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Evaluating Industrial Policies Using Event-Studies: Evidence from Stock Market Reactions to Obama's Stimulus Package

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By

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Abstract

This paper explores the possibility of using the event-study methodology to evaluate industrial policies. I utilize historical stock returns from the energy, telecommunications, and steel industries to calculate abnormal market reactions to several informational releases surrounding the Obama administration's "American Reinvestment and Recovery Act". The results of the event-study indicate a positive evaluation of the polices, both during the informational releases and in months following the official legislation. However, several mathematical and theoretical caveats are highlighted in the literature and in this particular study. The findings suggest that event-studies can be used as an evaluation tool for industrial policy to some degree, but innate weakness call for caution in their implementation

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I. Introduction

Macroeconomics, to a much greater extent than microeconomics, is subject to great debate between schools of economic thought. A key macroeconomic philosophy of the Classical School, for example, is the laissez-faire system: a free market dictates flows of wealth and capital, and government intervention only causes inefficiencies. Accordingly, only the “invisible hand” of the market is needed to correct periods of economic stagnation; historically this tended to be the case, until the Great Depression of the 1920’s-1930’s demonstrated a recession unable to be countered by simple market forces. This gave rise to the Keynesian School of thought, which calls for government intervention to correct economic inefficiencies.

To this day, economists still find themselves debating the extent of government policies targeting the economy. Specifically, "industrial policies" that aim to protect and foster growth in certain sectors within the economy can have extremely profitable—and detrimental— results. Given typically large levels of tax expenditure on these types of policies—and the potential economic damage they may bring about—it is important, as economists, to properly assess them for future implementation and allocation of government spending. In this paper, I explore a possible tool ("event-studies") for evaluating these industrial policies, which uses stock market reactions as a proxy for the predicted impact of said policies on their targeted industries. The crux of this idea lies in the efficient market hypothesis: all available information regarding markets is immediately priced in, and in turn, reactions to informational "events" can be observed in the changes of assets prices. Event studies analyze these immediate[1] changes as responses to these events and aim to draw conclusions from them. Currently, typical policy

[1] Issues of information lag and, conversely, speculation, are detailed in later sections

evaluation takes place after the resulting become observable; this can lead to issues of hindsight and misattribution. For example: if 5 years pass after implementation and empirical data indicates the desired policy results have were indeed achieved, Classical economists could still argue that a laissez-faire approach would have achieved better results without causing taxpayers. Instead, the event-study methodology provides instantaneous reactions to information regarding policies, and if the case can be made that these reactions by the market are indeed fair proxies for policy evaluation, they will be free from post factum bias.

To test the usefulness of event-studies in policy evaluation, I apply one to the "American Recovery and Reinvestment Act" of 2009, the Obama Administration's stimulus package containing a large set of industrial policies. The event-study analyzes the impact of the ARRA on the energy, telecom, and steel industries, by capturing the abnormal behavior of related stocks in response to new information regarding the policy. This is done by predicting expected stock returns from historical data, and comparing it to actual returns on days information is released. My findings indicate the prevalence of significant market reactions to these information releases, supporting the idea that event-studies may be used in some capacity as a policy evaluation tool. In reaction to the Obama Administration's industrial policy package of 2009, both energy and steel industry portfolios yielded significant abnormal returns to various events regarding news of the policy. On the official day of the ARRA being implemented, these two portfolios exhibited abnormally positive returns of ~7% and ~8%, respectively, significant at a 1% level. In addition, the portfolios exhibited negative and positive reactions during periods of debate and speculation over the drafting of the ARRA, again at statistically significant levels. These results indicate a compelling story for event-study application in policy evaluation. However, the study design highlights some methodological issues with using event-studies to analyze entire industries rather than single stocks, while the literature review assesses innate theoretical issues with drawing macroeconomic conclusions from microeconomic rational. Ultimately, this paper contributes a synthesis of the empirical and theoretical evidence for and against the use event-studies as a policy evaluation methodology, both by reviewing the surrounding literature and by conducting an example event-study on a series of industrial policies and their respective events, as to witness the methodology at work.

The paper is sectioned as follows: Section II provides a review of the literature surrounding the event-study methodology, and briefly discusses possible problems with using this process to evaluate industrial policies (as opposed to its typical application in finance and stock analyses). Section III describes the nature of the data and its collection process for this study. Section IV re-introduces the event-study model and methodology as it is to be used in this paper: to calculate abnormal stock returns during information leaks regarding industrial policies. Section V discusses results, robustness issues, and implications. Section VI offers a conclusion on these findings. Section VII contains an appendix of referenced diagrams and tables.

II. Literature Review

Background on Industrial Policy

Before we begin discussing the event-study as an assessment tool, it is important to discuss the principles and theories behind what we are, in fact, assessing. Industrial policy is defined by Ramizo (2016) as “the government’s use of its authority and resources to foster industry development, to intervene with private sector weaknesses or to create growth in selected areas”, a definition concatenated from Okimoto (1989) and Pack & Saggy (2006); this definition and the author's wording, however, need not be the universal standard. Across publications and textbooks, economists certainly agree that industrial policy is—to varying degrees—a form of intervention by the government in a particular industry, and that many policies and actions fall under this defining umbrella. What economists fail to agree on is the implementation of these policies, and perhaps more broadly speaking, the government's role in the economy in general.

The Classical Economists championed laissez-faire ideology and minimal government interference; it took the historical backdrop of the Great Depression in the early 20th century for Keynesian Economics to call for interventionist policies when the "invisible hand" of the freek market failed to correct deep economic recession. And even then, late 20th century economists failed to recognize or accept the power of industrial policy in newly industrialized economies (NIE's), leading to the now thoroughly examined economic "miracles" in East Asia, noted by Weiss (2005): South Korea, Hong Kong, Taiwan, Japan. Conversely, Ramizo (2016) argues that effectiveness of industrial policies, in a broad sense, is entirely contextual and differs case by case, country by country; Johnson (1982), Okimoto (1989), Rodrik (2007) and Wade (2004) present the case of the success of NIE's in East Asia, while Bell (2002), Wade (2010), and Rodrik (2007) outline the failures in Latin America and the Asia-Pacific region. The metrics behind categorizing these cases as successes or failures on macroeconomic levels typically involve post-policy reviews of macroeconomic-level data: Johnson (1982) cites, for example, a time series data set at the industry level illustrating the exponential growth of Japanese industries between 1926 and 1978, based on a variety of established indices. Wade (2004) empirically argues the success of industrial policy in East Asia through measured increases in per capita growth and private capital flow, though cannot find a similar story of policy success in Latin America. Indeed, various schools of thought cite a spectrum of theories regarding governmental—and specifically, industrial—policy, calling for a need of proper methodological practices to evaluate these interventions and facilitate proper debates among economists and policy makers.

However, the theories behind successful industrial policy are not the focus of this paper; rather, I am concerned with the theories surrounding evaluation methods of industrial policy (the two are, of course, intertwined to a degree). The concern with the above mentioned macroeconomic variables as measures for policy evaluation lies in the dangers of hindsight: if the policies in question had not been enacted, would the result (indicated by said variables) have been better or worse? Surely these metrics supply evidence of success or failure, but to claim direct correlation between the policies in question and the indicated macroeconomic changes (positive or negative) is perhaps not the entire story, and could be misleading. This has motivated me to explore an alternative, more "immediate" evaluation method, one that speculates the success of policies as they are announced in real time, rather than reflecting on subsequent macroeconomic results. This event-study methodology is indigenous to a microeconomic world, measuring informational impacts on financial markets rather than the national economy. The anticipated effects of policies on these markets may or may not be the best proxy for the entire welfare effect on the macro-economy. Industrial policy is not aiming to make investors happy, but to foster growth in key industries, whether that has an implication for firm profitability or not. Yet, a case can be made for the growing interconnectedness between a strong financial industry and a strong overall economy. By 2006, the financial industry had grown to around an 8% share of total GDP in the U.S., reinforcing the idea that evaluations made by participants in financial markets may carry significant weight in evaluating larger, macroeconomic policies.

Brief Overview of Current Policy Evaluation Methods

The literature regarding policy evaluation is itself as extensive as the literature regarding the theories behind said policies; the OECD consistently publishes work regarding evaluation techniques, and the issues coinciding with these methodologies (Warwick & Nolan, 2014). The authors extensively review the current pallet of evaluation strategies available to policy makers, separated primarily by policy type. For example, the standard OECD toolbox for evaluating policies regarding R&D (creating investment incentives for firms, tax crediting to minimize cost associated with R&D risk) consists of "surveys; quasi-natural experiments; techniques using statistically constructed control groups; and structural econometric modeling" [2]. The paper dives extensively into a variety of methods for assessing the industrial policies available to OECD countries (and others); surprisingly, there are gaps in the literature regarding event studies as a potential policy assessment tool, albeit Haji Ali Beigi and Budzinski (2012). Rivolta (2012) examines the effects of the European Central Bank's unconventional monetary policy via an event-study: the policies examined aimed to combat the European debt crisis, and thus the effectiveness of these policies was analyzed using changes in government bond yields. In this case, the policies targets were intrinsically related to financial assets, deeming an event-study appropriate. The lack of event-studies on industrial policy (as opposed to monetary policy) throughout the literature is possibly due to a weaker theoretical connection between policy targets and industry-related assets. Event-studies focus on instantaneous market reactions, rather than long-run impacts on stocks from discrete informational releases. A later section of the

[2] Quasi-natural experiments, unlike typical experiments, lack random assignment. In the case of more complex industrial policies, Warwick and Nolan note that secondary data analysis may not be sufficient, and often an iterative learning environment is necessary. Here, a quasi-experiment is appropriate, as policy conditions are not random but in fact chosen specifically. This is a consequence that may have to be accepted in the cases of extremely complicated macroeconomic issues, where countless variables cannot be controlled for.

literature review will expand on the possible issues with applying the event-study methodology to evaluating industrial policies, explaining the gap in policy-assessment literature. Before properly discussing the practicality of event studies in assessing industrial policy, we must first review the nature of the methodology in its original context.

Background of the Event-Study Methodology

Conducting an event-study entails measuring investors reactions to available information. Investor reactions are embodied in the movement of asset prices; most often we see event studies examining stock return rates at the individual, portfolio, or index level. Alternatively, with the breadth of financial instruments, event studies may proxy the market "reaction" to information in other ways; Shaikh et al (2013), for instance, use an Implied Volatility Index to gauge market uncertainty and volatility surrounding scheduled macroeconomic announcements in India. [3] This approach deviates slightly from the standardized version of the event-study, introduced by Fama, Fisher, Jensen, and Roll (1969) in their canonical paper (cited over 500 times since publication). Binder (1998) reviews the FFJR paper in a concise manner, presenting the event-study in its original form and highlighting the methodological evolution arising from its imperfections. The paper discusses two paths of implementation for event studies: the first discusses the case of measuring residual, abnormal returns from a modeled benchmark (using the market model / market portfolio), which is the essence of the original paper; the second presents the econometric case of abnormal returns being represented in a regression model as coefficients. The FFJR

[3] Often denoted VIX, an Implied Volatility Index is a composite of the implied volatility of individual stocks. This implied volatility is "implied" by the option prices of each stock. The calculation methods vary, as there are many options on a stock, but a detailing of this process is not necessary for the purpose of this paper. The idea behind using VIX (or individual implied volatility) instead of closing prices or trade volume is to better capture whether there are changes in uncertainty regarding events, rather than attempting to observe positive or negative changes in asset returns in lieu of an event.

paper, as Binder notes, aimed primarily to capture the effect of an event within some month on a stock, rather than on treasury bonds (Balduzzi, Elton, & Green 2001), treasury futures (Kurov et al 2016) or volatility indices (Shakih et al 2013). Rather, Binder notes that FFJR controls for the relationship between the return on stock *i* during month *t*, denoted R_{it}, and the return on a broad stock market index during the month *t*, denoted R_{mt}. The "market model" for each stock becomes the following, as seen in countless subsequent papers implementing the now standardized methodology of the seminal FFJR paper:

Expected Return of Stock *i* at Time *t* Using Market Model:

$$E[R_{it}] = \alpha_i + \beta_i R_{mt} + \varepsilon_{it} \quad \textbf{(1)}$$

As a statement: the expected return of a stock *i* at time *t* is equal to the return of the market at time *t*, multiplied by the correlation coefficient between the market and said stock, plus a linear intercept and error term (note that this takes the standard linear form of $y = mx + b$, where *m* and *b* are obtained by regression of historical data for the stock *i* and the market). As in Lee & Mas (2012), the original paper defines the event variable *t* as *t* = 0, with discrete time variables "before" and "after" the event being represented as negative and positive values, respectively. When looking for abnormal returns, FFJR use cumulative average abnormal return, given by

Cumulative Average Abnormal Returns between s_1 and s_2:

$$CAAR_{s_1 s_2} = \sum_{s=s_1}^{s_2} AAR_s \quad \textbf{(2)}$$

where *S* marks the boundaries of the event window.

Average Abnormal Return for *N* stocks:

$$AAR_s = \sum_{i=1}^{N_s} \frac{AR_{si}}{N_s} \quad \textbf{(3)}$$

which is the average abnormal return (of which we sum to obtain *CAAR*). Here, *N* is the number of firms (stocks) in the sample, and *AR* is the abnormal return for each respective firm (stock) in the sample. Abnormal returns are calculated as

Abnormal Return of Stock *i* at time *t*:

$$AR_{it} = [R_{it}] - R_{it} \textbf{ (4)}$$

the difference between the expected return of stock *i* at time *t* and the actual, observed return as a reaction to new information entering the market at *t*. This is the baseline methodology given by FFJR in 1969.

The "market model" has become the standard practice for event-studies, as it imposes few restrictions on calculating abnormal returns. Finance theory suggests that using the "CAPM model" would provide more accurate results, as it incorporates the risk-free rate (the "market model" does not).

Expected Return of Stock *i* at Time *t* Using CAPM Model:

$$E[R_{it}] = R_{f,t} + \beta_i(R_{m,t} - R_f) \textbf{ (1.2)}$$

where $R_{f,t}$ is the risk-free rate at time *t*. This additional restriction will result in larger variance of the error terms, and thus is less statistically powerful than the market model.

Adaptations of the Original Event-Study Methodology and Theoretical Assumptions

Binder (1998) notes that two modifications have become standard in event studies since the paper's publication. One regards the sample size; the author notes that, in fear of the drawbacks of stationary market model parameters, studies involving monthly observations (note: not daily, as in this paper) are to use five to seven years of monthly return data. The second adjustment, which the original authors (and others: Ball and Brown 1972) note, is the danger of including the event period of interest in the same period used to estimate the market model parameters. This will cause the coefficient estimates to be biased, as the estimation window for "normal" returns now includes an event which, by hypothesis, causes "abnormal" returns. FFJR fail to adjust for this, and studies with larger event windows must account for the inherent bias of using a timeframe to model “normal” stock activity that also includes the event which, under hypothesis, should trigger “abnormal” stock activity. Studies following FFJR account for this; Scholes (1972) estimates the market model parameters with data prior to the event window, and uses the error residual e to estimate abnormality during the actual event period. Here, e is derived from the historical returns of the stocks and overall market portfolio--by historical we mean pre-event-- and serves as a predictor variable for abnormal returns.

In formatting ways to discuss *CAAR* or *AAR*, authors may also choose hypothesis testing and linear regressions (OLS or otherwise) as their preferred methodological framework. Here, the event-study boils down to testing the statistical significance of their modeled *AAR* or *CAAR*; this can only follow from assuming that each individual stock’s return is independent, and that the returns are evenly distributed. Once these assumptions are made, the typical process for calculating variance and standard deviation is implemented. These underlying assumptions may be difficult to claim in the case of this paper. When discussing the impact of industrial policy on industry firms, it may be difficult to claim that each individual stock’s return is independent of its industry neighbors.[4] By nature of their competition with one another, stocks may have inverse performance relationships (based on competitors’ performance), or may move in tandem, as they

[4] The relationship between stocks in the same industry can be identified in an identical manner to calculating to relationship between stock *i* and the market, as per the event-study methodology. Regressing stock *i* on stock *j* will return a return coefficient and intercept β_i, j and $\alpha_{i,j}$ that can be used to interpret whether or not stocks in the same industry "portfolio" move in tandem or against one another, historically. For the breadth of this paper, this type of analysis is omitted. Instead, I assume the portfolio of stocks used (from Yahoo! Finance's "sector" lists) is random enough to perform significant analysis. This is discussed further in the Data section.

are affected by the same exogenous and endogenous industry shocks. A further assumption that must be made is that *AAR's* are independent over time; can we assume the case that past stock returns do not influence future stock returns? If that assumption can be made, then the standard deviation of *CAAR* can be derived:

Standard deviation of Cumulative Average Abnormal Returns:

$$AAR: \sigma(CAAR_{s_1 s_2}) = [\sum_{s=s_1}^{s_2} \sigma^2 (AAR_s)]^{\frac{1}{2}} \textbf{ (5)}$$

Though it is important to understand the simplified statistical tests that could be conducted on *AAR* and *CAAR,* given these strict assumptions, Binder notes this is quite unrealistic, and that these restricting assumptions must be relaxed for any kind of valuable statistical analysis to be performed. The big hurdles to overcome when working with the returns of many stocks over a period of time are as follows: returns are cross-sectionally correlated; returns have different variances across firms; returns are not independent across time for a given, individual firm; returns have greater variance during the event period than in surround periods. Referring to my initial concern regarding return correlation amongst stocks in the same industry sector, Binder cites King (1966) as identifying that “market model residuals are contemporaneously correlated for firms in related industries", an issue even noted before the publication of the 1969 FFJR paper on event studies. This highlights an immediate statistical weakness in using an event-study to assess policies at the industrial level; if market model residuals (error terms) are correlated across firms within the same industry, the results of an event-study regarding these firms' reactions to industrial policy announcements may have an autocorrelation issue. This has an innate relevance to my study, as I am specifically looking at stocks in the same industry. A typical result of autocorrelation is t-statistics that look "too good", which can cause misleading conclusions about market reactions to policy information[5]. Methods by various authors have arisen to combat these issues: Jaffe (1974) and Mandelker (1974) present alternative event-study methodologies to tackle these two cases of heteroscedasticity and autocorrelation via the "portfolio method" (Binder 1998). Here, AAR_t is standardized by dividing the estimate $\sigma(AAR_t)$ by the standard deviation, assuming that the AAR_t is independent over time. A similar issue regarding industry-specific stocks is peer effects (or informational contagion). Lang and Stulz (1992) highlight this phenomenon in relation to bankruptcy announcements, illustrating that an individual firm's bankruptcy creates negative expectations for firms within the same industry, regardless of their economic welfare. This contagion effect can, theoretically, affect the returns industry-akin firms more than the actual information about the related policy. Adjusting for this effect is beyond the scope of this paper, but we must be aware of it as we evaluate the legitimacy of event-studies for policy evaluation. Further detail regarding alternative econometric and statistical solutions to event-study issues will be discussed in the Methodology section.

Literature on Modeling Abnormal Returns (various methods)

Issues regarding correct modeling of returns are abundant in the event-study literature. It should be noted that the other key ingredient to an event-study--not just modeling returns accurately, but also fully understanding when information hits the market-- is not always easily obtained. According to Shakih et al (2013), scheduled announcements cause market volatility (preceding the known date) to increase, dropping after the announcement is made. Empirically illustrated by changes in a volatility index, return rate fluctuation before the information's release

[5] We indeed see large t-statistics in the average abnormal returns, discussed later in the Results section.

implies a period of speculation regarding the event. With surprise events (death of a CEO, sudden industry-wide exogenous shocks), issues with informational leakage and speculation are not as prevalent. In these cases, it is easier to capture market reactions in a narrower and precise event window, and abnormal returns become more apparent. However, with scheduled announcements--regarding macroeconomic indicators and, in my interest, industrial policies-- speculation and informational "drift" through the market makes it more challenging to pinpoint when exactly the market is reacting to the news. Kurov et al (2016) explores this issue. The authors use an event-study methodology incorporating MM weighted least squares, in conjunction with *CAAR* graphs, to detect "drift" of information regarding public, scheduled macroeconomic announcements in the private sector. [6] They note that "incentives for privately collecting information and leakage are high", and that indeed there is a motivation for investors to obtain early information (via leaks or superior analysis) to beat the market when it reacts to the scheduled announcement. The paper aims to fill the gap in event-study literature regarding pre-lease price drift, which is a phenomenon I aim to incorporate in my paper, as industrial policies are similar scheduled events, and correctly pinpointing market reactions--early or otherwise-- is crucial in evaluating the power of event studies as a policy evaluation tool. In the case of industrial policy, the official enactment date may not be the date the market reacts. Although there may be cases when abnormal returns occur on the day policies officially pass in Congress, information regarding preliminary talks must be taken into consideration when selecting appropriate event windows for market reactions. Kurov et. al (2016) uses a similar model to what we've seen in the preceding literature (FFJR and following):

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[6] MM-weighted least squares is an alternative to the Ordinary least squares methodology, and is more robust to outliers than OLS. A detailed derivation of MM-estimators can be found in Yohai (2012), which cites the original Huber (1981).

Return on Asset at Event *t:*

$$R_{t\pm\tau} = \gamma_0 + \gamma_m S_{mt} + \varepsilon_t, \textbf{(6)}$$

where γ_0 is the return on the asset unconditional to any abnormal information not already priced in--according to the efficient market hypothesis--which represents the constant α in our standard model. Here, the difference lies in the S_{mt} term, which represents the market's reaction to a macroeconomic announcements. We have

Return of Market at Event *t:*

$$S_{mt} = \frac{A_{mt} - E_{t-\tau}[A_{mt}]}{\sigma_m} \textbf{(7)}$$

where A_{mt} is the actual announcement released at scheduled time *t*, and $E_{t-\tau}[A_{mt}]$ is the market's expectation of the announcement before it is released. The authors proxy this expectation by "the median response of professional forecasters during the days before the release (a survey conducted by Bloomberg). The regression coefficients in equation (6) are obtained in an appropriately small high-frequency trade window of $[t - 5seconds, t + 5seconds]$, and the usual event-study methodology is conducted. Various event windows are reapplied; specifically, and event window of $[t - 30minutes, t + 5seconds]$ is used to capture the activity of investors with leaked or superior knowledge, who typically act within 30 minutes of the scheduled release time. This methodology poses as a template for conducting my particular event-study--albeit the difference in high frequency trading data versus my daily return data--around industrial policy release times and detecting possible scenarios of informational drift. Kurov et. al (2016) focus on informational leakage regarding macroeconomic indicators, which entails actual values that can be speculated on by the market prior to release. In the case of policy announcement dates, this may not always be the case; there may be speculation on, for example, the dollar amount of a subsidy or tax credit, but superior information regarding policies qualitatively boils down to whether or not the bill will pass. Thus, measuring drift in this case will entail identifying when (before the official announcement date) the market anticipates the certainty of policy implementation, and reactions accordingly based on its evaluation of said policy.

Examples of Event-study Application

With a basic understanding and outline of the event-study methodology (and some of its drawbacks and difficulties), I explore two instances of their implementation to serve as a template for my own; the first examines market reactions to labor union voting information, while the second explores, as I intend to myself, the reactions of investors to industrial policy in the electricity industry.

Lee and Mas (2012) aim to analyze the long-run impacts of unionization of employees on their respective firms. The "impacts" on firms are measured by firm profitability, which itself is measured by the reactions of investors to unionization news. Here, using an event-study is quite powerful, as the events and relative information to investors are all at the firm level, versus the industry level (which I aim to analyze). The authors take note that unionization (at the firm level) can be modeled discretely (voting regarding unionization happen at time *t*), allowing for a "before and after" analysis of stock returns to take place, rather than having to deal with the complexities of a continuous variable. A comparison of cumulative returns on a selected firm that underwent unionization and those of an equally weighted portfolio of stocks (unaffected by the unionization) is made before and after the event date, and graphed accordingly. This graphic motivates the event-study methodology in regards to union impacts on firms, as it clearly shows the vast deviation of cumulative returns between the single stock and the equally risky portfolio (the market index), post unionization; in just two years, the firm's stock price had fallen nearly -15%, while the market index had increased in price by +25%. Though these facts are not entirely pertinent to my studying of industrial policy, it illustrates a useful comparison technique for highlighting abnormal returns; comparing the stock (or industry) of interest to a similar proxy--that of an equally risky portfolio or index of some sort, rather than attempting to model the expected return of the stock (or industry) of interest based on any of the techniques mentioned in the previous paper. However, shortly after the authors demonstrate this glaring effect of the unionization of the firm, they beg the question of whether this is the exception, or the rule. What is the general case for unionization affecting firms, and what are the complications of using an event-study to answer this question (as the first paper addressed)? According to the authors, there are at least three reasons that "measuring these effects is quite challenging". They make that case that information regarding the extent of unionization is not readily available on a micro-level; this would be analogous to the issue of availability of data regarding the pervasiveness industrial policies in particular industries, which is undoubtedly a challenge. Second, they highlight the issue of abnormal returns being affected by "omitted variables and the endogeneity of unionization at the firm level", which makes it difficult to separate causal effects regarding investors reactions to unionization (or industrial policy, in our case) from other unobserved confounding factors. Their third point touches on the concept that, if this data is actually available, the chances that the firms for which data exists represent the entire population of firms that can be possibly affected by unionization are quite small. Analogously, we have less of a problem with this representative idea; industrial policies are, by nature, intended to affect the entire industry, and not affect individual firms in extremely varying ways. Yet we must take note that this is not always the case. Different policies may indeed affect firms in different ways (example: combatting anti-competitiveness and dismantling monopolies), and it will be crucial in my research to understand, perhaps, how this "general" or "targeting" policies affect various firms, and how investors react correspondingly. It may also be the case that the correlations between policy announcements and investor reactions may not be equally weighted throughout different industries; tech-industry policy and agricultural- industry policy may easily create strong or weak market reactions, based on both the nature of the investors in these industries, and the nature of the policies. In summary; making claims on the usefulness of event-studies as a tool of assessing industrial policy *in general* may be dangerous; it may be the case that it is a useful tool for some industries and some policies, but not all. In the case of unionization and using election results as the "events", the authors found sources readily available from the National Labor Relations Board, which archives all elections; the data regarding asset price changes came from the Center for Research on Security Prices. Though finding stock return data at firm level or industry level (via an index) will be a fairly simple task for my research, the difficulty comes with finding data on the "events": industrial policy announcements. In comparison to the previous paper on methodology for calculating "abnormal" returns, the authors define the term in an identical way. However, this paper uses cumulative abnormal returns surrounding an event window, as opposed to discrete abnormal returns within a window as the previous paper had, and taking the average. Thus, *Average Cumulative Abnormal Returns* is defined in this paper as

Average Cumulative Abnormal Returns Between T_1 and T_2:

$$ACAR(T_1, T_2) = \frac{1}{N} * \sum CAR(T_1, T_2) \quad \textbf{(8)}$$

where T_1 and T_2 define the months surrounding the window of interest. With this template, we may now explore another.

Dnes et al (1998) detail an example of using event studies to discuss the effects of industrial policy on a particular industry; here, the electricity industry in the UK, which was privatized in 1990. The motivation behind using an event-study as an evaluation tool for the newly imposed regulations was based on the idea that, if the stock market evaluated the regulatory regime as equivalent to a normal competitive regime, their returns would correspond with those of assets exposed to similar levels of risk. In other words, stocks would indeed reflect investors evaluating the regulation policies as fair and akin to conditions of a normal competitive market. The authors note the power of the event-study in its ability to illustrate trends in abnormal returns in the specific case of regulatory capture; regulatory regimes become “more captured” as time progresses and special interests and motives become more coherent and organized, which calls for a respective analysis over time. The “event” data was pulled from the index of the Financial Times within the particular period of interest (10 events were used in total), with event windows of around 3 months. To ensure the timing of information becoming available to the market was accurate, the authors cross-referenced the data from the *FT* with the records of OFFER and the REC’s. Daily share price data was pulled from Datastream, with around 1,200 observations per company involved in the study. The study used a market index for which the returns of an equally weighted portfolio of electricity company shares would be compared to. A note is made to adjust returns to take into account dividend payouts and share splits / buy-backs. In terms of the methodology of their event-study, Dnes et al. use 3 different measures of abnormal returns, and carried out both individual day and multi-day analysis of these returns. *MAAR* (mean adjusted abnormal returns), *MAR* (Market Adjusted Returns) and *OLSMAR* (OLS market model returns) were the 3 metrics used.

Mean Adjusted Abnormal Returns of Stock *i* at time *t*:

$$MAAR = A_{i,t} = R_{i,t} - E[R_i] \quad \textbf{(9)}$$

where

Average Return of Stock *i* over window *T*:

$$E[R_i] = \frac{1}{T} * \sum R_{it} \quad \textbf{(10)}$$

Mean Adjusted Return of Stock *i* at time *t:*

$$MAR = A_{i,t} = R_{i,t} - R_{m,i} \quad \textbf{(11)}$$

and

Ordinary Least Squares Mean Adjusted Return of Stock *i* at time *t*:

$$OLSMAR = A_{i,t} = R_{i,t} - a_i - b_i R_{m,i} \quad \textbf{(12)}$$

All three models are used in the event-study;[7] the *OLSMAR* methodology is used by running a regression on the past 150 days of trading, while the authors use 250 days of trading before the event when calculating *MAAR*. The events in question by the authors were dates regarding announcements of regulatory policies in the electricity industry; these events were not, however, just dates when policies were enacted. The authors included a range of events revolving around said policies: publishing dates of papers discussing the benefits and drawbacks of particular policies, direct announcements of price control proposals, revisions, and officially stated acceptances of these proposals by affected groups. By doing this, the authors avoid oversimplification of the steady inflow of information regarding policies via key events.

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[7] In later additions of this paper, recalculating using equation's (1) and (2) may lead to interesting results. Equation (2) defines abnormal return as the discrepancy between a stock *i*'s return and the overall market return. In the case of this paper, where the selected industrial policy effects more than one specific industry, it is possible that the overall market is subject to abnormal returns, and thus cannot operate as a reference point for modeling individual stock's behavior. I, however, found it important to mention the various methods of calculating abnormal returns throughout the literature.

Analyzing one event and marking it as the critical point where investors become aware of policies tells a misleading story; surely, the exact date a policy passes may be first news to some investors, and may mark the point of no return regarding speculative policy revision, but in reality most investors can speculate certain policies are guaranteed during the early stages of their proposals. By stretching out the “event-window” to encompass the various stages of policy creation and implementation, the authors are able to capture more of the information leakage, and perform a more accurate event-study (capturing of abnormal, reactionary returns to these sequences of policy-related events). This strategy is critical to ensuring that one develops an accurate time-frame to search for abnormal returns related to information regarding these policies, and not, for example, abnormal returns related to any other exogenous (or endogenous, for that matter) factors.

Though Dnes et al highlight the possible issues (and resolutions) with their methodology, it is rather case specific. Beigi et al (2012) give a brief overview of the complications that arise when using event studies to evaluate economic policies. The key components to using this methodology for an analysis of economic policies are 1) that information regarding these policies is available to the (public) market and is priced in accordingly (based off of the efficient market hypothesis) 2) traders in the stock market correctly anticipate the effects of the policies on their targets after gaining information about them 3) changes in stock returns, which reflect on the future profitability of the firm or on the firm’s value, are good proxies for economic welfare (this is policy dependent; for industrial policy, perhaps yes, but how rent-control policies affect the stock market and how they affect welfare may not be in tandem). Other issues revolve around the idea that these publicly traded companies are only a subset of the whole economy, and that investor’s views of the policy impacts may be short term compared to the policies’ intended long term aims. The paper gives 3 *brief* examples of papers using event studies to discuss particular policies; policies on trade, mergers, and crisis recovery, respectively.[8] As a whole, this paper served as a template for outlining the basic concerns with utilizing event studies outside their specific application in finance and in the realm of economic policy assessment, and was much less crucial to developing a deeper understanding of the mathematical and methodological variations in conducting the event-study. Beigi et al (2012) illustrate theoretical issues with applying event studies to economic welfare evaluations; Binder (1998) and Kurov (2016) outline methodological issues of event studies in general, regarding heteroscedasticity and autocorrelation among assets in related (and unrelated) industries, and regarding informational "drift" and identifying when informational reactions by investors actually occur.

Contributions

Though there is abundant literature on industrial policy (theoretical and empirical) and event-studies (again, both theoretical and in application), there is a significant gap in papers tying the two topics together. As noted earlier, the reason for this may lie in the several issues associated with modeling macroeconomic policies with reactions in a financial market, as well as several innate statistical issues that come with conducting an event-study on stocks within the same industry and over a longer period of informational speculation. With these caveats in mind, this paper aims to conduct an event-study on a set of industrial policies and confirm or dispute these potential issues, and assess whether or not this financial methodology holds a place in the realm of policy evaluation. Through this process, we will be able to draw conclusions on the

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[8] The description of the methodologies implemented in these three cases does not differ from the methodologies discussed early in the literature review

short-run market reactions to these policies, and the observable implications. From here, we may proceed to discuss the specific model and methodology of this paper.

III. Data

To understand the applicability of the event-study methodology on evaluating industrial policy, I chose to look specifically at the American Recovery and Reinvestment Act of 2009, Obama's economic stimulus package to the American economy following the 2008 recession. This policy was selected for a variety of reasons; it targeted several different industries (energy, telecom, steel) using distinct policy instruments, and was well covered by media sources, allowing for a more accurate timeline of information leaks. However, there are some drawbacks (which will be discussed in further detail in the results section), including a long period of speculation (starting during Obama's early campaign and following into the final days of drafting of the bill), and a major split in opinion--by the public and economic professionals-- on whether these policies were good or bad.

Data collection involved two distinct processes: finding exact dates for when information regarding the ARRA hit the market, and finding trading data on appropriate stocks that would be affected by the news. Searching the online archives of the New York Times, Wall Street Journal, and Washington Post yielded substantial results for headline news regarding the ARRA (some dating back well before the bill was formally introduced to Congress), while government sources--the website for the Speaker of the House-- provided information on the timing of official announcements, report releases, and speeches regarding the bill. [9] Table 1 contains the

[9] https://pelosi.house.gov/news/press-releases, accessed on 3/28/17

selected dates of interest for the event-study on the ARRA, while Table 2 breaks down the industries targeted by the ARRA and through what policy mechanisms.

Once the events regarding the ARRA were identified and targeted industries outlined, relevant stocks and their historical data were pulled from Yahoo Finance and into Excel using the built in API and VBA code.[10] The stocks were selected based on Yahoo Finance's categorization of industries, under the "Energy"[11] and "Telecom"[12] sectors. Stocks for steel and iron were chosen by researching publicly traded manufacturers and related firms in the industry.[13][14] Because these stocks were selected in an arbitrary manner, the true impact of information on the ARRA may be more or less significant than if more research was put into how the bill would affect specific, individual firms.[15] However, because the ARRA is such a broad policy, and these portfolios of stocks are randomized within the same industry, some significant results should still be achieved; Table 3 gives the selected stocks.

A larger list of stocks was used in an earlier version of these paper. The original list-- between 20-50 stocks depending on the industry-- was combed through to create a more representative portfolio of industry stocks. These selected stocks had to be headquartered in and primarily serve within the United States (the previous portfolio contained stocks within the correct industries, but were international and unaffected by the ARRA). These firms range from large to mid-size market caps, with a few small cap firms in the steel industry. In the telecom industry portfolio, the largest broadband providers were selected; the ARRA policy aimed to put

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[10] Kahn, Samir. *" Multiple Stock Quote Downloader for Excel"*, http://investexcel.net/multiple-stock-quote-downloader-for-excel/ (Accessed February 10, 2017)
[11] https://finance.yahoo.com/industries/energy
[12] https://finance.yahoo.com/industries/telecom_utilities
[13] http://investsnips.com/list-of-publicly-traded-steel-and-iron-product-companies/
[14] http://www.dividend.com/dividend-stocks/basic-materials/steel-and-iron/
[15] This will be explored further following the rough draft. For now, these lists of stocks suffice to run the remainder of the methodology. Certain stocks may be excluded based on their irrelevance to the "Buy American Provision" within the ARRA

$7 billion into nation-wide broadband access. The energy portfolio includes large and mid-sized oil and energy companies in the United States, as well as bio-fuel companies -- $800 million was put into biofuel research in the ARRA. The "Buy American Provision" policy protected United States steel by mandating all government building projects requiring the material to purchase it from domestic suppliers. International steel producers were thus eliminated from the original portfolio, and only large to mid-sized steel manufacturers that produce in the United States were selected. These firms ranged from raw steel producers to downstream firms that specialized in more niche steel products and refinements.

IV. Methodology

Model

To conduct my event-study, I closely followed the original methodology of FFJR (1969). The model for expected return of stock *i* at time *t* is

$$E[r_{it}] = \alpha_{im} + \beta_{im} r_{mt} \textbf{ (1)}$$

That is, that the expected return of stock *i* at time *t* is determined by its return coefficient to the overall market β_{im}, multiplied by r_{mt}, the return of the overall market at time *t* , plus some constant intercept α_{im}. Abnormal returns are calculated by

$$A[r_{it}] = E[r_{it}] - r_{it} \textbf{ (4)}$$

where r_{it} is the actual recorded return of stock *i* at time *t*. The difference in the observed return and the expected return is the abnormal return created by some reaction by investors to the event at time *t*, which became priced in according to the Efficient Market Hypothesis. Finally, the Average abnormal return for the portfolio of *n* stocks is calculated by

$$\text{(3) } AAR_t = \sum_{i=1}^{n} A[r_{it}]$$

for time *t*. When looking at the change in stock prices for the steel industry in particular, I calculated the *CAAR* for a 2-month period following the implementation of industrial policy, where

$$CAAR = \frac{1}{T} \sum_{t=1}^{n} AAR_t \text{ } \mathbf{(2)}$$

T-statistics were calculated by

T-statistic for Average Abnormal Returns:

$$t_{AAR} = \sqrt{N} \frac{AAR_t}{S_{AAR}} \text{ } \mathbf{(13)}$$

where

Variance of Average Abnormal Returns of *M* Stocks Over *N* Days:

$$S_{AAR}^2 = \frac{1}{M-2} \sum_{t=1}^{N} (AAR_t - \overline{AAR_t})^2 \text{ } \mathbf{(14)}$$

with *M* being the number of matched returns per event (i.e. number of stocks) and *N* is the number of events.[16]

Procedure

The window for the historical data was chosen to be 03/05/2008-2/17/2009, which allowed for ~350 days of trading historical before the first event in Table 1, as per the literature. Before importing into Stata, closing prices were converted into rates of return (again, as is common practice for the event methodology across the literature).[17] A do.file was created for the

[16] Muller, Simon D. *"Significance Tests for Event Studies"* https://www.eventstudytools.com/significance-tests (accessed March 28, 2017)
This was used as a reference for conducting significance tests for *AAR*'s.
[17] After conducting this event-study on returns derived from closing stock prices, the process was replicated using trading volume as a metric for market reactions. The results are exhibited in Table 7. Here, AAV is average abnormal volume.

following steps:[18] First, historical data for the all stocks listed in Table 3 was imported into Stata. After importing the individual spreadsheets per industry, daily stock returns were regressed over the ~350-day pre-event window against the overall market returns (in this case, the market being "GSPC", or the S&P 500). Then, the expected return for stock *i* was calculated using the market model

$$(1)\ E[r_{it}] = \alpha_{i,m} + \beta_{i,m} r_{m,t},$$

where $\alpha_{i,m}$ and $\beta_{i,m}$ were obtained from the previously mentioned regression, and $r_{m,t}$ was the overall market return on the date of interest (Table 1) found in the dataset. Finally, this value was compared to the actual return of stock *i* on the same day, giving

$$(4)\ A[r_{i,t}] = E[r_{i,t}] - r_{i,t}.$$

This process was repeated for the five different events of interest entailing information releases about the ARRA, which involved changing the corresponding pre-event window.[19] Once all 5 abnormal returns were calculated, the procedure was repeated for the remaining stocks in the industry portfolio. The final output was a list of 5 abnormal returns per stock, one each for the events listed in Table 1. An average was taken of the absolute values of these abnormal returns, across the 5 events, illustrated in Table 4. The abnormal returns, subscripted 1-5, correspond to the marked events in Table 1.

[18] A more detailed breakdown of the do.file code is given here: a *foreach* loop regressed stock *i* against the market over a pre-event window. These regression coefficients $\alpha_{i,m}$ and $\beta_{i,m}$ were stored as variables in Stata to generate a $E[r_{i,t}]$ variable. This process was repeated for the next 4 events in Table 1, until the loop moved onto the next stock in the portfolio. A second *foreach* loop then averaged these abnormal returns across stocks, giving the 5 AAR's per event, for each respective industry.

[19] For example, the ~350-day window used to calculate "normal" stock returns before event [1] in Table 1 is a different window than the one used to calculate the "normal" stock returns before event [2] in Table 1. However, because events [2],[3],[4], and [5] are fairly close in proximity, it's difficult to say whether shifting the pre-event window for each new event (thus capturing the abnormal returns associated with events [2]-[4] in the "expected returns" regression for [5]) may lead to some issues in modeling expected return; this in turn affects the AAR values.

There was also a motivation to look at abnormal returns of steel stocks *after* the event window (following event [5]), from the behavior of particular steel stocks after the official enactment of the bill, illustrated in Diagram 1. Thus, a separate do.file was created to obtain *AAR*'s for the steel industry, following almost identical code as mentioned in footnote 19. Here, we use

$$(3)\ AAR_t = \sum_{i=1}^{n} A[r_{it}]$$

to calculate average abnormal returns These *AAR's* were then summed across the 53 individual daily observations within this window to obtain *CAAR* (across stocks) for the 2 month period following the enactment of the ARRA and the "Buy American Provision".[20] These *CAAR'*s were then averaged across stocks to obtain *ACAAR* (Table 4).

V. Results

Abnormal returns to information regarding the American Recovery and Reinvestment Act varied between the three selected industries. In the Energy Sector, average abnormal returns varied between -1% and +7% across the event window, the with the largest abnormal return coinciding with the official enactment of the bill on February 19, 2009 (Table 4). Notably, there was a +6.7%. abnormal return to the final signing of the bill into legislation, illustrating the confidence felt in the energy industry regarding the ARRA. Two weeks' prior, an article was published by over 200 economists protesting the bill; as a result, we notice a significant, negative abnormal return of -3.1% in the energy sector. Noting the breadth and granularity of policies

[20] The Buy American Provision required all government projects (broadly defined) that involved construction utilizing steel to purchase these materials solely from American manufactures. As expected, this type of industrial policy would increase the price of steel assets for a proceeding period.

regarding energy in Table 2, the abnormal returns for energy were significantly higher than those for telecom, but not steel. The policy tools for the latter two industries took the form of trade protectionism and a pure cash injection, while the policy targeting the energy sector encompassed a far broader range: loan guarantees, R&D subsidies and direct funding, and cash opportunities for technological upgrades within the industry. The specific support of green and efficient energy (Table 2) by the Obama administration may have had a negative impact on some energy stocks included in the portfolio - investors in Exxon, Shell, and Chevron may have seen these policies as negatively impacting these firms, while the majority of the energy firms analyzed evaluated the policies in a positive manner. This may have pulled down the magnitude of abnormal returns for the energy sector in comparison with steel, which is a less diverse industry.

Average abnormal returns in telecom were only between -4% and +0.9% across the events; this is most likely due to the low impact of the industrial policy targeting telecom; the ARRA simply aimed to put $7.2 billion into completing broadband internet access across the United States. This type of policy was merely a guaranteed cash injection into the industry; which firms would be contracted in these deals was not guaranteed, however, possibly explaining the small abnormal returns associated with information regarding the bill. In fact, only one event return was statistically significant for telecom, when the WSJ issued an article anticipating the ARRA a year before its drafting.[21]

Steel stocks had average abnormal returns of +1.8% to +7.5% during the window capturing the negotiation and signing of the bill. However, as motivated by Diagram 1, the price

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[21] This negative abnormal return was most likely not due to the information release by the WSJ article, which did not mention telecommunications specifically being affected by a potential stimulus package, and was instead due to another event entirely.

of various steel stocks began to rise dramatically in the proceeding months after event [5]. An attempt to capture this as a *ACAAR* value was made, illustrated in Table 4. Initially, a negative value was obtained. This was most likely due to several stocks in the steel portfolio--generated arbitrarily from online sources-- being affected by the "Buy American Provision" in a negative way (though the firms may be traded in US markets, their manufacturing plants may be foreign and thus banned by the bill). In the current version of this paper, I combed through these stocks and identified which in the portfolio were not correctly categorized as U.S. based steel stocks. Once the cleaning process was complete, regressions were re-run and a positive *ACAAR* value of +8.2% was obtained for the ~3-month period following the signing of the "Buy American Provision" into legislation.

Combing through the selected stocks to identify which truly belonged in the portfolio and which would not be affected by the ARRA at all was integral to obtaining clear results. As mentioned previously in the paper, autocorrelation of error terms may result in t-statistics that are "too good", which had been the initial case when stock portfolios for the event-study had been almost 5 times as large and included international firms. In this previous version, all average abnormal returns across the 3 industries and 5 events were significant at the 0.1% level, which would imply a perfect capturing of abnormal returns in a large window of speculative events--the theories detailed throughout the literature suggest that this rarely can occur. I first hypothesized that this was due to the issue with calculating average abnormal returns across stocks within the same industry, which will indeed have correlated error terms, according to King (1966). Before cleaning the data, a plan to implement the statistical methods cited in Jaffe (1974), Mandelker (1974), and Brown & Warner (1984) to try and adjust for autocorrelation was made. The authors of these two papers make a note that several assumptions must be relaxed to obtain statistical values that seem "normal", and whether or not those assumptions are necessary for drawing meaningful conclusions is particular to each event-study. Decreasing the portfolio size to the "key[22]" firms within industries and averaging abnormal returns from a single event (rather than 5 in close proximity) may also provide clearer results, as the pre-event window will be clean of events hypothesized to cause abnormal reactions (as mentioned in King 1966).[23] This method may result in a starker difference in the $E[r_{i,t}]$ and $r_{i,t}$ values, increasing abnormal returns. Indeed, removing international firms from the portfolios and decreasing the portfolio size eradicated the autocorrelation issue.[24]

VI. Conclusions

In this particular event-study, results were both statistically significant and fit a proper narrative regarding expected reactions to the ARRA. However, event-studies have several intrinsic strengths and weakness as a method for analysis. They are able to provide insight on immediate reactions to information--in contrast to secondary data analyses that come years later and are effected by hindsight--but come with statistical complications that may outweigh these benefits. Additional complications come into play when re-applying this tool from a microeconomic setting to its macroeconomic counterpart. Typical event-studies will focus on a sole event and stock reactions; when looking at industrial policies, information that would illicit

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[22] "key" may be defined as 5 largest firms by market share, profits, or other types of valuation

[23] In this current methodology, the returns of stocks for the pre-event window of event *j* incorporate returns from events *j*-1, *j*-2..., *j*-k. This was unavoidable, as the events in question are close enough that they become captured when regressing historical data. This could be avoided by having the pre-event window stay constant for events 1-*j*, but the drawback here is the increased length of time between the end of the pre-event window and the actual event in question.

[24] Though many firms were removed, some firms were added to improve the analysis. For example, after re-examining how exactly the ARRA affected the energy sector, four bio-fuel firms were added to the energy portfolio to help capture market reactions to the bio-fuel research subsidies that were part of the policy

market reactions (the basis of our use for event-studies as an analysis tool) is spread out over a long period of time. This larger event window--preliminary media speculation, press releases, bill drafting sessions, final signing date--complicates the modeling of expected returns. Questionable expected return values will cascade issues throughout the remainder of the event-study methodology, rendering it ineffective.

If information regarding these policies could be boiled down to a single event, this issue may possibly be resolved. With this being the case, we still run into the issue of multicolinearity and autocorrelation of error terms between analyzing multiple stocks within the same industries. This is an issue that may be avoided using advanced statistical techniques mentioned throughout the literature, but ultimately is inherent to evaluating policies at an industrial level, which can only be represented by a portfolio of stocks effected by the same exogenous and endogenous variables.

Though this study was able to produce indications of stock market reactions to a package of industrial policies, the validity of these quantitative results may still be in question based on design complications. A theoretical issue with results, even after robust corrections, may still exist. That is, that the reactions of investors may not be the best proxy for evaluating industrial level policies; there is an argument to be made about whether or not the welfare of players in financial markets is equivalent to the welfare of the targeted industries, and the macro-economy as a whole. Though financial markets are continuing to make up a larger share of U.S. GDP, industrial policy makers are aiming to foster growth in key sectors--not necessarily increase firm profitability and investor pay off-- and must be evaluated on these premises. Therefore, the applicability of event-studies as a methodology for evaluating industrial policy remains questionable.

Though this study assessed a method for policy evaluation and not policies themselves, some conclusions may still be drawn on implications for future government involvement in particular industries. The event-study conducted illustrated positive evaluations of the ARRA by the energy, telecommunications, and steel industries; the implication here is that, given a similar economic context to that of the 2008 recession, research subsidies, tax exemptions, cash injections, and trade protectionism may again be successful in fostering industry growth. The event-study's positive assessment of the ARRA's energy policy is reinforced by the growth in the renewable energy sector following its legislation, exhibited in Table 5. An analogous story in the steel industry occurs when we examine both the increase in steel exportation following the drop during the 2008 recession (Table 6) and the increase in overall steel production (Diagram 2); these results coincide with the confident predication made by the market in our event-study. The implications here highlight the success of trade protectionism--specifically in steel--as an industrial policy following a recession. The implications regarding policy evaluation (not just policies themselves) should also be noted. This study concludes the event-studies, though they offer powerful insight, may not have a place in the policy-maker's evaluation toolbox. However, there is still a need to push evaluation methods that are protected from issues of hindsight and misattribution; a $10 billion injection to increase employment in an industry by X% is not a good policy if $5 billion would have achieved the same result, if not better. These "what if" questions cannot be answered by simply looking at macroeconomic data after a policy has had time to influence the industry, whereas more instantaneous methods of evaluation (event-studies) or more predictive methods (quasi-natural experiments) may be able to circumnavigate this issue.

Future research will examine alternative methods to event-study implementation, as mentioned early. Assessing based on *CAAR* values, for example, rather than looking at individual

*AAR*s at different points throughout the policy's event period, may give a different story; we saw that the steel market's reaction to the industrial policy manifested in proceeding months, rather than on any given day during the event window. Assessing a different set of industrial policies (different policy mechanisms, targeting alternate industries to those studied in this version) may also shed more light in generalizing the appropriateness of this methodology. Substituting changes in trading volume, rather than return rates, can also be implemented in further research; this will help illustrate speculation around policies during their drafting periods. Finding ways to more clearly identify a single "event" regarding industrial policies will help strengthen the power of the event-study in exposing abnormal returns; looking through more sources to determine which policy events generated the most "buzz" among investors will provide the best event to examine.

The event-study methodology is impressive in both its clever capturing of market sentiment and its highlighting abnormal behavior in system as complex as financial markets. Since its creation in 1969, it has provided economists and financial experts with a means of power analysis; however, the strength and reliability of an event-study is contingent on its proper implementation, both methodologically and theoretically. With these caveats in mind, policy makers may be cautious to utilize an event-study when assessing industrial policy, and must continue to explore and expand their evaluation tool-set.

VII. Appendix

Table 1

Date	Official Enactment		Speculative Dates		Notes
	[5]	02/17/2009	[1]	11/25/2008	WSJ Article: "Obama Signals Big Stimulus Plan"
				01/09/2009	Speaker of House Nancy Pelosi announces Congress and Obama admin have begun working on ARRA
			[2]	01/10/2009	Obama Admin releases preliminary report on the impact of the ARRA on jobs on based on recovery packages being considered
				01/23/2009	Speaker of House Nancy Pelosi announces bill is on track to be signed by president
			[3]	01/26/2009	House of Reps introduce version of bill
				01/28/2009	WSJ and NYT publish list of ~200 economists against bill
			[4]	02/08/2009	Letter to Congress written by ~200 economists in favor of bill
				02/11/2009	NYT Article: "Deal Reached in Congress on $789 Billion Stimulus Plan"

Table 2

Policy Type	Industry	Notes
Stimulus Package	Communications, information, and security technologies	$7.2 billion for complete broadband and wireless Internet access
	Energy efficiency and renewable energy research and investment	$6 billion for renewable energy and electric transmission technologies loan guarantees
		$2 billion for manufacturing of advanced car battery (traction) systems and components
		$800 million for biofuel research, development, and demonstration projects.
		$602 million to support the use of energy efficient technologies in building and in industry
		$602 million to support the use of energy efficient technologies in building and in industry
		$400 million for electric vehicle technologies
		$190 million in funding for wind, hydro, and other renewable energy projects
		$115 million to develop and deploy solar power technologies
		$42 million in support of new deployments of fuel cell technologies
	Iron / Steel	"Buy American provision"

Table 3

Energy	Steel / Iron	Telecom
XOM Exxon Mobil Corporation	**AKS** AK Steel Holding Corporation	**T** AT&T Inc.
RDS-A Royal Dutch Shell plc	**NUE** Nucor Corporation	**S** Sprint Corporation
CVX Chevron Corporation	**X** United States Steel Corporation	**VZ** Verizon Communications Inc.
WMB The Williams Companies, Inc.	**STLD** Steel Dynamics, Inc.	**TMUS** T-Mobile
BP BP p.l.c.	**WOR** Worthington Industries	**TWX** Time Warner Inc.
SLB Schlumberger Limited	**AME** AMTEK, Inc.	**CTL** Century Link
APC Anadarko Petroleum Corporation	**CMC** Commercial Metals Company	**FTR** Frontier Communications
ENB Enbridge Inc.	**RS** Reliance Steel & Aluminum Co.	
PSX Phillips 66	**CRS** Carpenter Technology Corporation	
COP ConocoPhillips		
EOG EOG Resources, Inc.		
SU Suncor Energy Inc.		
CEO CNOOC Limited		
OXY Occidental Petroleum Corp		
HAL Halliburton Company		
VLO Valero Energy		

Table 4

Event	Date	Description
1	11/25/2008	WSJ Article: "Obama Signals Big Stimulus Plan"
2	01/10/2009	Obama Admin releases preliminary report ARRa's impact on jobs
3	01/28 /2009	WSJ and NYT publish list of ~200 economists against bill
4	02/08/2009	Letter to Congress written by ~200 economists in favor of bill
5	2/17/2009	ARRA is officially passed
Energy		
	AAR	**Standard Deviation**
1	-0.011*	0.014
2	0.035***	0.014
3	-0.031***	0.014
4	-0.010	0.014
5	0.067***	0.014
	Observations	350 Returns per 16 stocks
Telecom		
	AAR	**Standard Deviation**
1	-0.041***	0.014
2	-0.002	0.014
3	-0.004	0.014
4	0.007	0.014
5	0.009	0.014
	Observations	350 Returns per 7 stocks
Steel		
	AAR	**Standard Deviation**
1	0.056***	0.020
2	0.056***	0.020
3	0.025***	0.020
4	0.019**	0.020
5	0.076***	0.020
	Observations	350 returns per 9 stocks
	ACAAR	0.082

Table 5

Year	Renewable energy consumption (% of total final energy consumption)
2007	6.400
2008	6.304
2009	6.846
2010	7.354
2011	7.505
2012	8.164
2013	8.481
2014	8.928
2015	8.913

Source: http://data.worldbank.org/indicator/EG.FEC.RNEW.ZS?locations=US

Table 6

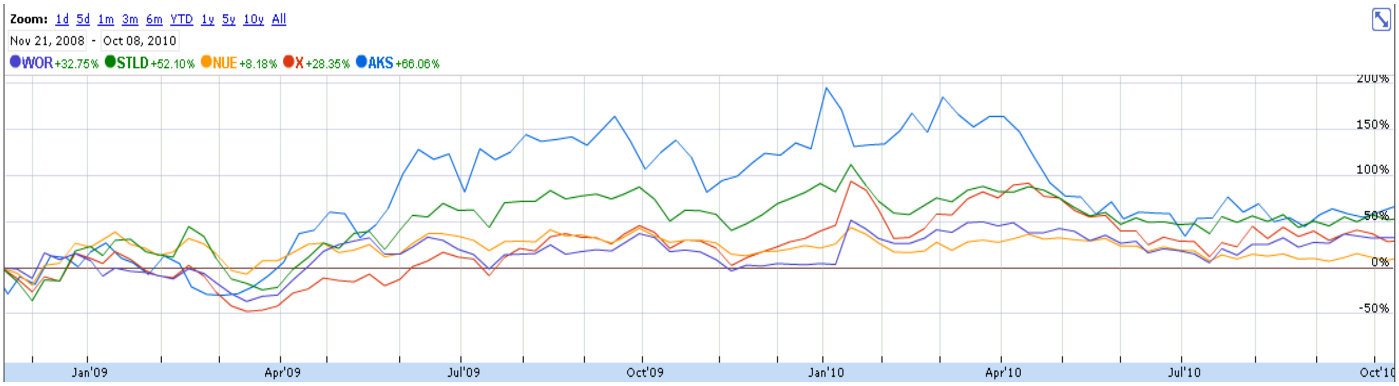
Year	Ores and metals exports (% of merchandise exports)
2006	3.619
2007	3.873
2008	4.127
2009	3.531
2010	4.086
2011	4.205

Source: http://data.worldbank.org/indicator/TX.VAL.MMTL.ZS.UN?locations=US

Table 7

Event	**Date**	**Description**
1	11/25/2008	WSJ Article: "Obama Signals Big Stimulus Plan"
2	01/10/2009	Obama Admin releases preliminary report ARRa's impact on jobs
3	01/28 /2009	WSJ and NYT publish list of ~200 economists against bill
4	02/08/2009	Letter to Congress written by ~200 economists in favor of bill
5	2/17/2009	ARRA is officially passed
Energy		
	AAV	**Standard Deviation**
1	2.387**	0.800
2	0.552	0.800
3	-0.500	0.800
4	1.041***	0.800
5	-5.461***	0.800
	Observations	350 Returns per 16 stocks
Telecom		
	AAV	**Standard Deviation**
1	0.097	0.014
2	0.244	0.014
3	0.923***	0.014
4	0.284	0.014
5	-2.068***	0.014
	Observations	350 Returns per 7 stocks
Steel		
	AAV	**Standard Deviation**
1	-0.240	0.020
2	0.716	0.020
3	1.478	0.020
4	0.751	0.020
5	-1.805	0.020
	Observations	350 returns per 9 stocks
	ACAAV	0.163862

Diagram 1



Source: Google Finance

Diagram 2

Source: World Steel Association

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