to the rule changes when they happened. The majority of the empirical studies examining this issue used traditional capital expenditures (paid) as the proxy for those investment decisions (see Section 3.2); however, capital expenditures are often planned (and incurred on balance sheets) on a multi-year basis with several years of lead time before the actual expenditure occurs (KPMG, 2015). This implies that at least some and perhaps much of the recorded capital expenditures examined in the years following the NN rule changes actually reflected decisions made years before and independent of the rule changes. It also implies the decisions may have been made before any expectation of rule changes. The current paper's metric, however, reflects the actual balance sheet incursion dates, meaning the author is able to specifically isolate *ex post* changes in capital expenditure investment decisions. Additionally, since the data provide exact dates, the paper is able to map them on a more precise timeline that follows specific regulatory actions rather than simply lumping them together into annual totals. These two aspects provide key improvements to the use of annual volumes while maintaining conceptual consistency with previous studies since the author continues to use a version of capital expenditures as the proxy for ISP network investment.

The use of capital expenditures (in general) as a proxy for telecommunication industry investment follows accepted practice in the NN debate. Authors arguing both for and against NN rules have employed capital expenditures in empirical studies specifically on telecommunication *network* investment by (see Brogan, 2016 and Free Press, 2016 as just two examples). The author notes the variable *Capital Expenditures Incurred But Not Yet Paid* is a measure of actual capital expenditures and not some other variable. The key difference between the current paper's dependent variable and other studies that have used *capital expenditures* relates to timing – the incursion of new capital expenditure obligations (the paper's variable) versus the payment of existing capital expenditure obligations (the capital expenditure variable generally). The author notes that the metric of capital expenditures cannot distinguish industry investment specifically in network capacity expansion, which is the particular element of interest in the NN debate (as discussed in Section 2).

Thus, the current paper examines industry investment generally with the assumption that capital expenditures serve as an appropriate metric for network investment. This assumption limits the overall interpretation of the results, which the paper discusses more fully in Section 4.3, but does not negate the contribution of the research. I justify the assumption in two ways. First, the use of capital expenditures follows the wide precedent already documented in Section 3 – both sides of the NN have relied on the metric as a proxy specifically for telecom industry network investment (see Free Press, 2017 and Singer, 2017 as two examples). Second, broader academic research on accounting and industry investment practices (across the private sector and not just among ISPs) has demonstrated the appropriateness of using capital expenditure as an investment proxy. Biddle, Hilary, and Verdi (2009) demonstrate industry investment as the sum of capital expenditures and specific non-capital expenditures, principally Research & Development expenditures and acquisitions.[5] Akdoğu and MacKay (2008) help demonstrate the use of capital expenditures specifically in the analysis of corporate investment patterns over time and broken out by subgroup – similar to the analysis needed for the current paper. Ambrose and Steiner (2019) demonstrate the applicability of changes in capital expenditure volumes specifically as a proxy for current, market-based investment choices by firms.

The author also collected a set of explanatory and dummy control variables to model telecom industry investment levels. This included the following explanatory variables: Quarterly GDP Percent Change, Ten-Year Bond Rate, Interest Rate (discount rate), Filer Status, and Industry (SIC code). The author included the following control variables: Year FE and Quarter FE. The author collected all variables with quarterly observations for the full time period of observation from Q1 2009 through Q3 2018 from the SEC, Bureau of Economic Analysis (BEA, n.d.), and the Federal Reserve (n.d.). Table 1 lists the full set of variables used in the dataset including an overview of the rationale for their inclusion and other details.

4.2. Empirical strategy

The author investigates the causal impacts of the 2015 Open Internet Order on telecommunication investment. It also repeats that analysis for the 2010 Open Internet Order for thoroughness. It runs two separate analyses – one for each of the orders.[6]

The author follows the same hypothesis of the previous empirical literature conducted during the 2017 repeal process and policy debate:

H1. The implementation of the 2015 (2010) Open Internet Order caused the telecommunications industry to decrease network infrastructure investment.

H0. The implementation of the 2015 (2010) Open Internet Order did not the telecommunications industry to decrease network

[5] The inclusion of acquisition would lend weight to the argument that Singer's (2017) removal of certain acquisition costs from AT&T's investment totals was inappropriate.

[6] Please note that each econometric test has the same set up and model, but the trigger dates vary to ensure only one order is tested at a time. The paper reports the results for both tests in the same table (see Table 4) for efficiency only.

Table 1
Variables description.

Variable	Type	Source	Purpose
Capital Expenditures Incurred But Not Yet Paid	Dependent	SEC	Measure for new telecom industry network investment; measures volume of new obligations assumed in capital accounts to upgrade physical assets (i.e. investment) in current period; capital costs are not expensed in period where they are incurred, but expensed over a period time
Quarterly GDP Percent Change	Independent	BEA	Measure for overall US economic performance, a key determinant of company financial decisions
Ten-Year Bond Rate	Independent	Federal Reserve	Represents cost of medium-term financing typical for infrastructure investment
Interest Rate (discount rate)	Independent	Federal Reserve	Represents cost of short-term financing charged by the Federal Reserve and used in discounted cash flow analysis
Filer Status	Independent	SEC	Accounts for the size and value of individual firms (e.g. large versus small firms) at the time of filing
Industry FE	Independent	SEC	Accounts for differences between major industries, such as different business cycles, using SIC codes of individual firms
Year FE	Control	Based on data from SEC	Controls for any year-to-year variation in dependent variable not attributable to independent variables
Quarter FE	Control	Based on data from SEC	Controls for any quarter-to-quarter variation in dependent variable not attributable to independent variables
Telecom Industry Dummy	Interaction variables for difference-in-difference model	NA	Binary indicator for telecom industry firms
2015 Policy Period Dummy		NA	Binary indicator for time periods that are after 2015 rule changes and before 2017 rule changes
2010 Policy Period Dummy		NA	Binary indicator for time periods that are after 2010 rule changes and before 2017 rule changes
2015 Treatment Variable		NA	Binary indicator for observations that are after 2015 rule changes and before 2017 rule changes
2010 Treatment Variable		NA	Binary indicator for observations that are after 2010 rule changes and before 2017 rule changes

infrastructure investment.

The author employs a difference-in-difference model using a full set of controls and examines two treatment scenarios – a 2010 trigger date and 2015 trigger date –, while also accounting for the 2014 court vacating of the 2010 order and the 2017 repeal of the order through the removal of the treatment effect in the appropriate periods. The methodology follows an accepted approach utilized by both sides of the debate up to now and is arguably the primary method of the current empirical literature set. And while the author applies the same method to both policy actions, it is important to recognize it does not imply the policy actions are the same or that they would have the same impacts. The chosen model and counterfactual strategy allows either policy to have stronger or weaker effects and I test each policy separately. In other words, while the empirical set up is similar, it does not imply that the paper treats them the same way. Finally, while previous studies lacked sufficient periods of observation and data to properly employ the technique and draw causal impact conclusions, the author argues the current paper largely addresses the issues that plagued previous studies.

The author identifies telecommunications firms using formal guidance from the Bureau of Labor Statistics (BLS) and their definition of the "Telecommunications subsector" (BLS, n.d.). The BLS provide a set of North American Industrial Classification System (NAICS) codes that comprise the telecommunications subsector. The author converted these NAICS codes to Standard Industrial Classification (SIC) codes using government crosswalks since the SEC dataset categorized firms by SIC code. Based on that process, the author used the following SIC codes to identify telecommunications firms: 4812 *Radiotelephone Communications*, 4813 *Telephone Communications except radio*, 4822 *Telegraph and Other Message Communications*, 4841 *Cable and Other Pay Television Services*, 4899 *Communications Services NEC*, and 7375 *Information Retrieval Services*. Each firm with one of the telecommunications SIC codes received a dummy indicator of 1 while all other firms received a 0, separating out the dataset into a treatment group (telecommunication firms) and non-treatment group (all other firms).

The author assigned treatment period dummies using the specific days (dates) that firms incurred new capital expenditures and the specific regulatory effective dates for the 2010, 2014, 2015, and 2017 NN actions. The data provide the specific *Balance Sheet Date* of each record, which allows the author to categorize each individual observation into treatment and non-treatment periods down to the exact day (date) while still controlling for seasonal and cyclical trends in fiscal periods (e.g. Q1) and years. The author uses the dates shown in Charts 1–4: December 21, 2010 for the 2010 Open Internet Order; January 14, 2014 for the vacating of the 2010 Open Internet Order by the DC Circuit Court; April 13, 2015 for the 2015 Open Internet Order; and, December 17, 2017 for the repeal of the 2015 Open Internet Order. So, in the 2010 analysis scenario, the author counted any new capital expenditure incurred on or after December 21, 2010 and before January 14, 2014 as part of the treatment period. In the 2015 scenario, the author counted any new capital expenditure incurred on or after April 13, 2015 and before December 17, 2017 as part of the treatment period. The author discusses the implications, advantages, and drawbacks of the selected treatment dates in detail in Section 4.5.

The author notes that the 2017 repeal date represents the moment of the FCC vote to repeal while the actual rule changes

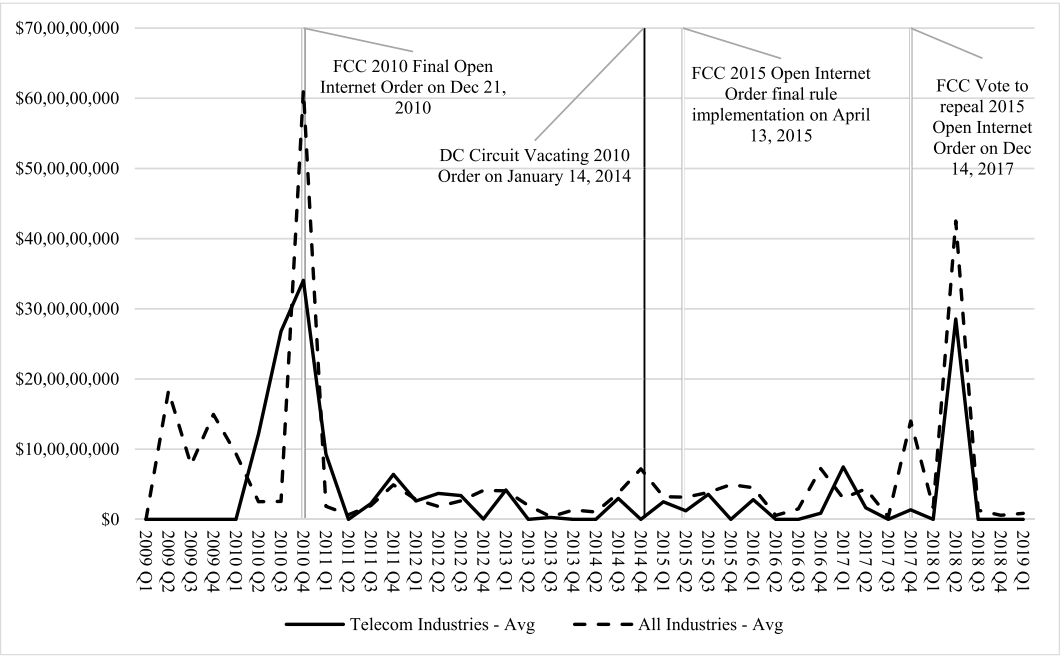


Chart 1. New capital expenditures incurred - average.
Source: Author's elaboration; data from SEC

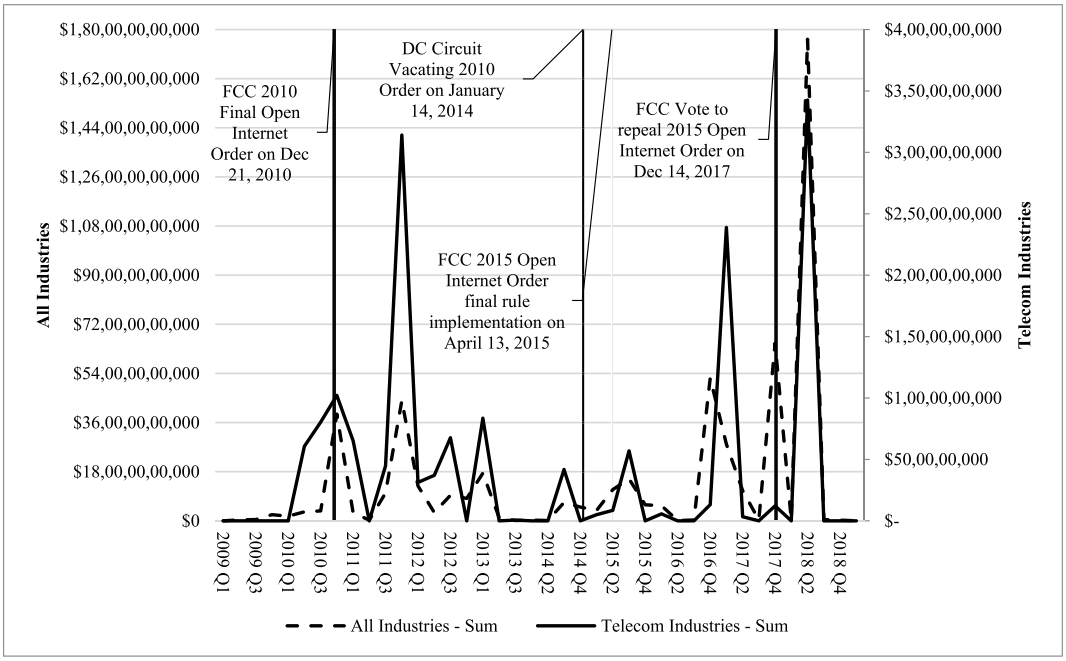


Chart 2. New capital expenditure incurred - total.
Source: Author's elaboration; data from SEC

occurred on June 11, 2018. The author chooses the vote date since the impending repeal became official at that point and companies could then incorporate it into future investment decisions. Furthermore, the paper also reran all of its analysis using June 11, 2018 as the repeal date since it had observations from after that date. This alternate scenario produced no changes to the statistical outcomes or overall conclusions and included only very minimal changes to coefficient size estimates. The paper argues they add nothing to the analysis and chooses not to include them here to ensure a reasonable length for the paper.

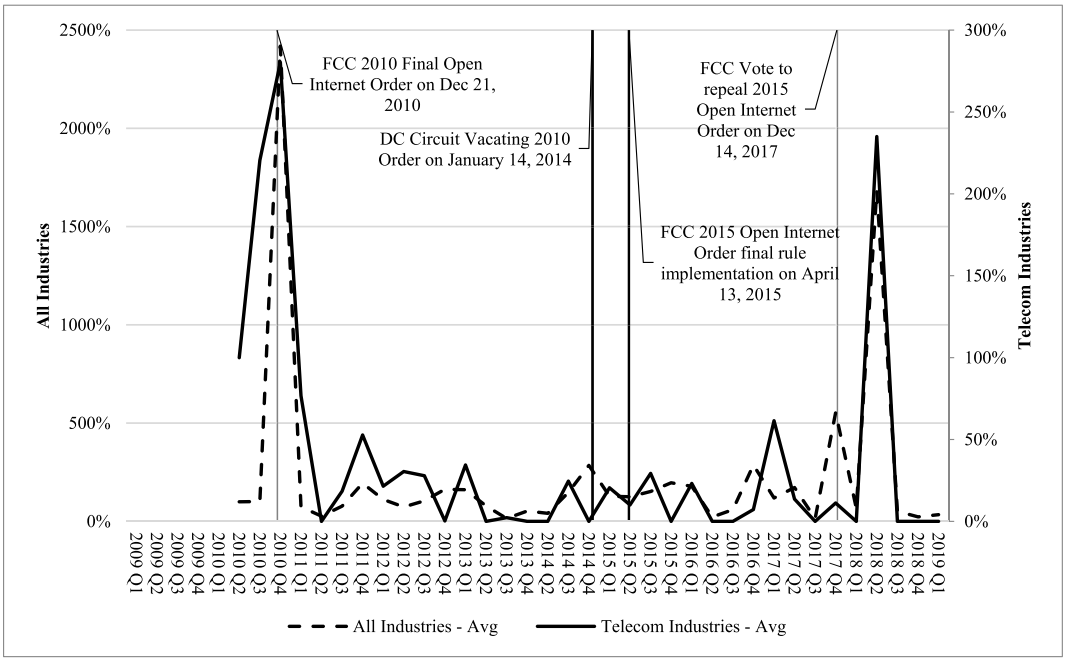


Chart 3. New capital expenditure correlation - telecom and all industries - average.
Source: Author's elaboration; data from SEC

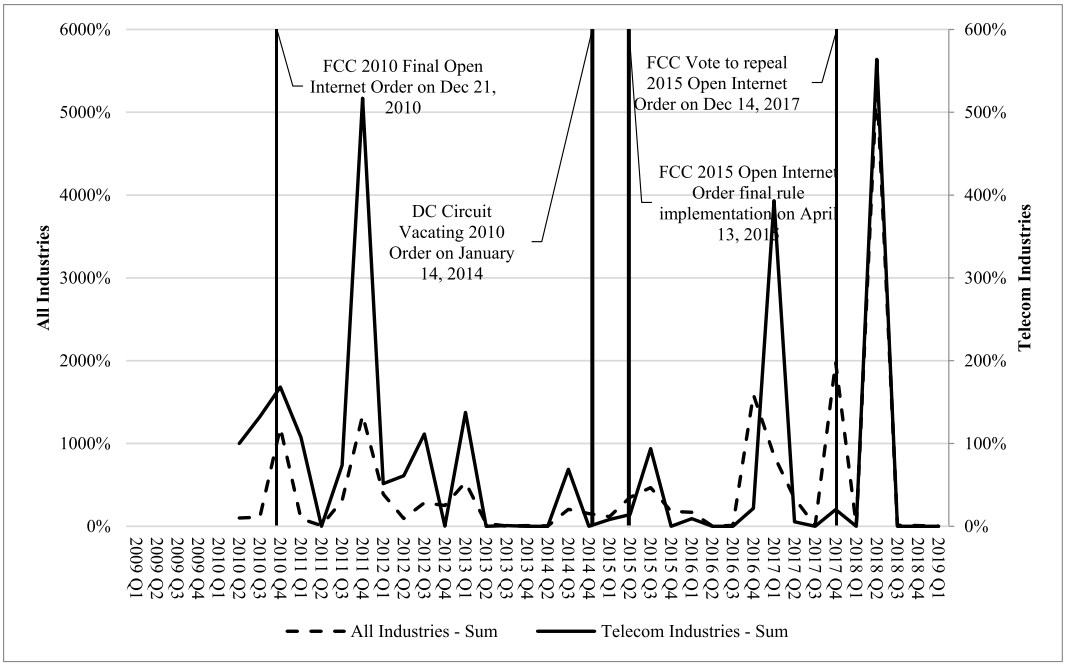


Chart 4. New capital expenditure correlation - telecom and all industries - sum.
Source: Author's elaboration; data from SEC

4.3. Counterfactual

The author runs its difference-in-difference model for two scenarios. First, the paper examines the impacts of the 2015 Open Internet Order, which was the primary subject of debate in the empirical works issued during the 2017 repeal process by the FCC. Second, it examines the impact of the 2010 Open Internet Order, which several researchers have argued as a key implementation date for NN rules and, consequently, telecom industry investment reactions.

The author's analysis is primarily interested in how new capital expenditure among ISPs before and after the treatment period in the two analysis scenarios. The use of the difference-in-difference model allows for causal effect analysis that provides estimates for the direct effect of the policy changes on telecom investment. To estimate these causal impacts, the author constructs a counterfactual that represents the "but for" scenario – i.e. the author uses a control group of non-telecom firms to examine how telecom investment changed as a result of NN while controlling for key factors.

Both analyses (for the 2010 order and 2015 order) have the same essential set up, but each event is treated individually as unique (i.e. the author does not necessarily expect the same outcome from both events). The author measures its dependent variable from Q1 2009 through Q3 2018. I then assign a start date (treatment) and an end date for the policy period – December 21, 2010 through January 14, 2014 for the 2010 Open Internet Order and April 13, 2015 through December 17, 2017 for the 2015 Open Internet Order. I then compare how the dependent variable changed from before the treatment start to after the treatment start for my treatment group (all ISPs). Next, I compare how the variable changed the for the control group. I then compare the changes for the treatment group with those for the control group. Appendix A provides stylized example chart illustrating the approach.[7]

The author began with a series of correlational analyses tracking *Capital Expenditures Incurred But Not Yet Paid* for each industry (SIC code) to identify its control group and counterfactual strategy. The author chose to employ the average across all non-telecommunications firms as its control group.

The principal reason for this was the high degree of correlation between the control and treatment group in pre-treatment trends. The pairwise correlation coefficient for new capital expenditures incurred between the paper's treatment and control was 0.7184 (using averages) and 0.7214 (using sums). Only three individual subindustries demonstrated higher correlations with telecom firms than the average across all firms. These were 3334 *Primary Production Of Aluminum*, 3576 *Computer Communications Equipment*, and 5190 *Wholesale-Miscellaneous Nondurable Goods*. However, these three industries suffered from a lack of observations in certain quarters, which made them less suitable than the paper's chosen approach.

The second reason for the selection was to minimize potential spillover effects given the use of only US industries, which prevent the author from isolating jurisdictional scope in the analysis. Every industry in the dataset is in the United States and, therefore, subject to the legal jurisdiction of the NN policy changes. While the FCC rules do not target every industry, it is impossible to completely isolate potential downstream effects to related industries or interconnected supply chains (e.g. the rubber industry, which may supply coatings for telecom wires).[8] By using the average across all non-telecommunications firms, the author can mitigate spillovers to any unintentionally impacted industry/firm while also mitigating any industry/firm with outlier levels of capital expenditures.

The third reason for the counterfactual strategy is completeness. Being unable to isolate jurisdictional scope, it is preferable (compared to alternate approaches such as a synthetic counterfactual) to ensure that all other industry trends are accounted for through the use of all data for all industries, assuming the mean and sum counterfactuals represent good fits to the treatment group in pre-treatment years. In this case they do. The author argues the approach is robust and follows guidance on temporal counterfactuals from others such as Galster, Temkin, Walker, and Sawyer (2004).

Charts 1–4 provide time series charts for the paper's treatment group and control group, demonstrating their correlation and the general tracking of telecom investment to that of the broader business community. Tables 2 and 3 provide a set of descriptive statistics showing differences in the paper's dependent variable for ISPs (treatment group), non-ISPs (control group), and all firms (Table 2) and for each year (Table 3).

4.4. Model

The paper uses a standard difference-in-difference model to assess the causal impact of NN rules on telecommunications industry investment. The applied model is defined as:

$$Y_{it} = \beta_0 + \beta_1 T_{it} + u_z + FY_t + FP_t + X_{it} + \varepsilon_{it} \quad (1)$$

where Y_{it} is the observed level of the outcome variable for unit (firm) i at time (day) t and T is a dummy treatment indicator, which takes the value 1 for treatment on a particular day and 0 otherwise. The coefficient β_1 provides an estimate of the impact of NN rule changes in the telecommunications industry.[9] The term u_z is a full set of group fixed effects for each industry, z and FY_t is a full set of year fixed effects while FP_t is a full set of quarter fixed effects. The term X_{it} is a vector of variables for filer status, city, recession periods, GDP change, ten-year bond rate, interest rate (discount rate).

The author runs three specifications of its model each for a 2015 Open Internet Order treatment and a 2010 Open Internet Order treatment, producing six specifications in total. The first specification is a baseline without any control terms. The second specification includes only firm-level controls derived from the SEC dataset (see Appendix B). The results of the first two are included only for informational purposes to illustrate the improvement in model fit. The author attaches these specifications as Appendix B. The third and preferred specification includes all controls. The author presents the third specification for the 2015 and 2010 orders in Table 4 in the next section. The author produces the tables using *stargazer* (Hlavac, 2018) in R.

[7] See Jakiela and Ozier (n.d.) for more on the difference-in-difference technique.

[8] The only way to do this in the case of NN is to use another country(ies) in the counterfactual strategy.

[9] In its functional form the treatment variable T is a full set of interaction variables defined as $A_{izt} \times T_{izt}$ with A equal to 1 in those years when treatment occurs and T equal to 1 if the unit belongs to an analysis group that will at some point receive treatment.

Table 2
Descriptive statistics for Capital Expenditures Incurred, all periods.

	n	Mean	SD	Median
ISPs	270	$ 61,491,244	$ 197,672,719	$ 6,618,500
Non-ISPs	8643	$ 64,865,670	$ 1,048,310,000	$ 1,816,500
All	8913	$ 48,462,892	$ 796,148,654	$ 1,936,000

Table 3
Descriptive statistics for Capital Expenditures Incurred, all firms by year.

	n	Mean	SD	Median	Skewness	Kurtosis
2009	25	$ 57,140	$ 61,755	$ 45,249	1.8892	−2.2533
2010	365	$ 175,495	$ 127,112	$ 141,881	1.6985	2.2396
2011	1721	$ 1,497,684	$ 548,516	$ 1,508,339	−0.0027	1.9941
2012	1308	$ 2,906,334	$ 857,759	$ 2,823,390	0.5838	0.4680
2013	598	$ 4,516,821	$ 310,450	$ 4,520,892	−1.9337	−0.0888
2014	278	$ 5,371,936	$ 290,796	$ 5,278,080	2.2906	6.7800
2015	1055	$ 6,217,999	$ 383,913	$ 6,239,175	2.8859	4.3558
2016	974	$ 8,500,799	$ 698,251	$ 8,712,502	−1.3342	16.9400
2017	1801	$ 9,629,193	$ 678,037	$ 9,342,316	0.9356	1.2703
2018	528	$ 11,754,110	$ 195,748	$ 11,818,006	−3.0379	−0.4092

Table 4
Effects of 2010 and 2015 open internet orders on ISP capital expenditures.

	New Capital Expenditures Incurred Per Quarter	
	(1)	(2)
Intercept	1,897,667.000	−53,574,984.000
	(280,084,094.000)	(280,390,973.000)
Telecom Industry Dummy	8,651,678.000	14,152,924.000
	(192,864,715.000)	(192,370,918.000)
2015 Policy Period Dummy	−13,172,086.000	
	(36,971,907.000)	
2010 Policy Period Dummy		126,849,992.000***
		(48,639,636.000)
GDP Qtr Change	−443,163.900	699,371.900
	(3,869,426.000)	(3,893,823.000)
Ten-Year Bond Rate	3,907,769.000	9,109,251.000
	(23,515,240.000)	(23,047,389.000)
Interest Rate (discount rate)	4,993,926.000	66,331,830.000
	(337,225,651.000)	(335,259,717.000)
2015 Treatment Variable	−4,358,449.000	
	(47,922,877.000)	
2010 Treatment Variable		−15,620,009.000
		(46,088,942.000)
Filer Status Control	Yes	Yes
Industry Control	Yes	Yes
Year and Quarter Control	Yes	Yes
Observations	7223	7223
R^2	0.707	0.707
Adjusted R^2	0.693	0.693
Residual Std. Error (df = 6903)	317,880,119.000	317,724,574.000
F Statistic (df = 319; 6903)	52.092***	52.164***

Note: *p < 0.1; **p < 0.05; ***p < 0.01.
SEC Financial State Data Sets readme files for more information on filer status.

4.5. Limitations of the analysis

The current paper analyzes how NN policy decisions affected telecommunication industry investment with the key advantages of a fuller dataset and sufficient time post-policy decision. The empirical contributions of the current paper, however, do not mean that the current study is without limitations. Specifically, there are three issues that limit the implications of the paper and that further research should work to address.

First, the paper is unable to specifically isolate telecom network investment (designed to boost network capacity) and must instead utilize the broader metric of capital expenditure, following the precedent of previous studies. The paper improves upon

previous studies that have used capital expenditures as a proxy for network investment by examining new capital expenditures incurred rather than current expenditures – again, this more accurately reflects new investment decisions. The paper also argues that capital expenditure is an appropriate proxy for network investment given that the indicator tracks investment in physical assets. Network assets fall under capital expenditures in company financials and the author argues network investment likely makes up the majority of capital expenditures for ISP firms. However, the fact remains that the paper is unable to remove other capital assets unrelated to the NN policy debate in its analysis and, thus, relies on assumption. Future research with data that can specifically separate network capacity investment by ISPs from other capital investment would be a welcome addition.

Second, the paper recognizes that the dates used to denote the treatment periods in its analyses follow specific regulatory actions, but may miss firms accounting for those actions ahead of time. The dates are the specific regulatory action dates and they mark the 'official' NN periods. However, the paper has noted that capital investment works on medium-term cycles with decisions often made several years in advance of actual expenditure. There is a clear argument that ISP firms may have accounted for expected NN rule changes well before the actual regulatory action dates. This implies that new investment incurred after the regulatory action dates do not fully reflect the impacts of NN on investment. The paper recognizes the issue and notes that its analyses may not capture the full causal impact effect of NN.

However, the paper argues two things as justification for its approach that address the issue. First, any adjustment in the analysis to account for leading or lagging investment effects would have *itself* been arbitrary. There is no guidance on how early or late firms may have made investment decisions related to expected NN rules and each firm would have made those decisions individually. So, any adjustment in the analysis would have merely guessed at an appropriate lead or lag time period and inappropriately applied that same period to all firms. Second, standard adjustments to address this issue, such as the use of a 1-year or 1-quarter lag of the dependent variable, are not appropriate for the current paper given the precision of the investment data down to specific dates. The lag procedure helps account for ambiguity in data where the highest level of time precision is a period (e.g. year), but the paper's data has no such ambiguity. Therefore, the author argues the use of specific regulatory action dates represent the best approach. The author also notes it accounts for time variation through the use of year and quarter fixed effects.

The author's primary strategy for selecting the treatment dates has the advantage of relying on public and official policy actions. This allows for reproducibility and provides a clear logic to the identification of the dates. Its disadvantage is its assumption that these dates represent actual 'cutoff' points for ISP investment decisions – a logical, but limiting assumption. Beyond the applied model terms added to help address this disadvantage, I also rerun the primary specification both a 6-month lag and 6-month lead for both the 2010 and 2015 policy actions. The author disagrees with utilizing a lag or lead as the primary approach given the precision of the dataset, but the additional tests help confirm the validity of the primary approach.

The third limitation of the paper is that it examines only one component of the NN debate – the impact of NN rules on telecom investment. This was the most prominent economics issue in the 2017 repeal process, but it leaves aside the issue of CSP innovation that may be promoted by NN rules. As noted by Reggiani and Valletti (2016), a key argument of NN advocates is the need to promote innovation among smaller content or edge firms – something that NN rules theoretically help to do. The author noted in its literature review that this issue has been largely unaddressed in the literature – only Hooton (2017) offered a brief descriptive analysis of innovation among ISPs (the other side of the issue). The primary reasons for the author's exclusion of the innovation issue are: 1) a lack of definitional guidance on what constitutes innovation and, thus, data to measure it; 2) the need to maintain a reasonable scope and length for a single journal article; and, 3) the need to maintain empirical consistency with previous empirical works since one goal of the author is to improve the empirical literature through better data and time.

Finally, while not a limitation *per se*, the author notes that the analyses did not include a term to differentiate treatment intensity. In other words, both tests used a standard 0, 1 binary interaction variable triggered in a standard difference-in-difference test. This masks the policy differences between the 2010 and 2015 Open Internet Orders (see Section 2.1), but it follows standard practice for this technique. The use of the same model framework for both orders does not imply the orders had the same impacts or expectations of impacts (though many have argued as much) and the author indeed tested them separately from each other, allowing different for different treatment impact effects. Again, I argue the 2015 order is the primary policy action of interest and I include the 2010 test only for thoroughness since previous literature has done so.

5. Results

5.1. Primary findings

The author presents the paper's main results in Table 4. The variables of interest are labeled: *2015 Treatment Variable* and *2010 Treatment Variable*. These provide the key metric on the evidence on the causal impacts of NN on telecommunications investment in the US (for the 2015 order and 2010 order respectively). Both are *not significant*, indicating *no causal impact*.

The author also notes that the lack of causal impact shows in multiple robustness tests later in the paper. Far from being beholden to a single regulatory action related to the interpretation of a single legal clause, the author argues the telecommunications industry (and indeed any major industry) likely has numerous long-term and short-term factors feeding into investment decisions. The NN rules appear to have had no impact on telecom investment, at least not one that can be detected in data.

The coefficient sizes of the two interaction terms also illustrate this point. While the treatment coefficients were not significant, their values are rather small as a percent of telecommunications investment. The values were approximately -$4.4 million for the 2015 treatment test and -$15 million for the 2010 treatment test. These represent approximately 0.1% and 0.6% of peak firm investment for any particular quarter in their respective post treatment periods.

Table 5
Correlation matrix of non-factor control terms.

	GDP Quarter change	Ten-Year Bond Rate	Interest Rate
GDP Quarter change	1.0000	−0.0491	−0.1368
Ten-Year Bond Rate	−0.0491	1.0000	0.2410
Interest Rate	−0.1368	0.2410	1.0000

5.2. Further checks

The author presents it's the baseline models for comparison in Appendix B. It also runs a set of additional analyses to further confirm the results from its primary specifications in Table 4.

First, the author tested for multicollinearity in its model terms, finding evidence of multicollinearity produced by the *industry fixed effects* variable used in its model. This created a methodological dilemma. It is standard practice to control for the specific industries examined in this type of analysis and the presence of collinearity from a factor variable (fixed effects) does not inherently mean it is without value. However, it indicates that additional checks must be conducted to ensure validity in the results. It also clearly demonstrates that previous research, which used industry fixed effects and which claimed robust results in their test may need to revisit their analysis – it is unlikely that multicollinearity from industry fixed effects would only affect the current analysis. Second, the author also found *Interest Rates* to have high gross variance inflation factor (GVIF) scores with some of the factor variable controls.

To address these issues, the author first confirmed the lack of correlation among its other non-factor control and explanatory variables. Table 5 presents the correlation matrix of these terms and shows no pairwise correlation above 0.3.

Next, the author examined it's the VIF and GVIF scores of its full set of control terms (see Table 6) except for the industry fixed effects term. Systematic testing of control and explanatory term combinations confirmed the high GVIF score for the Interest Rates variable as well as low GVIF scores (below the standard threshold of 5.0) for all other variables.

These first two simple diagnostic steps confirm that the industry fixed effects variable and the interest rates variable are the causes of multicollinearity issues. To confirm if either or both terms were indeed distorting results, the author next ran four new regression specifications for the 2015 treatment that systematically inserted and removed the two terms to analyze their impacts on overall model fit and results.

The author presents these new regression results in Table 7. In each of these specifications, the author once again finds *no causal impact* from NN – the treatment terms all remain statistically insignificant and the coefficient sizes remain fractions of total industry investment levels.

Specification 1 includes all control terms except for industry fixed effects and produces an adjusted r-squared of 0.0004. The coefficient size is approximately -$7.5 million. Specification 2 uses all control terms except for interest rate and the adjusted r-squared value is 0.254. Specification 3 removes the industry fixed effects term and interest rate term, but leaves all other control variables. The adjusted r-squared produced is 0.043. Finally, the author runs its regression keeping *only* the industry fixed effects and interest rate variables. This produces a strong adjusted r-square of 0.693 and negative coefficient on the treatment term.

These new tests demonstrate a couple things. First, the lack of statistically significant impact effects remains across the systematic regression variations. Second, they demonstrate that interest rates and simple industry controls account for more investment level variation than NN rule changes. This provides additional support to the observation that the NN rule changes did not drive telecommunications investment levels.

Next, the author addresses the asymmetric distribution of firm sizes among ISPs through two additional robustness tests.

In the first test, I rerun my primary specification (in Table 4) with a logged dependent variable. This test helps to determine if outliers are affecting the results and influencing the treatment variables. By logging the dependent variable, outliers (i.e. those with extremely large capital expenditures) become less extreme. This reduces the overall model fit (r-squared), but can produce better results for individual terms. Table 8 presents the results and show the continued lack of statistical significance for both analyses.

In the second test, I specifically address the argument made by some researchers that the large size of some ISPs (AT&T in particular) merit special consideration or, at least, separate analytical treatment. The argument has merit and there is potential for the current paper's primary analysis to mask the impact on these larger firms. By running a standard difference-in-difference model

Table 6
Variance inflation factors of control terms.

	Full Control Set			Full Control Set Except Interest Rate		
	GVIF	Df	GVIF^(1/(2*Df))	GVIF	Df	GVIF^(1/(2*Df))
Filer Status Control	1.1910	4	1.0221	1.1592	4	1.0186
Year FE	33364.4672	8	1.9174	79.8013	9	1.2755
Quarter FE	1540.0520	3	1.1301	1509.6108	3	1.1297
GDP Quarter change	2.3586	30	1.5358	2.3205	30	1.5233
Ten-Year Bond Rate	5.4271	1	2.3296	4.6828	1	2.1640
Interest Rate	3166.0000	1	17.7764			

Table 7
Robustness checks - effects of 2015 open internet orders on ISP capital expenditures.

	New Capital Expenditures Incurred Per Quarter			
	(1)	(2)	(3)	(4)
Intercept	328,300,958.000	280,544,460.000	783,584,057.000*	−27,388,452.000
	(346,536,144.000)	(688,575,962.000)	(463,394,409.000)	(184,096,472.000)
Telecom Industry Dummy	19,150,418.000	355,938,685.000	18,197,318.000	40,258,089.000
	(45,170,002.000)	(545,639,956.000)	(77,434,091.000)	(189,307,428.000)
2015 Policy Period Dummy	18,272,424.000	−65,295,787.000	−77,811,116.000	29,833,057.000**
	(63,115,315.000)	(51,441,812.000)	(55,632,668.000)	(11,695,974.000)
GDP Qtr Change	−7,937,472.000	−8,442,284.000	−7,691,051.000	
	(6,541,662.000)	(10,366,231.000)	(11,124,056.000)	
Ten-Year Bond Rate	−32,939,857.000	3,329,961.000	−20,243,118.000	
	(39,964,539.000)	(56,153,228.000)	(60,718,174.000)	
Interest Rate (discount rate)	−308,028,801.000			251,193.200
	(569,646,163.000)			(26,477,612.000)
2015 Treatment Variable	−7,546,955.000	−15,668,370.000	−18,594,149.000	−10,052,392.000
	(82,166,138.000)	(129,893,939.000)	(139,911,615.000)	(47,794,656.000)
Filer Status Control	Yes	Yes	Yes	No
Year and Quarter Control	Yes	Yes	Yes	No
Industry Control	No	Yes	No	Yes
Observations	7223	8515	8515	7223
R^2	0.007	0.282	0.049	0.705
Adjusted R^2	0.0004	0.254	0.043	0.693
Residual Std. Error	573,537,416.000 (df = 7171)	906,835,115.000 (df = 8192)	1,027,162,557.000 (df = 8463)	317,872,495.000 (df = 6950)
F Statistic	1.061 (df = 51; 7171)	10.005*** (df = 322; 8192)	8.493*** (df = 51; 8463)	60.924*** (df = 272; 6950)

Note: *p < 0.1; **p < 0.05; ***p < 0.01.

Table 8
Robustness checks - effects of 2015 open internet orders on ISP capital expenditures – logged dependent variable.

	New Capital Expenditures Incurred Per Quarter	
	(1)	(2)
Intercept	9.721*	9.163*
	(5.378)	(5.375)
Telecom Industry Dummy	0.831	1.074
	(2.970)	(2.956)
2015 Policy Period Dummy	1.247**	
	(0.596)	
2010 Policy Period Dummy	0.536	(0.912)
GDP Qtr Change	0.364**	0.330*
	(0.184)	(0.184)
Ten-Year Bond Rate	−0.402	0.163
	(1.030)	(1.006)
Interest Rate (discount rate)	−4.057	−4.506
	(4.894)	(4.915)
2015 Treatment Variable	0.648	
	(0.759)	
2010 Treatment Variable		−0.746
		(0.747)
Filer Status Control	Yes	Yes
Industry Control	Yes	Yes
Year and Quarter Control	Yes	Yes
Observations	6212	6212
R^2	0.285	0.285
Adjusted R^2	0.248	0.247
Residual Std. Error (df = 5903)	4.857	4.859
F Statistic (df = 308; 5903)	7.650***	7.632***

Note: *p < 0.1; **p < 0.05; ***p < 0.01.

that examines average changes – which treats changes by the largest ISPs and smallest ISPs as equally important – the impacts on larger firms may be lost. To address this, I run a series of quartile regressions for the 10th through 90th deciles, as well as the 25th, 75th, and 99th percentiles. This exercise breaks up the paper's model and allows the author to essentially weight changes in capital expenditure according to different bands rather than treating all ISPs firms equally. Table 9 presents the coefficients and p-value for

Table 9
Percentile regressions coefficients and P-Values for 2015 NN rule change.

	Coefficient	P-Value	
10th	95,923	0.0000	***
20th	14,144	0.7574	
25th	−59,759	0.8587	
30th	504,000	0.0000	***
40th	1,398,862	0.0000	***
50th	4,917,510	0.0000	***
60th	6,677,153	0.0000	***
70th	1,188,000	0.0257	**
75th	−1,211,488	0.9288	
80th	−11,567,120	0.0000	***
90th	−755,635	0.6856	
99th	402,902,700	0.0000	***

Note: Shows average estimated treatment size (quarterly) in USD and significance for the designated percentile.

the treatment term in each percentile.[10]

The results in Table 9 provide more nuance, but confirm the paper's overall conclusion. The treatment coefficient is negative for only four out of the 12 percentile groups. Of those, only one produced a statistically significant result – a decline of about $11.6 million on average per quarter for the 80th percentile. The other eight groupings produced positive coefficients with seven of those producing statistically significant results. This includes the 99th percentile, which includes the largest ISPs. The author cautions this test certainly does not provide conclusive proof that NN increased ISP investment and notes that the primary results in Table 4 show a negative (though insignificant) treatment coefficient for ISPs on average. However, it provide further evidence of no investment decline, even when weighting the analysis for firm size.

Finally, I address the potential issue related to the treatment dates of the paper's analysis. As discussed in Section 4.5, I use precise policy action dates for denoting the treatment periods for both the 2010 order and 2015 order. I argue this is the best approach for the current analyses (see previous discussion), but the use of the specific policy dates may mask either delays or preemption in investment changes among ISPs. The arguments are that firms may either change their investment levels ahead of the policy action change since they know it is coming (lead) or they may not be able to immediately change their investment levels after the policy action since capital investment decisions are long term. While I already address the issue to some degree through the use new capital expenditures incurred, I also rerun the primary regression specifications for a 6-month lead and 6-month lag for both the 2010 and 2015 orders. The author disagrees with this approach given the precision of the investment dates via the paper's dataset; however, the additional analyses yield very similar results to the primary approach. Once again, there is no statistically significant impact on investment decisions. Table 10 presents the results.

6. Discussion

Confirming a lack of evidence is sometimes just as or even more important than claiming to find new evidence. In the case of NN rule changes in the US, the lack of evidence of impacts found here is noteworthy given the quality of the data used in the paper and the flaws in previous analyses.

Again, the tests show no impact from NN rule changes in 2015 (or 2010) on telecommunication industry investment levels even when accounting for the repeal of rules in 2017 (and 2014). This lack of evidence suggests that the ISPs' investment incentive had not been sufficiently impacted to lower overall investment levels.[11] In other words, NN rules were not a strong enough factor to cause ISPs to decrease their investment as measured by new capital expenditures incurred (with an assumption of network capacity investment specifically). The author notes the finding was the same for both the 2015 and 2010 policy actions despite the fact that the paper's methodology treated them as separate events, allowing for differences in scope and design of the policy actions. The results in Table 4, while statistically insignificant, indeed show the two policy actions point in the same direction, but at different scales.

The evidence provides two significant improvements to the current empirical literature set. By using a fuller dataset and addressing many of the methodological flaws found in previous studies, the paper provides significantly more confident results to a weak empirical literature. Additionally, the paper more clearly lays out the theory behind the debate and, thus, the scope of the findings. Specifically, that more explicit linkage with theoretical modeling demonstrates that the current findings relate to the specific question, metric, and timeframe examined and should not be interpreted overly broadly.

Thus, it is important to temper the interpretation to some degree. Empirically, the paper illustrates no detectable influence on telecommunications investment using the general metric of capital expenditures. The paper was unable to specifically tackle the network investment component of that investment and reminds readers that such identification is a separately debated issue where

[10] Note that many researchers argue that r-squared measures are inappropriate for quartile regressions and the author does not include them here.
[11] Based on the paper's results, it appears neither the inability to generate new revenue through a differentiated (non-NN) pricing system nor the uncertainty around potential future price regulation during the NN period were strong enough disincentives.

Table 10
Effects of 2010 and 2015 Open Internet Orders on ISP Capital Expenditures - 6-month lag and lead

	New Capital Expenditures Incurred Per Quarter			
	(1)	(2)	(3)	(4)
Intercept	2,942,896.000	−24,293,499.000	−26,107,842.000	−17,410,506.000
	(280,015,796.000)	(282,046,294.000)	(282,076,488.000)	(280,379,742.000)
Telecom Industry Dummy	11,491,270.000	7,595,036.000	11,330,113.000	22,911,344.000
	(192,628,533.000)	(192,795,168.000)	(192,261,201.000)	(192,238,260.000)
2015 Policy Period Dummy - Lag	−24,221,069.000			
	(47,878,962.000)			
2010 Policy Period Dummy - Lag		−58,449,960.000		
		(96,440,007.000)		
2015 Policy Period Dummy - Lead			−29,141,867.000	
			(44,331,005.000)	
2010 Policy Period Dummy - Lead				33,887,693.000
				(38,262,494.000)
GDP Qtr Change	−668,924.300	−326,243.900	−627,987.800	−1,481,369.000
	(3,879,834.000)	(3,876,121.000)	(3,871,084.000)	(4,026,086.000)
Ten-Year Bond Rate	1,804,191.000	−411,857.200	1,681,645.000	12,609,510.000
	(22,875,008.000)	(23,216,715.000)	(22,872,084.000)	(26,369,001.000)
Interest Rate (discount rate)	11,955,711.000	84,587,059.000	65,806,222.000	−8,658,164.000
	(335,295,454.000)	(352,044,529.000)	(342,283,772.000)	(336,964,404.000)
2015 Treatment Variable - Lag	−4,929,080.000			
	(47,688,326.000)			
2010 Treatment Variable - Lag		5,022,130.000		
		(46,676,220.000)		
2015 Treatment Variable - Lead			−218,133.900	
			(53,459,386.000)	
2010 Treatment Variable - Lead				−32,061,752.000
				(44,547,222.000)
Filer Status Control	Yes	Yes	Yes	Yes
Industry Control	Yes	Yes	Yes	Yes
Year and Quarter Control	Yes	Yes	Yes	Yes
Observations	7223	7223	7223	7223
R^2	0.707	0.707	0.707	0.707
Adjusted R^2	0.693	0.693	0.693	0.693
Residual Std. Error (df = 6903)	317,877,085.000	317,874,548.000	317,873,329.000	317,854,142.000
F Statistic (df = 319; 6903)	52.093***	52.094***	52.095***	52.104***

Note: *p < 0.1; **p < 0.05; ***p < 0.01.
SEC Financial State Data Sets readme files for more information on filer status.

more work is needed (see Free Press, 2017; Singer, 2017). For pro-NN groups, the paper's evidence also dampens any claims of evidence that ISPs increased their investment post NN rule implementation.

Similarly, the paper's results imply that other claims of broad impacts from NN rules, such as increasing innovation or serving as a guard for free speech, may be equally fraught. This is not to say they are necessarily wrong, but that they are likely equally hard to back up empirically. The evidence here suggests they may have been exaggerated to some degree – if the effects of NN were insufficient to impact telecommunications industry investment, it may be reasonable to guess they were also insufficient to have a measurable impact on CSP innovation and content/speech moderation. This is, of course, speculative but worth exploring further with additional empirical work and relates to the previous comment on the paper's empirical-theoretical tension. The goal of the paper was to help advance the empirical work by addressing some of the key concerns of previous works. The author argues the paper achieves that goal. However, it meant sacrificing a proper examination of the broader theoretical questions on how NN would actually impact markets. There is significant nuance in the theoretical models that underlie the empirical questions, but the policy debate has largely ignored nuance in favor of a binary question on investment. The investment question has merit, but there are other aspects of net neutrality that require empirical investigation.

Finally, the results also do not rule out future changes in ISP investment. The period of analysis covered the full range of NN implementation in the US – the period when ISP investment should have dropped according to NN critics. It does not, however, speak to how long-term ISP investment (greater than the 8-year period measured in the 2010 treatment scenario) might change now that NN rules no longer exist in the US. The author acknowledges futures studies on the issue may find a significant increase in investment over time, assuming certain market conditions are met as laid out in theoretical models. Or, should NN rules return, they may disincentivize ISP investment in other circumstances at that time if market conditions are different.

Perhaps most importantly, the results here suggest that any claims of causal impact evidence from NN rule changes in the United States should be clearly scrutinized, particularly given the data and methodological flaws of previous studies documented in Section 3. The author does not claim the current paper as a panacea for all lingering NN questions. I argue this paper makes clear and important improvements from a methodological standpoint and draws stronger, but still imperfect conclusions. Yet, it presents a clear

challenge to any previous claims of NN causing investment to decline – including that of FCC Chairman Ajit Pai in his statement announcing the 2015 order repeal.

In the current context as US courts consider legal challenges and Congress considers legislation, there are important legal and regulatory issues worth debating and analyzing related to NN. There are important market considerations as well. The author argues the crux of these discussions relate to if and how the digital economy should be regulated. And while the legal classification of an industry may seem a mundane topic for some, the framework adopted – be it NN through Title II or something else – will have lasting ramifications for regulatory structures well into the 21st century. It does not, however, seem to have had any immediate impacts on the investment decisions of firms in the telecommunications industry.

For many interested parties, the NN debate seemed to offer a clear empirical question. Did investment go up or down? There are clear dates and clear policy actions to set up experimental tests. It was a deceivingly simple question on which too many individuals latched.

The current paper has the rare position to examine the issue while one step removed. The repeal process is over (apart from legal challenges) and sufficient data now exist to examine the issue properly. This does not eliminate the difficulties of conducting this type of policy evaluation, but it does allow the author to directly address many of the understandable weaknesses in earlier analyses. The author argues that, for the time being, this simple addition is an important step forward in what has been a far too contentious debate.

7. Conclusion

This paper examined the impacts of the 2010 Open Internet Order and 2015 Open Internet Order on investment by the telecommunications industry. Its main contribution is the addition of the first analysis to use a full dataset and established methods to examine these impacts.

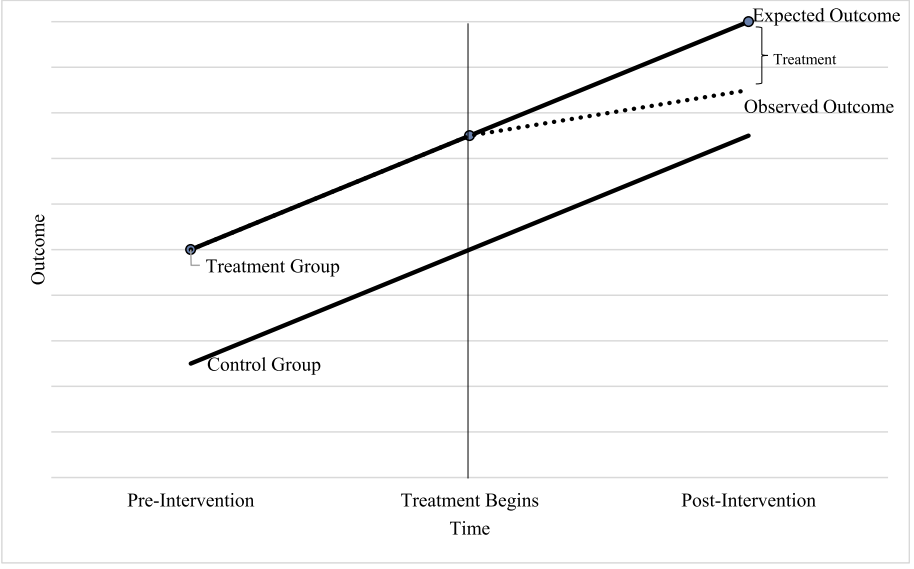
The author used a newly available dataset from the Securities and Exchange Commission with quarterly filings for every SEC-registered company from 2009 through 2018 and a comprehensive set of controls and explanatory variables. The level of detail in the data allowed the author to track new capital expenditures incurred through the full policy periods, down to exact dates based on financial statement issuances. Furthermore, the data allowed the author to address methodological issues in previous literature.

The results show no evidence of impacts from the NN regulatory actions. The treatment coefficients are not statistically significant and quite small given the level of investment by the telecommunications industry across multiple specifications and robustness tests. Overall, the analytical exercise highlights the difficulty of finding, selecting, and correctly controlling other influencing factors. The results suggest any claim of causal impact in investment decisions (be it up or down) are suspect.

Declaration of interest

Please note that the author has previously written on this issue in his role as Chief Economist & Head of Research at Internet Association, which is an industry trade group that argued in favor of net neutrality rules. However, the author has conducted the current research independently and strictly in his role as a Senior Scholar at George Washington University without comment, review, or input from any person associated with Internet Association.

Appendix A. Difference-in-Differences Stylized Example



Appendix B. Baseline Regressions – Specifications 1 and 2

Effects of 2010 and 2015 Open Internet Orders on ISP Capital Expenditures

	New Capital Expenditures Incurred Per Quarter			
	(1)	(2)	(3)	(4)
Intercept	82,886,160.000***	242,788,119.000	83,882,243.000***	243,530,580.000
	(14,741,361.000)	(661,682,387.000)	(15,184,938.000)	(661,729,180.000)
Telecom Industry Dummy	−13,046,341.000	362,686,094.000	−10,845,747.000	366,552,518.000
	(78,384,952.000)	(545,425,519.000)	(97,440,067.000)	(544,829,164.000)
2015 Policy Period Dummy	−41,556,907.000*	−66,746,121.000		
	(23,757,350.000)	(51,028,397.000)		
2015 Treatment Variable	13,797,897.000	−12,962,937.000		
	(142,377,424.000)	(129,851,591.000)		
2010 Policy Period Dummy			−40,427,169.000*	82,918,179.000
			(23,419,311.000)	(135,429,985.000)
2010 Treatment Variable			19,535,761.000	−24,401,422.000
			(131,574,558.000)	(123,767,064.000)
Filer Status Control	No	Yes	No	Yes
Industry Control	No	Yes	No	Yes
Year and Quarter Control	No	Yes	No	Yes
Observations	8515	8515	8515	8515
R^2	0.0004	0.282	0.0004	0.282
Adjusted R^2	0.00001	0.254	0.00000	0.254
Residual Std. Error	1,049,953,375.000 (df = 8511)	906,778,300.000 (df = 8194)	1,049,959,108.000 (df = 8511)	906,852,264.000 (df = 8194)
F Statistic	1.035 (df = 3; 8511)	10.066*** (df = 320; 8194)	1.004 (df = 3; 8511)	10.060*** (df = 320; 8194)

Note: *p < 0.1; **p < 0.05; ***p < 0.01.

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