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We are grateful to the following faculty members for reviewing papers this year: Stephen Trejo, Gerald Oettinger, Richard Murphy, Valerie Bencivenga, Michael Sadler, Shalah Mostashari, Helen Schneider, and Patricia Maclachlan, as well as graduate students Shreyas K.S. and Collin Hansen.

The journal has received significant support and encouragement from the Department of Economics at The University of Texas and we would like to thank Dr. Jason Abrevaya, Department Chair, and economics advisor Jana Cole for their help and advice throughout the year. Additionally, we would like to thank Nickolas Nobel from the Landmarks office for his assistance with the cover.

Lastly, we would like to thank the College of Liberal Arts, Omicron Delta Epsilon, and the generous donors who provide us with significant funding for this project each year.

Thank You,

Editorial Team

The Developing Economist

An Undergraduate Journal of Economics

Dear Reader:

Publishing this journal every year is a long and arduous process from beginning to end. This year–our third–was no different. With over thirty submissions and multiple rounds of editing, five papers were selected as representation of excellent undergraduate research in economics. These papers' authors, from various schools across the country, collectively put in countless hours of research, writing, and editing, and we thank them for bringing their hard work to our attention.

Undergraduate research is a cornerstone of a well-rounded and fulfilling university education. As someone who has been involved with this journal since its first edition, I am encouraged by the interest and support of students and faculty alike in our endeavor. We hope to continue promoting research at the undergraduate level, and that the papers published in *The Developing Economist* help inspire future researchers for years to come.

Thank You,

Daniel Chapman
Editor-in-Chief
President, Omicron Delta Epsilon at UT

A Note From
The Director of Undergraduate Studies

I am pleased to have been asked to write an introduction to the third volume of *The Developing Economist*, the undergraduate research journal in the Department of Economics at The University of Texas at Austin. This journal was founded by our Omicron Delta Epsilon chapter, whose membership saw the need for an outlet for undergraduate research in economics. Current ODE members manage the entire editorial and production process, including the soliciting of reviews of submitted papers, accepting papers for publication, and having the volume printed. The content and professional design of previous volumes have been impressive, and the faculty of our department as well as fellow students are looking forward with anticipation to this year's volume. *The Developing Economist* is one of a very small number of undergraduate research journals in existence.

Over the years, the faculty has worked with many undergraduates on their honors theses, which provide one important opportunity for undergraduates to formulate and conduct original research. During the research and writing process, the student increases their capacity for formulating questions and developing methodologies for theoretical or empirical analysis. Being involved in this process is one of the most rewarding aspects of teaching, and from this vantage point, the value of undergraduate research is clear. That is why *The Developing Economist* is a great contribution. By providing an outlet, this journal encourages and supports high quality undergraduate research and the development of skills, knowledge, and judgment during the research process. The Department of Economics congratulates ODE on production of the third volume of *The Developing Economist*. We hope that this journal will continue to play an important role in encouraging undergraduate research.

Dr. Valerie R. Bencivenga
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Executive Compensation at Credit Unions

Daniel Koslovsky

Abstract

This study is the first to examine credit union executive pay using compensation information from IRS Form 990. Using OLS, logistic, and tobit regression analysis to identify the determinants of base and bonus compensation of chief executive officers (CEOs), this paper finds evidence of misaligned incentives between CEOs and the institutions' member-owners– although caution is needed when trying to reach firm conclusions because of the limits of the study. Higher fees and lower dividend payments are costly to the credit union members, and yet are correlated with higher compensation for executives. On the other hand, improving the efficiency of the credit union through lower operating expenses has little to no impact on compensation at credit unions. Instead, financial performance indicators such as asset growth and net worth are positively related to higher pay. Other factors such as smaller boards or using a compensation consultant impact the bonus an executive receives.

I. Introduction

The purpose of this study is to examine the CEO compensation practices of credit unions. Specifically, I examine the factors that determine the base compensation for a credit union CEO, study what determines whether a credit union CEO will receive a bonus, and analyze the incentives credit unions provide for CEOs through bonus compensation. Extensive research has been done on executive compensation in for-profit firms. The banking sector in particular came under close scrutiny in the last decade for the role executive compensation structures may have played in the financial crisis. In comparison, nonprofit executive compensation has been studied much less. While there is a growing literature examining how nonprofits

incentivize their executives, the topic of executive compensation for credit unions has yet to be properly analyzed.

Credit unions play an important role in the US financial system and conduct business in a unique environment. At the beginning of 2015, there were over 6,300 credit unions in the United States, with total assets amounting to about $1.173 trillion. While the total assets in credit unions are over ten times smaller than the banking sector, they still constitute a significant portion of the US financial system. Moreover, credit unions provide a unique setting to examine executive compensation that has so far gone unexplored. Credit unions operate at the intersection of the nonprofit sector and the banking sector. As a nonprofit, credit unions are subject to the same advantages and constraints as all nonprofits—they are tax exempt, they have a social mission that is principal over making profit, and their compensation is subject to the non-distribution constraint, meaning income cannot be distributed to managers. Unlike most nonprofits, credit unions receive all of their revenue from commercial activities and are in direct competition with for-profit organizations, banks. Determining the incentives used by credit union directors will provide us with insight to what credit unions set as their objectives and how that compares to their expected mission. Additionally, the results for credit unions have broader implications as they could be used to make generalizations about all commercial nonprofits.

I use 2013 compensation data from Schedule J of IRS Form 990, combined with annual 5300 Call Report data collected by the National Credit Union Administration, to regress base and bonus compensation of credit union CEOs and presidents (from here on just referred to as CEOs) against credit union characteristics. I find that the base compensation received by credit union CEOs is significantly determined by financial performance. Further, CEOs are more likely to receive a bonus if their credit union employs a compensation consultant. Finally, evidence suggests that credit unions disincentivize better member services.

The rest of the paper proceeds as follows. Section II provides background and a literature review of previous studies of executive compensation. Section III details the data that is used in this study and the econometric models for base and bonus compensation are introduced. Section IV provides descriptive statistics of the data. Section V presents the results of the equations and offers analysis. Section VI concludes.

II. Background and Literature Review

For-Profit Executive Compensation

Executive compensation has been given extensive attention in the for-profit literature. It is typically studied under the paradigm of the principle-agent model, in which a company's board is the principle and the executives are the agents. The incentives of the board and the executive rarely match; the board seeks profit maximization for the firm, while the executive possesses rent-seeking incentives. This incentives mismatch incurs agency costs onto the board, which typically manifest in the compensation given to the executive, but also can take the form of welfare loss (Jensen & Meckling, 1976). To incentivize the executive to take actions that are in the best interest of the firm, the board's most effective tool is the way in which they compensate him/her, typically through bonuses that promote firm profit and discourage rent-seeking.

Following the most recent financial crisis, possible incentive misalignment in financial firms resulting from executive compensation structure was studied to see what role it may have played in precipitating the crisis. Prior to the recession, risky investments, most notably mortgage-backed securities, were increasingly made in order to maximize short-term profits. The failure of these investments catalyzed the recession. Once the market crashed, many economists began to investigate how compensation structures affected risk-taking in financial firms. One school of thought believes that executives were not overly incentivized towards risk-taking because the losses they assumed following the crash wiped out any short-term

gains from extra risk (Fahlenbrach & Stulz, 2011). Bebchuk, Cohen & Spamann (2010) counter this argument by showing that, although executives at Bear Stearns and Lehman Brothers lost all or almost all of the value on the bonus stock options at the height of the crisis, they still cashed out enough of their bonus options in the years prior to have made a lucrative amount of money overall. Thus, it is possible to conclude that the compensation structures in place before the crisis incentivized executives to make overly risky investments because it was in their best personal interest.

Looking at long-term trends, executive compensation among for-profit firms has risen sharply in the last few decades. As Frydman and Saks (2008) point out, the real value of executive compensation was strikingly flat from the end of WWII into the mid-1970s regardless of aggregate firm growth, firm performance, or overall economic performance. They use long-term data on executive compensation to challenge the theories that the rise in pay is directly tied to performance or growth in firm size. Instead, they posit that the increase in executive compensation is a result of either increased board diligence or changes in social norms that made higher relative incomes more acceptable.

Nonprofit Executive Compensation

Nonprofits offer a notable contrast in executive compensation structures. Just like in for-profit firms, agency problems with executives must be addressed by the board. Distinct from for-profit firms, nonprofits have a limit to how much they can incentivize their executives because they are subject to the non-distribution constraint, which prohibits nonprofits from distributing net earnings to anyone who oversees the organization. Additionally, nonprofits must balance maintaining the financial soundness of the organization with a social mission that is often difficult to quantify.

Agency theory plays an important role in the nonprofit sector as well, but the theoretical work is much less developed. Within the existing work, there are two views. One side of the

arguments suggests that that principle-agent relations in nonprofits are more problematic than in for-profits because of the difficulty in defining organizational objectives and ownership issues arising from the absence of shareholders. These issues make it more difficult for nonprofit boards to effectively control executives. In contrast, others believe that large, independent donors can effectively monitor and control management, thus reducing agency costs (Caers et al. 2004). But, both sides of the argument acknowledge that executive compensation plays major a role in controlling for agency problems.

The nonprofit executive compensation literature has identified a few key factors in determining a CEO's pay. Frumkin and Keating (2001) find a weak link between the amount of executive compensation paid out and the performance measures of improved fundraising results or better administrative efficiency. However, they attribute the weakness of the link to the non-distribution constraint as organizations with freer cash flows pay their CEOs significantly higher wages. Baber et al. (2002) and Hallock (2002) suggest that for charitable nonprofits changes in compensation are linked to charitable output. The former provides evidence that pay is significantly and positively associated with the level changes in spending on the organization's objectives. While the latter finds that a higher proportion of expenses going towards the nonprofit's mission leads to higher compensation. Evidence in support of nonprofit CEO compensation being related to financial performance is found by Sedatole et al. (2014), who find that increases in revenue and change in net assets are associated with higher pay. Finally, Balsam & Harris (2015) use IRS Form 990 data to examine nonprofit executive compensation's relationship to performance and found that bonus pay is positively associated with profitability, available cash, and the use of compensation consultants, and negatively associated with donations and charitable nature. In sum, it appears that nonprofit executive compensation is usually linked to the achievement of the organization's social mission and the financial sustainability of the nonprofit.

In addition to social output and performance, board oversight appears to play an important role in determining executive compensation. The logic behind the relationship between board oversight and executive compensation is that more oversight means closer monitoring of CEOs by the board, which reduces the amount the board must pay to combat agency problems. Moreover, board size matters as well—a bigger board is believed to broaden the focus of an organization, making the incentives provided in the form of compensation less concentrated and therefore smaller. Balsam & Harris (2015), Hallock (2002), and Aggarwal et al. (2011) all examine the relationship between the makeup of nonprofit boards and executive pay. Balsam & Harris (2015) provide evidence that board approval reduces bonus pay, but that the size of the board has no effect. Hallock (2002) finds mixed results for board size and executive pay; he shows that the number of paid directors is significantly and negatively related to compensation, while the number of unpaid directors is positive, but not robust. Lastly, Aggarwal et al. (2011) discover strong evidence that nonprofit board size is negatively associated with managerial incentives, particularly for commercial nonprofits. While the literature is mixed on whether board oversight definitively plays a role in compensation, it is clear that if a relationship exists it is a negative one.

Credit Union Executive Compensation

Credit unions provide an interesting environment to study executive compensation because they exist at the intersection of banks and nonprofits. Banks want to maximize profit, so they incentivize CEOs using performance and risk measures. Nonprofits exist to maximize social output, so they incentivize executives to spend more on their social mission, reduce administrative expenses, and ensure the financial viability of the organization. Credit unions are nonprofit organizations; their *raison d'etre* is to serve the financial needs of their members (Cargill et al. 1980). But, they also face stiff competition for business from for-profit banks and rely solely on the com-

mercial revenue they generate from that business for income. This seemingly creates a paradox for credit unions, they must balance incentivizing member services with financial performance.

While there has not been any previous academic work examining executive compensation in credit unions, some attention has been paid to nonprofits who derive almost all of their income from commercial revenue. Aggarwal et al. (2011) find that such organizations, termed commercial nonprofits, are larger than traditional nonprofits and pay their executives more. Moreover, commercial nonprofits typically have a smaller organizational focus, smaller board size, and stronger managerial incentives. Change in revenue is found to be significantly positively related to compensation, but not more so than traditional nonprofits. Aggarwal et al.'s findings suggest that credit union compensation will be similar to nonprofit compensation, but the scope of their analysis between commercial nonprofits and traditional nonprofits is fairly limited. Thus, extensions of this the commercial nonprofit and credit union literature are necessary to expand our understanding of these sectors.

III. Data and Model Specifications

Data

To examine executive compensation for credit union CEOs I pulled compensation data on all state chartered credit unions with over $500 million in assets at the end of 2013 from IRS Form 990 Schedule J. IRS 990 forms were found at the National Center for Charitable Statistics (NCCS) and using Foundation Center's 990 finder. The sample was limited to state chartered credit unions because federally chartered credit unions are not required to file annual IRS 990 forms. Data is used from the end of 2013 because it is the most recently available. There were 223 state chartered credit unions with over $500 million in assets at the end of 2013. Of the 223 credit unions, a 990

form could not be found for five of them[1], no Schedule J was provided for four of them[2], compensation data was only available upon request for two of them[3], and no CEO was listed for three[4]. This left me with a sample of 209 credit unions and 210 CEOs. Navy Army Community Credit Union changed CEO's in the middle of 2013, so they had two CEO's listed on their call report. Since it appears that both CEO's were employed by the credit union before and after the change, I assume that the compensation data in the 990 form reflects a full year and leave both executives in the dataset.

The compensation data were then merged with the quarter four 2013 Call Report data from the National Credit Union Administration. Average credit union wage data for the county in which a credit union is headquartered uses data from the Bureau of Labor Statistics.

Empirical Model

I use OLS regression analysis to estimate the effects of different credit union characteristics on base compensation. A logistic regression model is used to identify what determines whether a CEO receives bonus compensation. Lastly, to estimate the amount of bonus compensation I use a tobit model. Robust standard errors are used in each equation in order to control for heteroscedasticity.

Equations (1)-(3) estimate the natural log of base compensation, the probability of a CEO receiving bonus compensation, and the natural log of bonus compensation received:

$$\log Y_j = a + \mathbf{bP_j} + \mathbf{cM_j} + \mathbf{dX_j} + \varepsilon_j \tag{1}$$

$$Pr(B_j = 1) = a + \mathbf{bP_j} + \mathbf{cM_j} + \mathbf{dX_j} + \mu_j \tag{2}$$

[1]Max Credit Union, General Electric Credit Union, LAFCU, Park Community Credit Union, and University of Michigan Credit Union

[2]Texans Credit Union, Weokie Credit Union, Self-Help Credit Union, and Schlumberger Employees' Credit Union

[3]DFCU and Lake Michigan Credit Union

[4]School Employees Credit Union, Melrose Credit Union, and Triangle Credit Union

$$\log B_j = a + \mathbf{bP_j} + \mathbf{cM_j} + \mathbf{dX_j} + \eta_j \tag{3}$$

Each equation contains matrices $\mathbf{P_j}$,$\mathbf{M_j}$, and $\mathbf{X_j}$.

Matrix $\mathbf{P_j}$ is composed of the credit union financial performance variables net income growth, loan growth, asset growth, net worth, non-interest expenditures as a share of total assets, and return on assets. The growth rates measure the change in the variable from the fourth quarter of 2012 to the fourth quarter of 2013. Each financial performance variable provides a way of measuring distinct indicators of a credit union's financial health. The connections between the empirical variable and the conceptual measures are fairly straightforward; net income growth measures the growth in profitability, loan growth measures the growth of new business for a credit union, asset growth measures a credit union's growth in size, net worth measures the level of size, non-interest expenditures as a share of total assets is a measurement of the operating costs of a credit union, and return on assets tells us how well a credit union's assets are performing. Based on the existing non-profit executive compensation literature, we should expect to see net income growth, loan growth, asset growth, net worth, and return on assets to be generally positive. Reverse causality is a concern for net worth because a credit union with a higher net worth will likely have more money to provide its CEO as compensation. In contrast to the other financial performance variables, non-interest expenditures as a share of total assets is expected to be negative. This variable measures the operating costs of a credit union. From what we know from the non-profit literature executives who can reduce the operating costs of their organization will be rewarded. Between the five financial performance variables, the ones we should expect to be largest in magnitude based on previous executive compensation studies are non-interest expenditures as a share of total assets and loan growth.

$\mathbf{M_j}$ contains the variables that measure the member services provided by a credit union: member growth, share of income from fees, and dividend yields. We should expect mem-

ber growth and dividend yields to have positive coefficients. Credit unions have incentives to grow their membership because more members typically means more business, both in deposits and loans. Additionally, if a credit union's membership is growing, then that could be a proxy for good services attracting more members. As for dividend yields, the conceptual link is quite straightforward: members are going to prefer credit unions with higher dividend yields because that means they are getting a higher return for their deposits. Finally, the share of income from fees is expected to be negative—*ceteris paribus*, the more a credit union charges in fees, the worse for its members. Out of the three member service variables, dividend yields is expected to have the greatest magnitude because it is the most direct way of measuring credit union member benefits.

Finally, $\mathbf{X_j}$ is made up of a number of miscellaneous variables that may have an effect on executive compensation, including whether a credit union uses a compensation consultant, how many board members a credit union has, the gender of the CEO, and, in order to control for differences in cost of living across the country, the average salary of the credit union employee in the county in which a credit union is headquartered. The literature tells us to expect credit unions that use a compensation consultant to provide a higher bonus and for a credit union with more board members to give lower bonus compensation. There is no literature I surveyed that examined gender in non-profit executive compensation pay, but based on wage trends in the US the expected sign is positive, meaning higher compensation for males.

In equations (1) and (2), the CEO for East Texas Professional Credit Union was left out because the 990 form listed his base compensation was over $2.8 million and there are no other CEOs who receive even $1 million. Therefore, this observation is either an error or a major outlier. Similarly, the three CEOs whose ratio of bonus compensation received to credit union assets in millions exceeded 790 were also excluded. The cutoff was set at 790 because the ratio of the three CEOs who

are greater than 790 are over twice as large as the next closest CEOs ratio, making it a clear break in the data that are either outliers or mistakes in the 990 form.

IV. Descriptive Statistics

Figure 1 shows the mean and median for CEOs by pay type:

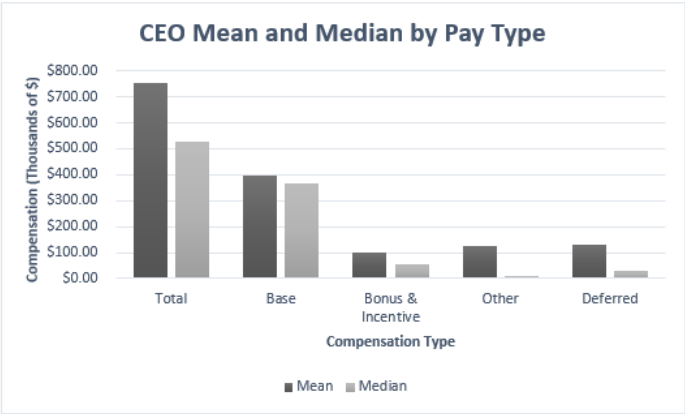


Figure 1—CEO Mean and Median by Pay Type

Definitions for pay type were taken from IRS Form 990 instructions. Total compensation is defined as the sum of base, bonus, other and deferred compensation. Base compensation is made up of primarily salary, but also 401k deferrals and health benefit plans. Any bonuses or compensation given out as incentive pay falls under bonus compensation. Severance or change of control payments, tax gross-ups, vacation/sick leave, life insurance, and goods or services provided (legal, housing, travel, etc.) are all classified as other compensation. Finally, deferred compensation is any income deferred that is not taxable in the current period.

Mean and median total compensation for CEOs in the sample were $761,257 and $530,171, respectively. Base compensation had a mean of $400,601 and a median of $368,237. While the mean and median for bonus compensation came out to $100,835 and $58,493.

As can be seen in Figure 1, base compensation accounts for a large share of CEO pay. Bonus compensation is the only other pay type with a median above $50,000. This study focuses on base compensation and bonus compensation because they are the most useful for learning about credit union compensation practices. Other and deferred compensation vary greatly between credit unions and over time within credit unions. Moreover, they are not associated with size or performance of the credit union. They are often payouts of retirement or other benefit plans that had been accumulating for a number of years. Total compensation is not examined because of the outsized influence outlier values of other or deferred compensation may have on the data.

Moving on to credit union characteristics, table 1 provides the mean, median, maximum, and minimum values for the characteristics examined in equations (1)-(3).

Table 1—Summary Statistics

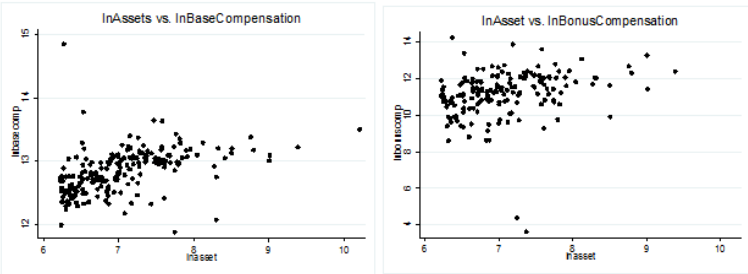
	Mean	Median	Max	Min
Total Assets (millions)	$1,541.34	$1,005.48	$27,107.07	$501.03
Net Worth (millions)	$160.0	$105.0	$1,990.0	$42.9
ROA	88.73bps	84.31bps	304.97bps	-15.15 bps
NIE	3.18bps	3.16bps	6.05bps	0.58bps
Fee income share	14.68%	13.66%	51.20%	0.86%
Membership	121,060	79,801	1,870,759	4,015
Asset Growth	5.61%	4.56%	34.63%	-3.20%
Net Income Growth	15.25%	-2.19%	1,935.10%	-223.50%
Loan Growth	10.29%	9.46%	53.15%	-9.80%
Member Growth	4.40%	3.95%	37.52%	-15.81%

The median credit union in the sample has approximately $1 billion in assets and 80,000 members. One surprising statistic presented in the table is that median net income growth fell by over 2%, yet the mean was positive.

Lastly, and unsurprisingly, base compensation and bonus compensation are very well correlated with asset size. Figure 2 shows the log of assets plotted against the natural logs of base

compensation and bonus compensation, respectively. The R-value of log assets vs. log base compensation is 0.44, while the R-value of log assets vs. log bonus is 0.267. To account for this stylized fact, I control for asset size for the rest of the study.

Figure 2 — Log of Assets vs. Log of Base Compensation



Sample Comparison

To ensure that the results found from the sample taken are applicable to credit unions that are not state chartered or are below $500 million in assets, I compare credit union characteristics of the sample to all state chartered credit unions, all nationally chartered credit unions, and nationally chartered credit unions above $500 million in assets. The results are displayed in table 2.

Table 2—Federal vs. State and Over $500 million in assets

	All Federal	All State	Federal>500m	Sample (~State>500m)
Number of Credit Unions	4,105	2,449	201	209
Total Assets (millions)	571,319	490,644	372,054	320,053
Asset size (millions) (median)	$19.10	$30.40	$984.60	$999.58
# of employees (median)	6	9.5	231.5	240.5
Compensation/Assets (median)	1.72%	1.73%	1.58%	1.60%
Asset growth (median)	1.40%	2.06%	4.30%	4.52%
Loan growth (median)	2.19%	2.86%	8.41%	9.43%
Net Worth (millions) (median)	$2.23	$3.70	$106.16	$104.89
ROA (median)	22.3bps	31.1bps	72.7bps	84.3bps
NIE (median)	3.57bps	3.58bps	3.13bps	3.10bps
Dividend Yields (median)	0.37%	0.30%	0.54%	0.31%
Fee Income Share (median)	11.36%	12.63%	13.47%	13.66%

As shown in the table, credit unions with over $500 million in assets perform much differently than credit unions on the whole, particularly in key measures such as asset growth, loan growth, and return on average asset. This is unsurprising, and suggests that the results of this study are only applicable to credit unions with over $500 million in assets. As for the comparison between federal and state credit unions with over $500 million in assets, the characteristics are quite similar. Noteworthy differences occur between loan growth and return on average asset, but particularly striking is the difference in dividend yields. In 2013, federally chartered credit unions provided 0.54% dividend yields, while the same number for state chartered credit unions was just 0.31%. These divergences do not necessarily reduce the saliency of the study, but should be kept in mind when the results are presented.

Beyond comparing credit union characteristics of the sam-

ple to all credit unions, it is useful to compare compensation characteristics of the sample to industry survey and banking sector data. Such a comparison is made below in figures 3.1 and 3.2.

Figure 3.1—Sample, Survey, and Bank median base salary

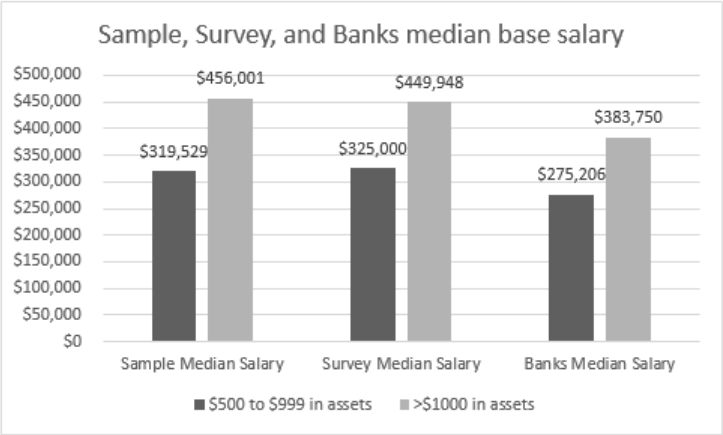
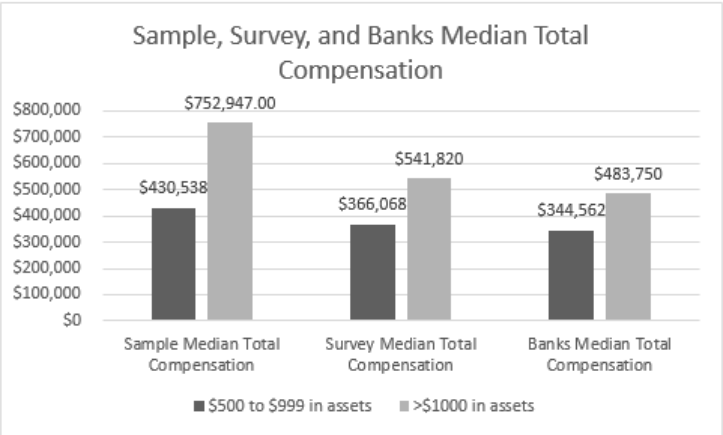


Figure 3.2—Sample, Survey, and Bank median total compensation



The survey I use was conducted by the Credit Union Executives Society in 2013. They polled 443 credit unions, 427 of which provided compensation information for their CEO.

The information on bank salary comes from the Independent Community Bankers of America and American Bankers Association compensation surveys.

Results for median salary (or base compensation) between my sample and the survey are remarkably similar. My sample of credit unions with assets between $500 and $999 million in assets has a median base compensation of $319,529, while the median for my sample of credit unions with over $1 billion in assets is $456,001. In comparison, the credit unions from the survey with $500 to $999 million in assets had $325,000 and the credit unions from the survey with over $1 billion have a median of $449,948. Such similarity suggests that the sample represents executive compensation for all credit unions over $500 million well. The difference between credit union base compensation and bank base compensation is also notable. Credit union CEOs appear to make about 15-20% more in base compensation than bank CEOs.

A comparison of total compensation is also provided, however an important caveat must be made regarding the total compensation statistics. What is defined as total compensation for the sample is different than the definition for the surveys. In the sample, total compensation is all compensation received, while in the surveys total compensation only includes cash compensation and excludes benefits, long-term incentives like stock options, and perquisites. This explains why the sample has a much greater total compensation than the survey and can also be used to illustrate the amount of non-cash compensation credit union CEOs receive. Additionally, it should be noted that bank CEOs receive a much greater proportion of their compensation in forms that would not be included in the survey and so a comparison between the sample total compensation and survey bank total compensation is not suitable.

V. Results & Analysis

The results to equations (1)-(3) are given in table 3:

Table 3- Regression Results

Variables	(1) ln Base Compensation	(2) Received Bonus	(3) ln Bonus Compensation
Asset Size	**0.322*** **	0.185	**0.648*** **
	(0.031)	(0.345)	(0.156)
Net Income Growth	0.008	-0.128	**-0.239***
	(0.012)	(0.144)	(0.126)
Loan Growth	0.075	0.033	-0.502
	(0.280)	(3.008)	(1.410)
Member Growth	**-.840** **	4.396	0.330
	(0.385)	(4.368)	(1.979)
Asset Growth	**1.464*** **	-2.773	2.42
	(0.525)	(5.659)	(2.857)
Net Worth	**0.018** **	-0.010	0.006
	(0.008)	(0.089)	(0.051)
ROA	-0.031	**1.066***	0.049
	(0.047)	(0.566)	(0.227)
Fee Income Share	0.153	-3.294	**2.81***
	(0.288)	(2.897)	(1.546)
Dividend Yields	**-.198*** **	-0.579	0.239
	(0.060)	(0.636)	(0.325)
NIE	**0.060** **	0.058	-0.074
	(0.026)	(0.268)	(0.134)
Compensation Consultant	0.027	**0.785** **	0.066
	(0.036)	(0.394)	(0.184)
Board Members	-0.011	0.035	**-0.107** **
	(0.009)	(0.087)	(0.045)
Salary	**0.006*** **	-0.004	**0.039*** **
	(0.002)	(0.021)	(0.011)
Gender	0.07	-0.617	-0.284
	(0.049)	(0.650)	(0.233)
Constant	**9.95*** **	-0.066	**5.3*** **
	(0.295)	(3.242)	(1.530)
Observations	210	210	164
Adj. R-sqr/Pseudo R2	0.3965	0.0928	0.1827

***p-value<.01, **p-value<.05, *p-value<.10

For the equation measuring base compensation, the results are in line with expectations for the performance variables, but a surprising trend is found for the member services variables. A number of performance variables are positive and significant in the regression for base compensation, including asset growth, net worth, and non-interest expenditures as a share of total assets. Asset growth has the most economically signif-

icant coefficient, as a 1.46 percentage point increase in asset growth is associated with a 1% increase in base compensation. Strikingly, the variables capturing member services are negative and significant. The coefficient for member growth is -.840, indicating that a .84 percentage point decrease in the number of members in a credit union will lead to a 1% increase in the CEO's base compensation. Moreover, dividend yields is also negatively related to base compensation, suggesting that the less a credit union pays its members in dividends, the more it will pay its CEO. As for the miscellaneous variables, only the control variables total assets and salary are significant. On the whole, the model developed for base compensation has strong explanatory power, as the adjusted r-squared equals .3965.

In contrast to the base compensation regression, the logit regression for whether a CEO received bonus compensation is not explained well by the independent variables. Only two of the variables included in the regression are significant and the pseudo R^2 is only .0928. Interestingly, however, the use of a compensation consultant is one of the significant variables, which is consistent with the literature on executive compensation.

Finally, the equation measuring the bonus compensation contains some interesting findings as well. First, the share of income a credit union receives from fees in positively related to the amount of bonus compensation a CEO receives. This implies that the more credit unions charge their members for services, the bigger bonus a CEO receives. The result is similar to what was found in equation (1), that CEO's are disincentivized from prioritizing member services. Another fascinating result is that the size of a credit union's board is negatively related to the bonus pay of a CEO. This result is consistent with the literature on nonprofit executive compensation. Lastly, one perplexing result is that net income growth has a negative relationship with the size of a CEO's bonus. Net income growth is a fairly generic performance indicator so it is difficult to find a story that fits why this may be the case.

In comparison to the results of those found in the literature

for for-profit financial institutions and non-profit firms, the results for credit union executive compensation more closely match up to for-profit financial institutions. The results of the model suggest that in credit unions, executive compensation is primarily determined by financial performance indicators, which is the main determinant in for-profit financial institutions. Further, there is no evidence that credit unions incentivize member services, while in the non-profit literature the organization's social mission usually plays a central role in executive compensation.

It must be noted, however, that the relationships implied by the results are very tenuous and should be taken with a great deal of caution. First, the sample size of the study limits how confident we can be in the significance and magnitude of the relationships observed. Further, since we cannot safely assume that the independent variables are exogenous, we can only establish correlation and not causation.

VI. Conclusion

This study is the first to examine credit union executive pay using compensation information from IRS Form 990. I use OLS and logistic regression analysis to identify the determinants of base compensation, examine what factors lead to credit union CEOs receiving bonuses, and study the incentives credit unions place on their CEOs using bonus compensation. For base compensation, the results suggests that credit unions incentivize financial performance at the expense of member services. The evidence for bonus compensation somewhat supports what is found in equation (1), as fee income share is negatively related to bonus compensation. Board size also appears to have a large impact on the bonus a CEO receives. Finally, factors that determine if a CEO gets a bonus are return on average asset and the use of a compensation consultant.

The findings from this study help to illustrate what credit unions use to determine executive compensation and how that affects the incentives placed on credit union CEOs. Moving forward, the most obvious way to expand on the study

of credit union executive compensation would be to include time series data into the sample. Including time series data would highlight any industry trends in executive compensation and help control for idiosyncrasies in compensation practices among firms that cannot be identified with a single year of data. Additionally, the size of the dataset could be expanded to include state credit unions with under $500 million in assets.

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Two Roads in a Wood: An Econometric Analysis of the Major Choice of First-Generation College Students

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Abstract

Using data from the National Longitudinal Survey of Youth 1997, I estimate a multinomial logit choice model for the college major decisions of first-generation college students—students who are the first in the families to attend college—and non-first-generation students. The model controls for other factors such as sex, race, ability, and family income to isolate the effect of first-generation status on major choice for two otherwise identical students. I find that first-generation college students do make statistically different college major selections than otherwise identical students. I then examine whether the estimated differences between the major selection of first-generation and non-first-generation students is systematically related to characteristics of the majors. In particular, I use data extracted from the American Community Survey to create these measures of safety and stability. First-generation college students tend to be more risk averse than otherwise identical non-first-generation students whose parents have attended college, as they are more likely to select majors with well-defined career paths, high expected wages, and low unemployment rates.

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I. Introduction

In the fall of 2015, over 13 million American students attended 4-year universities[1]. Needless to say, not all 13 million students attended similar colleges or received a similar academic experience. An incoming freshman might decide to enroll at Arizona State University with annual tuition of about $10,000, over 65,000 undergraduates, and a mix of small and large lectures classes to receive a B.S. in computer science. Contrastingly, another incoming freshmen might instead enroll at Vassar College, with an annual tuition of almost $50,000, under 2,500 undergraduates, and small, discussion based classes to receive a B.A. in Philosophy. The wide range of educational institutions, degrees, and majors present in the United States illustrates a diversity in both the pedagogical philosophies and methodologies held by higher education providers and the resources, preferences, and educational interests of individual students.

To many students, a college education represents a transformative life experience. As Plato wrote over 2000 years ago, "the object of education is to teach us to love what is beautiful," a sentiment echoed on the University of Texas Plan II Honors program's website. There the interdisciplinary liberal arts program is advertised as "an education without boundaries," one "for a life, not for a living." To other students, a college education simply represents a practical investment in their human capital, affording degree holders higher wages and other rewards in the labor market. In 1967, then Governor of California Ronald Reagan embodied this outlook when he famously criticized the University of California system for offering courses on organizing social movements, arguing there was no value in "subsidizing intellectual curiosity" at universities.

Most American students likely view their college education as a healthy compromise of the positions represented by

[1] "College Enrollment and Work Activity of 2014 High School Graduates." U.S. Bureau of Labor Statistics. U.S. Bureau of Labor Statistics, 16 Apr. 2015. Web. 26 Aug. 2015.

Plato and President Reagan. That is, students pursue education both because it adds value and texture to their lives and because it builds human capital that the labor market rewards. Though normative questions about the proper goals of "good" education are difficult to address concretely, the social and economic repercussions of the existence of a diverse set of higher educational opportunities, like the range of college major options, can be analytically explored.

A student's college major decision is not a trivial one; not only does each college major require a unique set of coursework and talents, but not all college degrees are equally valuable. While a college degree today may be worth more than ever before[2], expected earnings vary tremendously across different majors. According to a 2015 report titled *From Hard Times to Better Times*[3] from the Georgetown University Center on Education and the Workforce (GCEW), there exists a wide range in returns to education by major, with median mid-career full-time earnings at $29,000 for Counseling Psychology majors compared to $120,000 for Petroleum Engineering majors. Thus, studying the tendencies in college major choice across demographic groups can serve to deepen an understanding of economic and educational disparities present in America.

Economic and sociological research has long explored how disadvantaged students make decisions differently in regards to higher education when compared to other students. The different contingent factors in a student's life such as their family's monetary constraints, their information about different degree options, and their unique set of cultural expectations likely influence the type of college degree an individual elects to pursue. In particular, we would expect these factors within a student's life to affect his or her likelihood of selecting dif-

[2]According to the same GCEW Report, the ratio of the median wage between workers with a college degree and workers with only a high school education has steadily increased from the mid-1970s to present day. College graduates now make over two times more than high school graduates.

[3]Carnevale, Anthony, and Ban Cheah. "From Hard Times to Better Times." *Center on Education and the Workforce.* Georgetown University, 05 Feb. 2015.

ferent college majors. While some students may be drawn to the renaissance-style education offered by the Plan II Honors program, it may not be a practical or appealing option for all college-bound youths.

Due to the disparate returns to education across different major fields, research targeted at reducing economic and educational inequality should focus not only on investigating motivations of college attendance but also of the selection of a field of study. In this paper, I investigate how first-generation college students differ from other students in their choice of college major. Using data from the National Longitudinal Survey of Youth 1997 (NLSY97), I estimate a multinomial logit choice model for the college major decisions of first-generation college students and non-first-generation students. The model controls for other factors such as sex, race, ability, and family income to isolate the effect of first-generation status on major choice for two otherwise identical students. I find that first-generation college students—students who are the first in the families to attend college—do make statistically different college major selections than otherwise identical students.

I then examine whether the estimated differences in major selection between first-generation and non-first-generation students is systematically related to characteristics of the majors. In particular, I use data from the American Community Survey (ACS) to create measures of the economic safety and stability of different majors. I find that first-generation college students are more risk averse than otherwise identical students who have parents who have attended college and tend to select majors with a well-defined career path, high expected wages, and low expected unemployment.

II. Literature Review

A growing body of economic and sociological research explores how disadvantaged students—namely members of certain minority groups, those from low-income families, and first-generation college students—make decisions differently in regards to higher education when compared to other students.

For example, disadvantaged minorities are less likely than other groups to prepare themselves academically for college. Using a nationally representative sample of the National Educational Longitudinal Study (NELS) data set, Stage, Droogsma-Musoba, and Brown[4] (2002) found that, controlling for ability, Asian American and White students were more likely than Black, Hispanic, and Native American students to take mathematics courses that would prepare them for college. Furthermore, low-income students are significantly less likely to decide to even apply to college. Using the same data set, Cabrera and La Nasa[5] (2002) found that while 76% of high socioeconomic status students submitted applications to four-year colleges, only 21.3% of low socioeconomic status students applied to college.

Not only are students from these disadvantaged groups less likely to apply to college, but when they do apply they tend to under-match, or in other words, apply to colleges below their academic ability level. Using a recent sample of Texas high school graduates, Black, Cortez, and Lincove[6] (2014) investigated race and ethnicity differences in college application decision-making using a sample recent Texas high school graduates. They found that disadvantaged minorities exhibit different college application decisions than Whites and Asians with similar levels of academic achievement. Inequality in college access is magnified through these under-matching tendencies of minority high-performing students. This reinforces the findings of Hoxby and Christopher[7] (2012), who first demon-

[4]Stage, F. K., Droogsma-Musoba, G., & Brown, C. (2002, April). *Mathematics achievement: Racial-ethnicity and course taking patterns.* American Educational Research Association, New Orleans.

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[6]Black, Sandra, Kalena Cortes, and Jane Lincove. "You Have to Apply Yourself: Racial and Ethnic Differences in College Application." Working Paper (2014). 1 Jan. 2015.

[7]Hoxby, Caroline, and Christopher Avery. "The Missing "One-Offs":

strated the tendency of high-achieving low-income students to choose not to apply to selective colleges.

While the behavior of disadvantaged students leading up to and navigating through higher education is increasingly the subject of academic research, only minor inquiry has been made regarding how these disadvantaged groups tend to select a field of study. Saks and Shore[8] (2005) estimated the risk associated with different careers and found education, health care, and engineering careers to have relatively safe streams of labor income; on the other hand, business, sales, and entertainment careers are more risky. Controlling for observable measures of ability and demographic background, they showed that students from low-income families tended to select majors that would result in less risky careers than high-income students.

That little economic research on college major selection has been done is surprising, given the strong connection between college major and labor market outcomes. Recent survey information has provided social science researchers with new data linking one's college major to future employment and financial outcomes. Starting in 2009, the ACS began asking for its respondents to report their college field of study. Altonji, Blom, and Meghir[9] (2012) use this data to review literature on the heterogeneous nature of educational specializations and the link between college majors and occupational paths. Amazingly, they find that the "difference in returns across college majors rivals the college wage premium." After adjusting for basic demographics, work experience, and postsecondary degrees, the gap in log wages rates between male electrical engi-

The Hidden Supply of High-Achieving, Low Income Students." National Bureau of Economic Research (2012): Working Paper. Web.

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[9]Altonji, Joseph, Erica Blom, and Costas Meghir. "Heterogeneity in Human Capital Investments: High School Curriculum, College Major, and Careers." Annual Review of Economics 4 (2012): 185-223. Web.

neering and male general education majors is a striking 0.56, nearly as large as the 0.57 difference between college graduates and high school graduates.

In this paper, I expand upon Saks and Shore's work by exploring the additional effect of first-generation status on student college major decisions. In doing so, I am able to separate out the impact of parental education from race and socioeconomic status. By better studying this important educational choice, my research adds to a greater body of work that aims to understand the decisions made by disadvantaged students while preparing for, pursuing, and specializing within higher education.

III. Theory

That low-income students tend to be more risk averse in their college major selection compared to middle and high-income of students is unsurprising. Post-secondary education in America is expensive; there exist substantial explicit academic costs to higher education (namely tuition). These explicit costs are heightened by the implicit opportunity cost of the forgone wages during the time spent in school.

Under perfect credit market conditions, a student might choose to borrow money now for the cost of college only to repay the money later with improved future labor market outcomes. However, human capital cannot be collateralized in the same way that other investments can be, making educational loans riskier than other types of lending. This credit market failure drives up interest rates for college loans, causing an increase in the marginal cost of education for the low-income students who are forced to take out loans. For this reason, attending college may only be a rational investment for low-income students wishing to specialize in a narrower, more lucrative set of college majors.

My research, however, focuses on the extent to which being a first-generation college student affects an individual's college major choice. First-generation college students are disproportionately from low-income families and members of disadvan-

taged minority groups in addition to facing a unique set of educational challenges. For these students, going to college is uncharted territory. First-generation college students are likely exposed to less or at least different information about college than those students whose parents have attended college. College, like most things, rewards students with both explicit financial returns and also intangible utility. First-generation college students may lack family member accounts of the non-financial rewards to a college education, leading them to give the expected labor market rewards of a major more relative weight when selecting their field of study.

In addition, because pursuing postsecondary education is atypical for their family and peer group, a first-generation student may be forced to more frequently justify his or her decision or articulate his or her post-graduation plans, thereby incentivizing a more pragmatic major selection. Preference may be given by first-generation college students to majors with higher wages and a more well-defined career path. For example, first-generation students might avoid studying areas such as communications or psychology and instead give preference to degrees in areas such as healthcare or engineering.

Being asked to answer the "Why college?" question might also alter the timeline of the college major decision of first-generation students. Many students enter their first year of college as undeclared students or adopt a major only tentatively, but first-generation students may experience pressure to select a major before they get to college. When a student makes their specialization could easily impact the major he or she selects.

Finally, first-generation students are a group comprised entirely of individuals who are is making a decision that is a departure from their familial and social norms. An individual's propensity to "go against the grain" and make such a choice could be the result of particular character traits or interests, which would therefore be more common in first-generation students than other students. This might result in the selection systematically different majors, though the major characteris-

tics that would be preferred remains unclear.

IV. Data Set

I begin by analyzing individual-level data from the National Longitudinal Study of Youth 1997 Cohort[10] (NLSY97). The NLSY97 consists of a nationally representative sample of approximately 9,000 youths who were between 12 and 16 years old as of December 31, 1996. The initial round of the survey took place in 1997. In that first round, both the eligible youth and one of the youth's parents received hour-long personal interviews. These youths were asked questions on their family background, education, work, and life decisions and were re-interviewed on an annual basis.

My sample consists of all the individuals in the NLSY97 who completed a 4-year college degree by 2010. Though interesting questions exist on a student's initial college major decision and its effect of major switching and drop-out rates, I limit my study to a student's final major selection. A student's final major can be thought of as their ultimate revealed preference and by using this as my metric of analysis I am able to draw connections between a student's college major and their expected labor market outcomes.

First I construct my college major variable COLLEGE_MAJOR. Though the NLSY97 has an already constructed major variable available based off of college transcripts, transcripts for many students were not collected. To increase my number of usable observations, I elect not to use this variable. Instead, I construct my own variable for college major using a self-reported "current college field of study" question that participants were asked on an annual basis. To do so, I order the annual responses from each individual chronologically and use each individual's final non-missing self-reported field of study for their COLLEGE_MAJOR value.

[10]The NLSY97 survey is sponsored and directed by the U.S. Bureau of Labor Statistics and conducted by the National Opinion Research Center at the University of Chicago, with assistance from the Center for Human Resource Research at The Ohio State University.

To obtain major variables each with a sufficient number of observations, I next group together similar COLLEGE_MAJORs to create 11 MAJOR_GROUP variables. In constructing these major groups, I worked to strike a sensible balance between groupings based on both similar expected labor market outcomes and related academic content. The few observations with major variables that did not fit well into any categories were dropped from my dataset. These dropped observations were *Home Economics, Automotive Mechanics, Transportation & Materials Moving, Security & Protective Services*, and *Uncodable*. The final major groupings of my NLSY97 sample set are displayed in Table 1.

Table 1 NLSY97 Major Groupings

MAJOR_GROUP	Number of Observations	NLSY97 Major Code (COLLEGE_MAJOR)
Arts	99	Fine & Applied Arts
Biology & Life Sciences	135	Agriculture & Natural Resources, Biological Sciences
Business	402	Business Management, Hotel & Hospitality Management
Communication	123	Communications
Computer Science & Math	104	Computer & Information Sciences, Mathematics
Education	194	Education
Physical Science & Engineering	128	Architecture & Environmental Design, Engineering, Other Sciences & Applied sciences, Physical sciences
Health	162	Nursing, Nutrition & Dietetics, Other Health Professions, Pre-Dental, Pre-Med, Pre-Vet
Humanities & Liberal Arts	205	Area Studies, English, Ethnic Studies, Foreign Languages, History, Interdisciplinary Studies, Philosophy, Theology & Religious Studies
Psychology & Social Work	153	Human Services, Psychology, Social Work
Social Science	276	Anthropology, Archaeology, Criminology, Economics, Geography, International Relations, Legal Services, Political Science, Pre-Law, Sociology
Total	1,981	

Next, I construct a FG_COLLEGE dummy variable for those individuals who never had a parent that attended college. For the purposes of this paper, I define first-generation student as one whose does not have a parent with greater than 12 years of schooling.

Finally, I define several key demographic variables for the individuals in my sample. I begin by creating a FEMALE dummy variable representing a female participant and a BLACK_HISPANIC dummy variable representing an individual who is Black or Hispanic. Next, I create a measure of

family income. Unfortunately, only the initial year of family income was usable for my analysis. In the year following the initial NLSY97 interview in 1997, a fraction of the youths in my sample set become emancipated. Any income reported thereafter is the individual's own personal income. Because comparing an 18 year old's personal income against 17 year old's family income would be unreflective of their actual relative financial situations, I limited my income measure to just family income in the year of 1997 and call this variable INCOME_1997. For a measure of cognitive ability, I use percentile scores from the Armed Forces Qualification Test (AFQT) to create an AFQT_PCT variable.

Unfortunately, a small number of observations are missing values for both AFQT_PCT and INCOME_1997; I drop these observations from my dataset. For any remaining observations, I then estimate any missing values for either AFQT_PCT or INCOME_1997 by forming predicted values based from OLS regressions with the other 4 variables as independent variables. For example, I estimated 168 missing AFQT_PCT values by using an OLS regression with INCOME_1997, FG_COLLEGE, FEMALE, and BLACK_HISPANIC independent variables.

Table 2 contains summary statistics of my NLSY97 sample. There are several substantial differences in characteristics between the first-generation and non-first-generation members of my sample, suggesting that it might be important to control for these differences in order to isolate the true effect of being a first-generation college student on major selection.

Table 2 NLSY97 Dataset Description

	FG_COLLEGE	Not FG_COLLEGE
Percent Female	67.25%	55.77%
Percent Hispanic or Black	47.37%	25.99%
AFQT Percentile Mean	58.58	70.48
AFQT Percentile Standard Deviation	24.88	22.89
1997 Family Income Mean	$45,985	$74,649
1997 Family Income Standard Deviation	$29,804	$54,224
Number of Observations	342	1,639

In addition to the NLSY97, I use data from the American Community Survey (ACS) to create variables measuring

wages, unemployment rates, and occupational concentrations for recent college graduates. The ACS is an ongoing statistical survey conducted by the U.S. Census Bureau, sent to over 3 million households each year. Beginning in 2009, the ACS began asking for its respondents to report their college field of study, providing researchers with unprecedented access to a large data set linking an individual's college major to their employment and financial outcomes.

I begin with an extract of the ACS that contains over 1.5 million observations from 2009, 2010, and 2011. Because I am primarily interested in measuring the labor market rewards for recent college graduates, I keep only observations on individuals between the ages of 22 and 26, leaving me with just over 60,000 observations[11].

Next I have to code the ACS field of study variable into my MAJOR_GROUP variable. I tried to be as consistent with my NLSY97 groupings as possible. The final ACS major groupings are displayed in Table 3.

Next I have to code the ACS field of study variable into my MAJOR_GROUP variable. I tried to be as consistent with my NLSY97 groupings as possible. The final ACS major groupings are displayed in Table 3.

[11] As a check for robustness, I also conducted the same analysis with all observations in the ACS sample. Changing the age did not significantly alter the results.

Table 3 ACS Major Groupings

MAJOR_GROUP	Number of Observations	ACS Major Code
Arts	3,579	Fine Arts
Biology & Life Sciences	6,577	Agriculture, Biology & Life Sciences, Environment & Natural Resources
Business	12,120	Business
Communication	3,918	Communications, Communication Technologies
Computer Science & Math	2,746	Computer & Information Sciences, Mathematics and Statistics
Education	5,879	Education Administration & Teaching
Engineering	5,191	Architecture, Engineering, Engineering Technologies, Physical Sciences, Nuclear & Industrial Radiology
Health	4,012	Medical & Health Sciences
Humanities & Liberal Arts	6,380	Area & Ethnic Studies, English, History, Liberal Arts, Linguistics & Foreign Languages, Philosophy, Theology
Psychology & Social Work	4,613	Psychology
Social Science	5,634	Law & Public Affairs & Social Work, Social Sciences
Total	**60,649**	

With the ACS data, I construct three major group characteristic variables: occupational concentration, mean hourly wage, and unemployment rate. The occupational concentration variable measures the percentage of all graduates from a given major group that work in the 3 most common occupations for that major group. A detailed table showing the underlying most common occupational fields for each major used to construct this occupational concentration value can be found in the data appendix of this paper. For the occupational groupings, I used occupational fields from the ACS[12].

Occupational concentration serves to describe the clarity of the career path of a major group; a major group that has a very high fraction of its graduates employed in the 3 most common occupational groups can be thought of as having a clear career path. In other words, the question of "What can I do after college with this major?" has a relatively more well-defined answer for majors with high occupational concentrations than

[12]The ACS has both broad and narrow occupational concentration groupings available. I chose to use the broad groupings, but as a check for robustness I also conducted the same analyses using the narrow groupings, which yielded very similar results.

for majors with low occupational concentrations.

Next I calculate the average hourly wage for each major group. I adjust[13] all the wage variables to be in 2010 USD and then remove all individuals who work less than 50 weeks a year. The ACS has only interval data on the number of weeks worked for part-year workers, so I was unable include them when constructing my wage variable. To create a measure of hourly wage for each major group, I divide the annual earnings of every individual in a major group by the number of weekly hours typically worked by that individual times 50. I average all the hourly wages in a particular major group to create my wage variable for that major group.

Finally, I calculate the major group unemployment rate. I simply divide the number of people without jobs seeking work in a particular major group by the total number of people in the labor force from that major group. Chart 4 contains summary statistics from the ACS for each major group.

Table 4 ACS MAJOR_GROUP Statistics

MAJOR_GROUP	Occupational Concentration		Mean Hourly Wage	Unemployment Rate
	Narrow	*Broad*		
Arts	21.17%	51.37%	15.62	8.22%
Biology & Life Sciences	16.40%	46.12%	18.06	4.98%
Business	23.64%	55.25%	20.87	5.40%
Communication	13.36%	48.65%	17.59	5.99%
Computer Science & Math	37.66%	66.28%	24.65	5.28%
Education	67.45%	85.64%	16.51	3.77%
Engineering	24.92%	62.18%	25.34	5.37%
Health	51.99%	78.39%	24.93	3.30%
Humanities & Liberal Arts	17.73%	49.44%	16.78	7.57%
Psychology & Social Work	26.00%	46.28%	16.60	6.25%
Social Science	10.65%	36.92%	20.30	7.82%

Notice a wide range of values exists for occupation concentration, mean hourly wage, and unemployment rate among the major groups. For example, *Arts* has an occupational concentration of 51%, a mean hourly wage of $15.6, and an

[13] I use the "U.S. City Averages" Consumer Price Index from the Bureau of Labor and Statistic from the years 2009, 2010, and 2011 to perform these calculations.

unemployment rate over 8% whereas *Health* has an occupational concentration of 78%, a mean hourly wage of almost $25, and an unemployment rate of 3.3%. Importantly, a major group's rank in one descriptive category is not always similar to its ranking in the other categories. For example, *Education* is near the minimum value of mean hourly wage distribution while *Health* is close to the maximum, but both *Education* and *Health* are among the fields with the highest occupational concentrations.

V. Data Analysis

I begin by using my sample from the NLSY97 to construct a naïve comparison of major choice between first-generation students and non-first-generation students. Table 5 contains a simple breakdown of the percentage of first-generation and non-first-generation students in each major group within my sample.

Table 5 NLSY97 MAJOR_GROUP Breakdown

MAJOR_GROUP	FG_COLLEGE	Not FG_COLLEGE	Difference (FG_COLLEGE - Not FG_COLLEGE)
Arts	2.63%	5.49%	-2.86%
Biology & Life Science	5.26%	7.14%	-1.88%
Business	18.13%	20.74%	-2.61%
Communication	3.8%	6.71%	-2.91%
Computer Science & Math	5.56%	5.19%	0.37%
Education	13.45%	9.03%	4.42%
Engineering	6.43%	6.47%	-0.04%
Health	9.65%	7.87%	1.78%
Humanities & Liberal Arts	5.85%	11.29%	-5.44%
Psychology & Social Work	11.11%	7.02%	4.09%
Social Science	18.13%	13%	5.07%

A statistical significance test rejects the hypothesis that these differences are equal to zero at conventional levels. In other words, this table suggests that there is a relationship between first-generation status and college major selection. However, this sort of analysis fails in addressing my true line of inquiry. As seen earlier in Table 2, first-generation students in my sample set are quite characteristically different than non-first-generation students. Specifically, they are more heavily female and minority than non-first-generation students. In

addition, first-generation students have lower average AFQT scores and come from families with lower average income. We would expect to observe differences in first-generation students' college major selection fueled by these demographic differences alone. In order to answer the question of the causal impact of first-generation status on college major selection, I need to account for these confounding variables.

In order to isolate the effect of first-generation status on major choice for two otherwise identical students, I use my NLSY97 sample to estimate a multinomial logit choice model for college major decisions. The model has MAJOR_GROUP as its dependent variable and FG_COLLEGE, FEMALE, BLACK_HISPANIC, AFQT_PCT, and INCOME_1997 as its independent variables. Because FG_COLLEGE is a dummy variable, its average marginal effect measures the average excess likelihood that a first-generation student selects a given major group compared to an otherwise identical non-first-generation student. We can think of this value as the average effect of being a first-generation college student on college major selection that is independent of differences in sex, race, family income, and ability.

I next examine whether these estimated differences between a first-generation and a non-first-generation students' likelihood of choosing different major groups are systematically related to the characteristics of the major groups themselves. I consider three separate major group characteristics: occupational concentration, average wage, and unemployment rate. Each one is illustrative of a different aspect of the relative safety and stability of a major group's labor market rewards. Unemployment rates represent the risk of not being able to find suitable work, average wages represent expected pay conditional on employment, and occupational concentration serves to describe the clarity of the career path of a major group.

I create 3 scatter plots, each one with a different major group characteristic on the x-axis and the average marginal effect of FG_COLLEGE on the y-axis. Here, I link my college

major choice data from the NLSY97 with my college major returns data from the ACS. Unfortunately, with only 11 major groups to use as data points, rigorous statistical tools are unsuited to measure how precisely a major group's characteristics relate to the excess likelihood that first-generation college students select that major relative to otherwise identical non-first-generation students. However, the scatter plots at least allow for a visual inspection of the relationship between the variables.

VI. Results

I find that first-generation status has a statistically and economically significant effect on college major selection. This effect is independent of the compositional differences of first-generation students, including sex, AFQT score, family income, and race (which also all have a statistically significant effect on college major selection). Table 6 contains the results from a joint significance test across all equations for each variable in my multinomial logit model[14].

Table 6 — Significance test results for the hypothesis that the coefficients on a variable are 0 in all equations of the multinomial logit model

Variable	Chi^2 Statistic	P-value
FG_COLLEGE	24.84	0.0057
AFQT_PCT	58.1	0.0000
INCOME_1997	17.15	0.0711
FEMALE	181.59	0.0000
BLACK_HISPANIC	32.29	0.0004

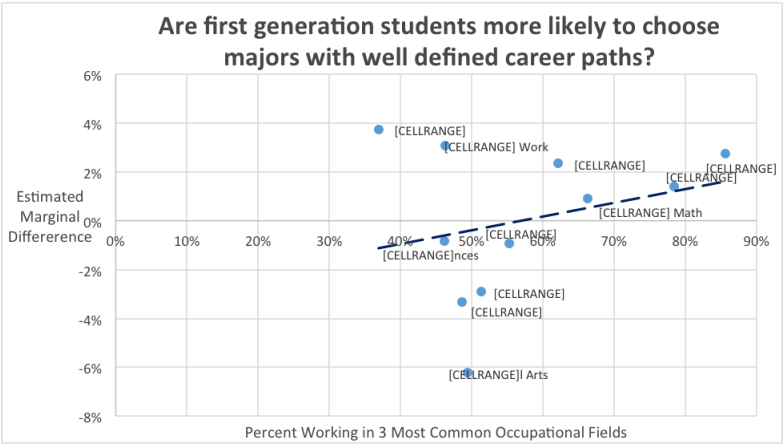
Using the results from my multinomial logit model, I isolate the effect of being a first-generation college student on the probability of selecting a particular of major by calculating the average marginal effect of the FG_COLLEGE variable. These impacts are displayed in table 7.

[14] A full table of results from the multinomial logit model can be found in the data appendix at the end of this paper.

Table 7 Logit Model FG_COLLEGE Difference Results

MAJOR_GROUP	**Raw Difference**	**Adjusted Difference**
Arts	-2.86	-2.89%
Biology & Life Sciences	-1.88	-0.85%
Business	-2.61	-0.93%
Communication	-2.91	-3.33%
Computer Science & Math	0.37	0.92%
Education	4.42	2.73%
Engineering	-0.04	2.34%
Health	1.78	1.40%
Humanities & Liberal Arts	-5.44	-6.20%
Psychology & Social Work	4.09	3.08%
Social Science	5.07	3.73%

Next, I construct scatter plots to examine the relationship between the propensity of first-generation students to select a given major group and characteristics of that major group. The vertical axis measures the excess likelihood that a first-generation student selects the major group compared to an otherwise identical student. The horizontal axis measures a particular descriptive statistic of the major group, taken from the ACS. Figures 1 & 2 display scatter plots using the major group's occupational concentration and average wage, respectively. In both cases, simple OLS regressions using the 11 major groups have positive slopes, with the slope in Figure 1 being .26 and the slope in Figure 2 being .40. This suggests that as occupational concentration and expected wages of a major group increase, so does the excess likelihood that first-generation college students select that major relative to otherwise identical non-first generation students.



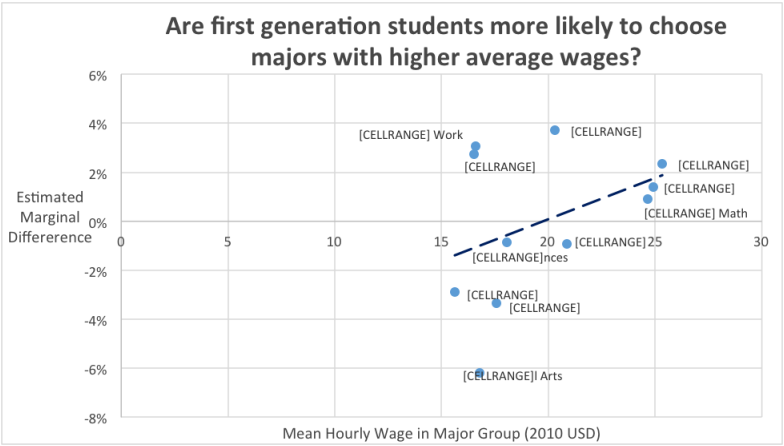
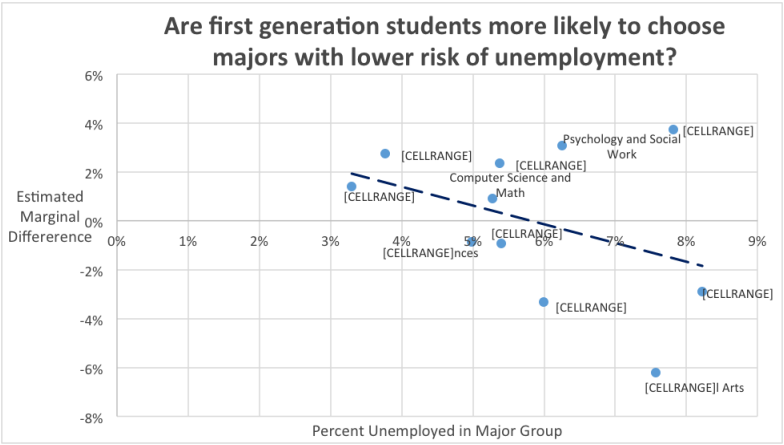


Figure 3 displays a final scatter plot for major group unemployment rates. A simple OLS regression using the 11 major fields has a negative slope with a coefficient of -.39. This suggests that as the unemployment rate for graduates of a major field increases, the likelihood that first-generation college students select that major compared to otherwise identical non-first-generation students decreases.



VII. Discussion

My results highlight how a simple comparison between the college majors selected by first-generation students compared to non-first-generation students can be misleading. For example, a naïve inspection of the raw data would suggest that first-generation students are actually slightly less likely than non-first-generation's students to become engineering majors. However, when I control for the systematic differences in race, ability, family income, and gender between the two groups, it becomes clear that engineering is actually among the specializations which first-generation students prefer most relative to their non-first-generation counterparts.

The multinomial logit model that I estimate not only confirms Saks and Shore's (2005) findings on the effect of family income on a student's college major decision, but also shows that not having parents who have attended college has a significant effect on a student's college major selection. Though an individual's lifetime socioeconomic status is likely partially captured in any variable measuring parental education levels, that first-generation status had an effect even alongside the family income variable suggests that first-generation status may represent an independent effect and mechanism[15].

[15]Variables of FG_COLLEGE interacted with the other 4 explanatory

Perhaps unsurprisingly, in addition to first-generation status, family income, ability, race, and gender all appear to be significantly related to an individual's college major selection.

According to my multinomial logit model, compared to non-first-generation students, first-generation students prefer the following majors: *Computer Science & Math, Education, Engineering, Health, Psychology & Social Work*, and *Social Science*. These same students are less drawn towards the following majors: *Arts, Biology & Life Sciences, Business, Communication*, and *Humanities & Liberal Arts*. The majors groups that first-generation students prefer tended to have low unemployment, high average wages, and a high occupational concentration. The existence of a preference among first-generation college students towards majors groups with these characteristics is consistent with my theoretical framework. Lacking information on intangible benefits to education, these students emphasize labor market rewards when selecting their field of study.

That occupational concentration appears to be related to first-generation student major preferences is particularly interesting. Unlike unemployment rates and average wages, occupational concentration is not directly linked to any economic returns to a college major. In fact, many major groups on both the high and low ends of average wages have similar occupational concentration scores (for example, education and health). Instead, occupational concentration is linked to the clarity of career path post-graduation. That first-generation students, who are likely forced to answer the "Why college?" question more frequently than non-first-generations students, tend to select fields with clear career paths is an important finding.

Unfortunately, data limitations prevented me from attempting to model the joint impact of these major group characteristics. Because the many qualities and expectations

variables failed statistical significance tests, suggesting that this first generation effect may not vary systematically with to income, race, sex, and ability.

of a major simultaneously contribute to its desirability, further research should focus on better understanding the combined effects of these characteristics on the major selection decisions of different groups of students. Additionally, future research should investigate the consequences of having certain disadvantaged and underrepresented groups concentrated in particular major fields. For example, liberal arts programs like Plan II Honors that are interested in having a diverse student body might struggle to seem attractive to low-income and first-generation students. Additionally, universities seeking a diverse faculty may find their supply limited by the practical, more job market oriented focus of the specializations preferred by these underrepresented students.

VIII. Conclusion

The educational decisions an individual makes can have a large impact on many aspects of their life. For example, the wide range of college majors an individual can select from have a correspondingly wide range of economic outcomes. The selection of a college major is a nuanced decision significantly influenced by numerous factors, including ability, sex, race, income, and parental education.

The empirical analysis in this paper suggests that first-generation students, compared to otherwise identical students, are more likely to select major groups with strong labor market rewards and a clear career path. Importantly, these differences exist even after controlling for sex, race, ability, and family income. Given that first-generation students are disproportionately from low-income families, this behavior is likely to contribute to the reduction of economic inequality over the long run. However, that these students are inclined towards economically safer majors with clear career paths suggests that they might be more constrained in their decisions relative to other students. If students have idiosyncratic, major-specific abilities, working to reduce these "constraints" could allow more suitable specialization among certain low-income and first-generation students and improve economic efficiency.

What exactly causes these differences in major selection is unclear. It may be that first-generation students receive less exposure to information on the potential non-monetary rewards to a college education; or perhaps being the first in one's family to attend college places certain pressures on a student; or it could simply be that a characteristically-distinct subset of the population of those whose parents did not attend college decide to pursue higher education. The underlying mechanisms driving the findings of this paper present interesting questions for future research focusing on educational inequality.

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Appendix

Top 3 Occupational Fields by Major Group of Recent College Graduates (Wide Groupings)

MAJOR_GROUP	Top Occupational Field Share	Second Occupational Field Share	Third Occupational Field Share	Total Top 3 Share
Arts	Arts, Design, Entertainment, & Media 22.67%	Sales 14.69%	Office and Administrative Support 14.01%	**51.37%**
Biology and Life Sciences	Healthcare Practitioners 18.25%	Education 16.06%	Scientific Research 11.80%	**46.12%**
Business	Financial Specialists 21.52%	Sales 17.86%	Office and Administrative Support 15.87%	**55.25%**
Communication	Office and Administrative Support 17.20%	Arts, Design, Entertainment, & Media 16.27%	Sales 15.18%	**48.65%**
Computer Science and Math	Computation, Programming, and Math 46.79%	Education 13.41%	Sales 6.08%	**66.28%**
Education	Education 75.70%	Sales 5.21%	Office and Administrative Support 4.73%	**85.64%**
Engineering	Architecture and Engineering 41.46%	Computation, Programming, and Math 13.27%	Education 7.45%	**62.18%**
Health	Healthcare Practitioners 69.91%	Office and Administrative Support 4.41%	Sales 4.07%	**78.39%**
Humanities and Liberal Arts	Education 23.13%	Office and Administrative Support 15.25%	Sales 11.07%	**49.44%**
Psychology and Social Work	Education 16.63%	Community and Social Services 16.29%	Office and Administrative Support 13.36%	**46.28%**
Social Science	Office and Administrative Support 14.40%	Sales 11.71%	Community and Social Services 10.81%	**36.92%**

NLSY97 MAJOR_GROUP Multinomial Logit Model
Coefficients and Z-statistics

	Arts	*Biology & Life Sciences*	*Business*	*Communication*	*Computer Science & Math*	*Education*	*Engineering*	*Health*	*Humanities & Liberal Arts*	*Psychology & Social Work*	*Social Science*
INCOME_1997	-0.001	-0.003	(Omitted)	0.003	-0.006	-0.002	-0.004	-0.003	-0.005	0	-0.001
	(-0.24)	(-1.08)		(-1.37)	(-1.95)	(-0.96)	(-1.71)	(-1.31)	(2.45)*	(-0.01)	(-0.8)
AFQT_PCT	0.009	0.015	(Omitted)	0.007	0.009	-0.006	0.024	0.011	0.015	0.004	0.005
	(2.01)*	(3.56)**		(-1.65)	(-1.92)	(-1.53)	(5.55)**	(2.73)**	(4.01)**	(-1)	(-1.36)
FEMALE	0.467	0.185	(Omitted)	0.597	-1.271	1.282	-1.231	(1.22)	0.466	1.08	0.174
	(2.03)*	(-0.92)		(2.77)**	(4.97)**	(6.26)**	(5.20)**	(5.67)**	(2.64)**	(5.02)**	(-1.09)
FG_COLLEGE	-0.545	-0.087	(Omitted)	-0.502	0.229	0.334	0.411	0.216	-0.566	0.452	0.321
	(-1.41)	(-0.29)		(-1.49)	(-0.75)	(-1.44)	(-1.43)	(-0.85)	(2.00)*	(-1.83)	(-1.53)
BLACK_HISPANIC	-0.012	0.04	(Omitted)	0.86	0.397	-0.274	0.179	0.268	0.148	0.385	0.626
	(-0.04)	(-0.16)		(3.62)**	(-1.48)	(-1.24)	(-0.68)	(-1.18)	(-0.69)	(-1.69)	(3.37)**
_cons	-2.194	-2.094	(Omitted)	-2.498	-1.288	-1.035	-2.198	-2.336	-1.592	-2.131	-0.956
	(5.25)**	(5.68)**		(6.59)**	(2.99)**	(2.89)**	(5.83)**	(6.11)**	(4.77)**	(5.44)**	(3.16)**

N= 1,981
* $p<0.05$; ** $p<0.01$

How Does Medical Marijuana Legalization Affect The Number of Marijuana Users? An Inclusion and Examination of Different Age Groups

Anthony Duong †

Abstract

This paper studies the effect of medical marijuana legalization on the number of marijuana users in a given state's general population. To measure the number of marijuana users in each state, I used survey data that covers multiple years. Given this data, I used a differences-in-differences model to see if the changes in marijuana use over time in legalized states are significantly different from those in non-legalized states. While my initial results showed that legalization significantly increases the percentage of marijuana users in the general population, this was no longer the case when I specified the medical marijuana states that allow for home cultivation and those that do not. When I added home cultivation to the regression, the home cultivation dimension of medical marijuana laws was shown to significantly increase the number of past month users. These results suggest that the marijuana demanders in the general population are not deterred by legal penalties, but that the reduction of supply side penalties via home cultivation increases the number of past month users.

I. Introduction

The main question that this paper seeks to answer is: "How does the legalization of medical marijuana affect the number of

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marijuana users in a given state?". Marijuana users were measured in two ways: first time users, and past month users. The former has implications for medical marijuana legalization's influence on introducing people to marijuana, while the latter has implications for medical marijuana legalization's influence on those who have tried it to use it on a regular (monthly) basis. Meanwhile, medical marijuana legalization, in its purest sense, is defined in this paper as the legality of qualifying patients and their caregivers to consume marijuana. Most medical marijuana states require patients to be 18 to qualify, and in states that do not, minors rarely qualify for medical cards (Pacula, 2007).

To answer this question, I compared marijuana use over time in medical marijuana states with marijuana use over time in non-medical marijuana states. Using a differences-in-differences analysis, I attempted to estimate the policy effect of medical marijuana legalization. To do this, I examined the changes in marijuana use in states that legalized medical marijuana with the changes in marijuana use in states that did not legalize marijuana, over the same time period. Furthermore, I attempted to isolate medical marijuana legalization in its simplest form by distinguishing between medical marijuana states that allowed patients to cultivate marijuana plants at home and those that did not in my final regressions. Data for first time use and past month use came from the annual National Survey on Drug Use and Health, while the data on medical marijuana legalization and its components came from previous studies on the policy.

The results of my model including home cultivation indicate that medical marijuana legalization does not have a significant effect on the number of first time users or past month users. These new results contribute to the literature on marijuana use because there has been little work studying the rates of marijuana initiation within the general population; most studies focus on non-first time use among the youth. Another contribution includes estimating the effect of both medical marijuana legalization and home cultivation laws on

different age groups.

II. Literature Review

The marijuana regime literature is important for estimating the effects of marijuana policy liberalization on marijuana consumption. According to supporters of drug prohibition, increased marijuana consumption would have mostly negative **implications**. They argue that consuming illegal drugs like marijuana is irrational because consumers tend to underestimate the drugs' addictiveness, and that the drugs have negative health consequences (Miron and Zwiebel, 1995). They also state that a negative externality of consuming illegal drugs is that it causes users to commit crimes. Supporters of drug policy liberalization argue that the negative externalities of consumption are small compared to those of prohibition such as resources expended on enforcement, and violent crime caused by an illegal drug trade (Miron and Zwiebel, 1995). Thus, supporters of drug policy liberalization assert that without regard to its high negative externalities, marijuana prohibition can only be justified if it reduces use that produces negative externalities or is individually irrational.

As medical marijuana legalization has taken place more over time, study on the policy has grown within the marijuana regime literature. Not only is medical marijuana legalization an insightful topic because it affords even more legal protection for patients than decriminalization can, but also because it grants legitimacy to some suppliers (dispensaries). For most of recent history, almost nothing has been known about supply side policies. However, Anderson and Rees (2014) were able to study the effect of dispensaries on teen marijuana use by using a differences-in-differences model comparing teen marijuana use across years and counties. They used Los Angeles County, which had hundreds of dispensaries open in the 2000's, as a treatment group, counties with no dispensaries in the same period as the control group, and local Youth Risk Behavior Survey data to measure teen marijuana use. Their results revealed that dispensaries did not have a significant ef-

fect on teen marijuana use, since adults still faced high risks of selling marijuana to minors[1]. In a previous study, Anderson and Rees (2013) compared trends in hospital admissions to non-federal hospitals in Colorado (recorded by the Drug Abuse Warning Network) before and after Colorado's mass opening of dispensaries to study the effect of dispensaries on marijuana use. Their results showed once again that dispensaries have no effect on levels of marijuana use[2].

While dispensary proliferations allow for groundbreaking study of reduced supply side penalties, the majority of medical marijuana legalization literature has focused on the demand side penalties. Harper, Strumpf and Kaufman (2012) used a differences-in-differences model to estimate the effect of medical marijuana legalization on state prevalence for past month use and perceived risk of monthly use. Over the period 2002-2009, they found no significant impact of legalization on either past month use or perceived risk for any age group[3]. This is the only study I came across that looked at the survey data for the general population, as the rest of the studies either focused on adolescents or followed a cohort through time. One such study by Anderson, Hansen and Rees (2013), used state and national Youth Risk Behavior Survey data to construct a linear probability model, finding that legalization has no significant effect on teen marijuana use[4]. As the medical marijuana literature has developed, researchers have increasingly treated medical marijuana as a heterogeneous policy. Pacula, Powell, Heaton and Sevigny (2013) looked to isolate the different dimensions of medical marijuana laws, such as requiring "pain" for a card, allowing home cultivation, and allowing for dispensaries. Using three different datasets, the National Longitudinal Survey, Youth Risk Behavior Survey, and the Treatment Episode Data Set to measure marijuana use, they ran regressions on different dimensions of the medical marijuana law. Their differences-

[1] Anderson, Rees, and Hansen (2013), 19-20.
[2] Anderson and Rees (2013), 4.
[3] Harper, Strumpf, and Kaufman (2012), 210-211.
[4] Anderson, Rees, and Hansen (2013), 17.

in-differences results suggested that home cultivation had a positive effect on heavy youth use and that dispensaries led to more hospital admissions[5]. However, Anderson and Rees (2013) criticized this study's inclusion of a dispensary dummy, noting that there are often many years in between the law allowing for the dispensary and when dispensaries actually begin to open[6].

Given the flaws of a dichotomous dispensary variable, and the results in Anderson and Rees (2014) and Anderson and Rees (2013) that suggest dispensaries have no significant effect on marijuana use, I wanted to study the home cultivation dimension of medical marijuana legalization. Pacula, Kilmer, Grossman and Chaloupka (2007) present the theory that any reduction of legal penalties for suppliers, particularly home cultivation, will reduce price and increase marijuana use. The first argument is that it is impractical to stop home cultivators from being illegal sellers, while the second is that home cultivation creates "social availability"[7]. This "social availability" argument states that minors would come into contact with marijuana growers more often under home cultivation and have increased opportunities to try marijuana[8]. There was no specific variable for home cultivation, which motivated my inclusion of a home cultivation variable in my model.

I believed my model would contribute a new aspect to the literature and also synthesize several distinct aspects of different studies that have not all been brought together in a single study. I had yet to encounter a measure of first time use in the literature - I had only come across survey data for use in the past year or past month, or measures of the intensity of marijuana use among past users. I obtained my measure from the NSDUH, which has the advantage over the NLSY or YRBS of surveying a representative population. This allowed me to account for legalization's effect on the general population, but

[5]Pacula, Powell, Heaton, and Sevigny (2013), 22.
[6]Anderson and Rees (2013).
[7]Pacula, Kilmer, Grossman, and Chaloupka (2007), 6.
[8]Pacula, Kilmer, Grossman, and Chaloupka (2007), 7.

also potentially examine preferences across age groups, and diversion from adults to minors. While one aforementioned study used the NSDUH for estimating the effect of medical marijuana legalization, I was able to add the past three years of observations (in which several states legalized medical marijuana). And unlike this study, I attempted to isolate home cultivation and as a result, medical marijuana legalization in its most basic sense.

III. Theory

My main hypothesis was that medical marijuana legalization would not increase the number of first time users or past month users because legal penalties do not deter consumers of marijuana. Enforcement against consumers is impractical because as established by the literature, marijuana transactions mainly occur in the privacy of one's residence[9]. Considering the difficulty for law enforcement to accurately guess when marijuana transactions are occurring within a home, law enforcement needs to violate civil liberties in order to curb these kinds of transactions. And unlike for dealers of harder drugs, who commit large amounts of violent crime and reap much bigger profits, law enforcement has little support for invading the homes of potential marijuana dealers. Given a near non-existent risk of consuming marijuana, I maintained that marijuana legalization does not make it significantly easier for demanders of marijuana to consume it for the first time or on a regular basis.

While those 18 or older can obtain a medical card with relative ease by claiming ailments that are difficult to verify, I predicted that the number of first time and past month users in this age group would remain stable. This is because the legal protection afforded by medical marijuana legalization would only significantly benefit those who already consume marijuana frequently. If that were the case, intensity of marijuana use among already frequent users might increase, but not necessarily the number of people who want to use mari-

[9]Pacula, Kilmer, Grossman, and Chaloupka (2007), 29.

juana for the first time or on a monthly basis. For the latter types, such modest use makes the expected risk of consumption quite small and the fixed cost of purchasing a medical card unjustifiable.

Since marijuana use is more common among 12-17 year olds and 18-25 year olds (see Table 2), it may be argued that 26+ year olds lack the underground networks to consume marijuana in the absence of a medical legalization. While this may be the case, 26+ year olds who have not tried marijuana for the first time would not want to at their current age. As I claimed earlier, transactions in the black market occur with little risk to the dealer or consumer. Thus, I predicted that those who are 26+ and have not tried marijuana at their current age decided not to do so because they simply derive little to no utility from it. I expected this to remain the case in ages 26+ because marijuana use becomes less attractive as career and family obligations become more time consuming and as leisure time decreases.

Table 2. Summary Statistics of State Prevalence Rates

Variable	Observations	Mean	Std. Dev.	Min	Max
First Use of Marijuana					
Age 12+	561	1.842799	.3416988	1.18	3.19
Age 12-17	561	6.196506	1.148745	3.42	10.35
Age 18-25	561	7.245027	1.574028	2.96	12.47
Age 26+	561	.1543137	.0590635	.02	.64
Past Month Use of Marijuana					
Age 12+	561	7.017059	2.493228	3.11	19.75
Age 12-17	561	8.191462	2.611606	3.68	21.5
Age 18-25	561	19.03519	6.191116	7.74	46.99
Age 26+	561	4.77467	1.978433	1.9	16.07

IV. Empirical Strategy

To turn my question into a testable hypothesis, I compared medical marijuana states and non-medical marijuana states across time in a difference-in-differences model. In essence, the model compares the changes in a variable over time for a treatment group and the changes in the same variable over the same time for a control group to see if there is a significant difference. If the difference is significant and shifts in group characteristics have been controlled for, the treatment is said to have a significant effect on the chosen variable.

For my study, I compared changes in marijuana use before and after legalization in legalization states with changes in marijuana use in non-legalization states over the same time period. Controlling for changes in state characteristics, any significant difference would have been caused by the passing of a medical marijuana law. Knowing this, I expressed the model in terms of a regression. In my regression, my dependent variable was whichever measure of marijuana use I was testing ñ first time use or past month use. To indicate whether a state was a treatment state, I used a dummy variable that would equal 1 if the state would eventually have legalized medical marijuana. This variable accounts for inherent differences between legalization states and non-legalization states that might affect marijuana use (such as liberal attitudes towards marijuana use). The variable that estimates the effect of legalization (thus our variable of interest) is a dummy variable that equals 1 in legalization states in years after the legalization has taken place. If this policy effect variable was significant at the 5% level, then I would reject my hypothesis that medical marijuana legalization does affect marijuana use. Otherwise I would fail to reject my hypothesis. Finally, I added a dummy variable for every year in the time period I studied minus one, to control for any time-related trends in marijuana use.

To control for differences across states, I added variables for policies that were not specifically medical marijuana legalization, and added some controls for time-variant state characteristics. First I added decriminalization variables because de-

criminalization may increase marijuana use via reduced penalties on the demand side. I also added each state's unemployment rate and median household income. The former may increase marijuana use by allowing for more leisure time, while the latter may increase use if marijuana is a normal good. Lastly, I added each state's tax on a pack of cigarettes since cigarettes and marijuana may be substitutes, in which case increased cigarette taxes could increase marijuana use.

Thus, the regression that I ran to estimate the effect of medical marijuana legalization on the percentage of first time marijuana users was:

$$\begin{aligned} Firstuse = & \beta_0 + \beta_1 Mml + \beta_2 Post + \beta_3 Decrim + \\ & \beta_4 Postdecrim + \beta_5 Year2012 + \beta_6 Year2011 + \ldots + \\ & \beta_{14} Year2003 + \beta_{15} Unemployment + \\ & \beta_{16} Medincome + \beta_{17} Cigtax + E \end{aligned}$$

where *Firstuse* is the percentage within a state that reported using marijuana for the first time in the last year. *Mml* equals 1 if the state has ever passed a medical marijuana law up until now and 0 otherwise. *Post*, the variable of interest, equals 1 if *Mml* equals 1 within that state, in a year after medical marijuana has been legalized in that state. Decrim equals 1 if the state has ever passed a marijuana decriminalization up to this date, and 0 otherwise. *Postdecrim* equals 1 if *decrim* equals 1 for the state and the observation takes place in a year after the state has decriminalized marijuana. The variables *year2003-year2012* are dummies indicating the year to which the observation belongs to. *Unemployment* is the state's unemployment rate, *Medincome* is the state's median household income, and *Cigtax* is the state's tax on a pack of cigarettes. I ran this regression on three age cohorts (12-17, 18-25, and 26+). Lastly, I ran the same regression with *Monthuse* in place of *Firstuse* (for the general population as well as the 12-17, 18-25, and 26+ cohorts).

In my final regressions, however, I added a variable for home cultivation. 16 out of the 21 states allow patients and

caregivers to grow a certain number of marijuana plants at home (see Table 1). Since home cultivation laws have all happened concurrently with legalization, I added a dummy variable, *posthc*, which equals 1 in years in a home cultivation state in a year after the home cultivation law passed and run additional regressions. The purpose of making my regression more specific was to isolate the effect of a medical marijuana law in its basic form, which is a law that grants patients and caregivers legal protection to consume marijuana.

Table 1. States with Marijuana Regime Changes as of March 17th, 2014

State	Medical marijuana legalization	Year	Home Cultivation	Decriminalization	Year
Alaska	Yes	1998	Yes	Yes	1975
Arizona	Yes	2010	Yes		
California	Yes	1996	Yes	Yes	1975
Colorado	Yes	2000	Yes	Yes	1975
Connecticut	Yes	2012		Yes	2011
Delaware	Yes	2011			
District of Columbia	Yes	2010			
Hawaii	Yes	2000	Yes	Yes	2008
Illinois	Yes	2013	Yes		
Maine	Yes	1999	Yes	Yes	197?
Massachusetts	Yes	2012	Yes	Yes	
Michigan	Yes	2008	Yes		
Minnesota				Yes	197?
Mississippi				Yes	2004
Montana	Yes	2004	Yes		
Nebraska				Yes	197?
Nevada	Yes	2001		Yes	
New Hampshire	Yes	2013			
New Jersey	Yes	2009	Yes		
New Mexico	Yes	2007	Yes		
New York				Yes	197?
Ohio				Yes	1975
Oregon	Yes	1998	Yes	Yes	1973
Rhode Island	Yes	2007	Yes	Yes	2012
Vermont	Yes	2004	Yes	Yes	2013
Washington	Yes	1998	Yes		

V. Data Description

The dataset that I used to measure marijuana use was the National Survey on Drug Use and Health (NSDUH), which fit my differences-in-differences model in that it allowed for plenty of variation in time and space. The NSDUH, which is an annual survey sponsored by the Substance Abuse and Mental Health Services Administration (SAMHSA), provides information on the use of illicit drugs, alcohol, and tobacco in the non-institutionalized population of the United States aged 12 or older, with a sample of approximately 67,500 (Harper, 2012). The fact that it is annual fit my model because I was able to examine years before and after medical marijuana legalizations. The data covered the period 2002-2012, or 11 consecutive years of marijuana use outcomes. This time period fit well ñ it was recent enough to capture pre-legalization and post-legalization for 9 out of the 20 medical marijuana states (plus District of Columbia). Since the NSDUH covers all 50 states and District of Columbia, I had 9 treatment groups and 42 control groups in my difference-in-differences model. Finally, multiplying the number of years and the number of states gave me 561 observations of state marijuana use.

The way the NSDUH measures marijuana use and for whom it measures marijuana use fit the purpose of my paper. I was interested in the number of marijuana users, and not the frequency or intensity of marijuana use among past users. The NSDUH provides state prevalence rates for first time marijuana use in the past year, which accurately measures marijuana initiates. It also provides prevalence rates for past month use, which I believe is an indicator of an occasional user. These prevalence rates are also broken down into four age groups: ages 12+ (the general population), 12-17(minors), 18-25, and 26+. This allowed me to run regressions on different age groups, which could have implications for the exclusionary power of the medical card's age requirement, preferences across age groups, and the prevalence of legal-to-black market diversion. This age group breakdown was also valuable for the home cultivation variable, since that is another age-

exclusionary law.

Though the NSDUH fit my model, the survey data has some inherent flaws. First, their state level estimates are derived from Bayesian hierarchical models and are associated with some uncertainty (Harper, 2012). Also, a single prevalence rate will be an estimate based on an average of data from two years. For example, NSDUH surveys are claimed to cover "2010-2011" rather than just 2010 or 2011. This made choosing which year to attribute to a give prevalence rate a personal decision, but ultimately the results were not sensitive to this decision. For independent variables, I simply coded the various marijuana policies as 1 or 0, and use government data on the controls for economic conditions. I coded medical marijuana legalization and home cultivation according to the legal status of each state as listed in Pacula, Powell, Heaton and Sevigny (2013). The listing was verified by legal scholars, and for the most part overlapped with information I found on http://medicalmarijuana.procon.org/. My criteria for decriminalization was the elimination of jail time for carrying up to .8 ounces of marijuana. For decriminalization I found news reports documenting decriminalization laws, as well as the website called NORML.org that linked me to state bills passing decriminalization.

While I considered the legal interpretation of each state's medical marijuana laws accurate, there were still shortcomings in my specification of medical marijuana legalization. As to be expected with a policy that varies across states, the main concern is unobserved heterogeneity. While I could accurately pinpoint medical marijuana legalization, as well as home cultivation, a major component that I was missing was the allowance of dispensaries. There is a shortage of accurate data on the number of dispensaries in each state, let alone the number of dispensaries in each state for every year in my model. As pointed out in the literature, the lag between the passing of a dispensary law and the actual opening of dispensaries in a state is significant and makes a dichotomous dispensary indicator inaccurate. Not being able to pick up the effect of

dispensaries may have led to the overestimation of at least one of the medical marijuana legalization effect and the home cultivation effect.

VI. Results

I started off by running the simple model of the regression for both measures of marijuana use. However, adding home cultivation to the regressions and running them again changed the results. After I added home cultivation to the regressions for the total population, legalization was the only variable for which significance changed. However, there were many changes across age groups after adding home cultivation. While I believe home cultivation was a proper specification, some part of the home cultivation model may be causing erroneous interactions between variables.

Table 3. Estimates of percentage that used marijuana for the first time: marijuana policy

	Age 12+	Age 12-17	Age 18-25	Age 26+
Medical marijuana state	.324**	1.244**	1.587**	.032**
	(.033)	(.112)	(.164)	(.006)
Post medical marijuana law	.091**	0.204	-.005	.018**
	(.036)	(.121)	(.177)	(.007)
Marijuana decriminalization state	.039	.052	.207	-.010**
	(.025)	(.086)	(.125)	(.005)
Post decriminalization	.023	-.016	-.067	.012**
	(.030)	(.103)	(.150)	(.006)
State unemployment rate	-.009	-.068**	-.118**	.003**
	(.008)	(.027)	(.039)	(.001)
State median household income	7.40E-06**	-3.69E-06	4.86e-06**	6.16E-07
	(1.77e-06)	(6.01e-06)	(8.79e-06)	(3.29e-07)
State cigarette tax	.010	.041	.302**	.000
	(.019)	(.066)	(.096)	(.004)
Constant	1.520	7.024	7.063	.087
	(.097)	(.330)	(.482)	(.018)
Fixed year effects	Yes	Yes	Yes	Yes

Table 3 shows the differences-in-differences coefficients for the independent variables tested for their effect on the percentage of first time marijuana users in a given state. The

coefficient on post-medical marijuana legalization tells us the effect of medical marijuana legalization on first time marijuana use. This coefficient ended up positive and significant at the 5% level. At this point, I rejected the null hypothesis that medical marijuana legalization has no significant effect on first time marijuana use. However, legalization was found to have different significance for different age groups. For those ages 12-17, as well as those ages 18-25, legalization's coefficient was insignificant at the 5% level. The only age group for which legalization had a significant effect was the 26 and older group, but the difference that legalization created in this group's first time use was pivotal in making legalization's effect on the total population significant.

Some of the coefficients on the controls had potentially valuable implications. The coefficient for a state having passed a medical marijuana law at any point to this date was positive and significant at the 5% level for all age groups (and would remain so in every regression). This suggests medical marijuana states contain characteristics besides for a medical marijuana law (such as liberal attitudes toward marijuana use) that have a positive and significant effect on the number of first time users in those states. Thus, prior to any exogenous shock, medical marijuana states can be expected to have a significantly higher percentage of first time users than non-medical marijuana states in any given year. Next, the coefficients for belonging to a decriminalization state and the decriminalization law were insignificant on overall first time use, adding support to the idea the theory that penalties on marijuana consumers are largely ineffective.

Table 4. Estimates of percentage that used marijuana in the past month: marijuana policy

	Age 12+	Age 12-17	Age 18-25	Age 26+
Medical marijuana state	1.711**	1.780**	4.989**	1.210**
	(.170)	(.163)	(.453)	(.141)
Post medical marijuana law	.838**	.468**	-.293	1.078**
	(.183)	(.176)	(.489)	(.152)
Marijuana decriminalization state	.210	.302**	1.016**	0.073
	(.130)	(.125)	(.347)	(.108)
Post decriminalization	.487*	.268	.660	.506**
	(.155)	(.150)	(.415)	(.129)
State unemployment rate	0.016	-.140**	.014	.038
	(.040)	(.039)	(.108)	(.034)
State median household income	-3.36e-06	.000	.000	-6.54e-06
	(9.09e-06)	(.039)	(.000)	(7.54e-06)
State cigarette tax	.409**	.357**	1.243**	.266**
	(.099)	(.096)	(.265)	(.082)
Constant	5.204	8.748	13.550	3.190
	(.498)	(.481)	(1.333)	(.097)
Fixed year effects	Yes	Yes	Yes	Yes

Table 4 contains the coefficients for the same set of independent variables on a different measure of marijuana use, this one being past month use. Once again, the coefficient telling us the effect of medical marijuana legalization was positive and significant at the 5% level. Once again at this point, I rejected the null hypothesis that medical marijuana legalization does not significantly affect past month marijuana use. When breaking down the total population, legalization is found to be significant for 12-17 year olds and 26+ year olds. This suggests medical marijuana legalization has no effect on the percentage of 18-25 year olds that try marijuana or use it on a monthly basis.

While the variable of interest (medical marijuana legalization) had the same effect on both measures of marijuana use for the general population, some control variables had different effects on past month use than on first time use. The decriminalization law itself became significant for the total population once I used past month use as the dependent variable, though only the 26+ year olds were affected. This may be attributed

to a 26+ year olds being more risk averse than younger populations. Lastly, the state cigarette tax became positively significant for the total population. Perhaps, increased cigarette taxes lead to people substituting occasional cigarette use with occasional marijuana use.

Since our results suggested that medical marijuana legalization significantly increases the number of marijuana users, both first time and past month, I wanted to see by what mechanism legalization was driving these increases. The dimension of medical marijuana legalization that I wanted to examine was one of its components for which there is variation across states and that happens concurrently with legalization ñ home cultivation. To see if there was any difference between marijuana laws that allow home cultivation and medical marijuana laws that do not, I simply added a post-home cultivation law indicator to our first time use and past month use regressions.

Table 5. Results for first use after adding post home cultivation variable

	Age 12+	Age 12-17	Age 18-25	Age 26+
Medical marijuana state	.326**	1.233**	1.608**	.032**
	(.033)	(.112)	(.163)	(.006)
Post medical marijuana law	.028	.532**	-.641**	.028**
	(.059)	(.200)	(.292)	(.011)
Post medical marijuana law with home cultivation	.080	-.414**	.803**	-.012
	(.060)	(.202)	(.294)	(.011)
Marijuana decriminalization state	.035	.0743	.165	-.009
	(.025)	(.086)	(.126)	(.005)
Post decriminalization	.018	.007	-.112	.013**
	(.030)	(.103)	(.150)	(.006)
State unemployment rate	-.008	-.072**	-.108**	.003**
	(.008)	(.027)	(.039)	(.001)
State median household income	7.38e-06**	-3.59e-06	4.68e-06	6.18e-07
	(1.77e-06)	(5.99e-06)	(8.74e-06)	(3.29e-07)
State cigarette tax	.007	.055	.275**	.000
	(.019)	(.066)	(.096)	(.004)
Constant	1.518	7.031	7.050	.088
	(.097)	(.329)	(.479)	(.018)
Fixed year effects	Yes	Yes	Yes	Yes

Table 5 shows the coefficients for a set of independent vari-

ables that includes post-home cultivation, with respect to the dependent variable of first time use. After adding home cultivation, the resulting coefficient for medical marijuana legalization lost its significance for the total population. It is positively significant for 12-17 year olds and 26+ year olds, which suggests that there is some diversion of marijuana from the legal market to the black market and that 26+ year olds are responsive to legal penalties (meaning I reject my hypothesis for each of these age groups). The fact that these groups were not pivotal in making legalization significant for the entire population suggests that legalization does not increase the number of users in a given state. Though home cultivation itself was insignificant, it seemed to pick up some effects from decriminalization state, decriminalization itself, and unemployment, since they lost their significance for the total population as well. Overall, while adding home cultivation makes medical marijuana legalization insignificant and suggests that we should fail to reject my main hypothesis, there may be some error with this model. This is because the coefficients for medical marijuana legalization's effect on 18-25 year olds, and home cultivation's effect on 12-17 year olds are significant and negative, which is difficult to explain.

Table 6. Results for past month use after adding post home cultivation variable

	Age 12+	Age 12-17	Age 18-25	Age 26+
Medical marijuana state	1.736**	1.778**	5.054**	1.233**
	(.168)	(.164)	(.450)	(.139)
Post medical marijuana law	.093	.539	-2.268**	.410
	(.301)	(.293)	(.806)	(.250)
Post medical marijuana law with home cultivation	.993**	-.089	2.492**	.842**
	(.304)	(.296)	(.813)	(.252)
Marijuana decriminalization state	.160	.306**	.884**	.028
	(.130)	(.126)	(.347)	(.107)
Post decriminalization	.435**	0.273	.522	.459**
	(.155)	(.151)	(.415)	(.128)
State unemployment rate	.0277	-.140**	.044	.048
	(.040)	(.039)	(.108)	(.033)
State median household income	-3.57e-06	.0000	.000	-6.73e-06
	(9.02e-06)	(8.77e-06)	(.000)	(7.47e-06)
State cigarette tax	.377**	.360**	1.160**	.238**
	(.099)	(.096)	(.265)	(.082)
Constant	5.189	8.750	13.510	3.176
	(.494)	(.481)	(1.323)	(.410)
Fixed year effects	Yes	Yes	Yes	Yes

Table 6 describes the same independent variables as Table 5, but with respect to past month use. Once home cultivation was introduced to the past month use regression, medical marijuana legalization lost its significance on the general population (thus I failed to reject my main hypothesis), with decriminalization itself and state cigarette tax becoming significant. Post-home cultivation ended up being positively significant. Looking across the age groups for legalization and home cultivation has interesting implications. For medical marijuana legalization, the insignificant effect for the 12-17 group suggests there is no significant diversion from adults to minors. On the other hand, only the 18-25 and 26+ groups are significantly affected by post-home cultivation, undermining support for the "social availability" argument that being around growers increases the chances of minors using marijuana in the past month. Similar to the more specified model for first use, this model may be flawed since there is a negatively significant co-

efficient for legalization on 18-25 year olds, which is difficult to explain.

VII. Conclusion

Since the literature currently focuses on teens, there is not much study of medical marijuana legalization's effect on the general population or specifically its effect on adult age users. Under legalization, the population over the age of 18 is the group that can gain legal protection to consume marijuana with a medical card. Thus examining legalization's effect on this group should reveal just how much, if at all, legal penalties deter marijuana adult consumers. Also, adults can more easily obtain marijuana, and it may be difficult for law enforcement to practically stop them from distributing or sharing with minors. Thus, legalization's effect on adolescent use has implications for the power of diversion from legal market to black market, and for the effectiveness of age-exclusionary measures.

My results, after distinguishing between medical marijuana laws with a home cultivation component and medical marijuana laws in their essential form, suggest that medical marijuana legalization does not significantly increase the number of marijuana users or past month users in the general population. For the first use measure, legalization was found to be significant for minors and 26+ year olds, suggesting diversion contributed to first time use by minors and that 26+ year olds are sensitive to legal risk. It is in the past month use regression where the limitations of the home cultivation specification are found: a negatively significant coefficient for legalization on 18-25 year olds suggests some error.

One way this study could be improved would be to include a method of controlling for erroneous interactions between different marijuana policy variables. In addition, state-level data on both the first openings of dispensaries, and the number of dispensaries operating in a given year would allow researchers to isolate components of medical marijuana legalization, both supply side and demand side, accurately and assess their ef-

fects on marijuana use.

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The Effect of a 311 Vacant Building Call on Crime Rates

Bharat Chandar and Oliver Dean

Abstract

It is generally accepted among researchers that incidence of crime is on average higher around vacant and abandoned properties because they can serve as safe havens for criminal activity. However, there has been little research investigating the effect of government intervention to rehabilitate vacant and abandoned properties on crime rates. We examine whether crime is reduced in Chicago near vacant and abandoned buildings after the city receives a service request call notifying them that the building is vacant. We find that reduction in crime is minimal following a service request.

I. Introduction

Abandoned and vacant properties have long been a source of economic and political debate in the United States. Historically, property abandonment has been considered a symptom of urban disinvestment and not a cause. However in recent years, many studies have investigated whether there is a casual link between vacant buildings and the crime rate in the surrounding areas. Since these spaces offer an area of low supervision, they can act as a haven to individuals with criminal intent. Consequently, these buildings if left unsecured often evidence acts of prostitution, drug use, and gang-related activities.

This paper seeks to investigate the relationship between a 311 call reporting a vacant building in the Chicago area and any following reduction in the level of crime. Specifically we are interested in the presence of any decrease in crime near a vacant building once the city government becomes aware of its presence and can induce rehabilitation. We make use of crime and property data from the City of Chicago data portal, demographic data from the American Community Survey,

and weather data from the National Climatic Data Center. We use the first difference estimator to address the problem of omitted variable bias. This paper analyzes the outcome of the regression, the implication of the findings, and potential policy relevance. The subsections of the paper are broken down in the following manner. Section II is dedicated to a review of relevant literature. Section III describes the process that follows a 311 call reporting a vacant property in Chicago. Section IV addresses the issue of why our contribution is relevant. Section V describes our empirical approach and model. Section VI provides an explanation of the obtained results. Section VII analyzes our findings. Finally, Section VIII offers some concluding remarks on the topic.

II. Literature Review

A substantial body of literature suggests that the presence of vacant properties induces greater crime in a region. One popular justification for this is the "broken window theory," which asserts that signs of abandonment and disorder encourage further disorder (Kelling and Wilson 1982). An alternative argument with considerable supporting research is rational choice theory, which holds that vacant properties reduce the risks and costs of illegal activity for a prospective criminal since they offer privacy and low supervision (Clarke 1995).

To assess the affect of vacant property rehabilitation on crime rates, our paper mainly draws upon Spelman's 1993 paper "Abandoned Buildings: Magnets for Crime?" and Branas, Rubin and Guo's 2012 paper "Vacant Properties and Violence in Neighborhoods." The premise for our paper relies upon their evidence that vacant and abandoned properties are a significant driver of crime in urban neighborhoods. In addition, we draw on their conclusions that government responses to these properties can result in a tangible reduction in crime. Spelman compares blocks with and without open abandoned buildings in a low-income Austin, Texas neighborhood using a matched pairs design. His results showed that crime rates on blocks with open abandoned buildings were twice as high as rates on

similar blocks without open buildings. He argues that reverse causality is not a factor since the majority of vacant building owners are absentee landlords rather than residents. Finally, he states that 85% of housing inspections in this dataset were conducted as a result of citizens' complaints, and that only 15% were conducted by an inspector on his or her own initiative. However, Spelman's experiment design could have been liable to overmatching and thus statistical bias. Furthermore, as he only included one Austin neighborhood in his work it is dubious that the results can be generalized.

Branas, Rubin, and Guo extend this direction of inquiry and show that increasing levels of vacancy were associated with increased risk of assaultive violence in urban block groups. They achieve this by using a study sample of 1816 block groups in Philadelphia County. A census block group is a smaller geographic unit than a census tract and in their dataset the average block group was 0.07 square miles. The researchers then compiled and linked aggravated assault and vacant property data between 2002 and 2006. Finally, they produced their results by using a mixed effects negative binomial regression model in order to show increasing levels of crime in the presence of vacant buildings. Cui shows in a study of vacant properties and foreclosures in Pittsburgh that this effect is fairly localized, with crime 15% higher within 250 feet of a property than it is between 250 and 353 feet. He also argues that the longer periods of vacancy have a greater effect on crime rates (Cui 2010). This previous academic literature has linked abandoned buildings to the crime rate in the surrounding area. However, there is a significant lack of work investigating the effectiveness of any governmental response to vacant buildings.

In this paper we aim to use empirical evidence and techniques to explore this question thoroughly in the Chicago area. If the Chicago Department of Buildings is effective in its response to vacant properties, we can expect to see a statistically significant decrease in the surrounding areas' crime rates after a 311 call. This is particularly relevant to policy discussions because past research indicates that displacement of crime

is likely limited following reductions in opportunity; indeed, there may be more general benefits of crime reduction through displacement through a "multiplier" or "halo" effect (Gabor 1990; Clarke and Weisburd 1994; Weisburd et.al. 2006). Succesful rehabilitation also has the potential to boost property values, which Han shows diminish in the presence of vacant buildings at an increasing magnitude and in a growing geographic region over time (Han 2013). If building rehabilitation is successful, then there will likely be substantial benefits to the community both around the vacant property and in surrounding areas.

III. Inspection and Rehabilitation Process After a 311 Call

The diagram below shows the process that takes place when a 311 call is placed to report a vacant property in Chicago. First, the building is assessed by a city housing inspector. If the building is deemed in violation of the State Housing Code, the Chicago Building Department sends a notice directing the owner to put the building in a safe and secure condition within 15 days. In the case where the owner cannot be identified, the government sends the notice to the person or persons in whose name the building was last assessed. If the building is not secured within the time limit the Corporation Council applies to the Circuit Court of Cook County for an order that either authorizes the city to demolish, repair, or enclose the property, or requires the owner to do so. Furthermore if the process gets to this stage, the owner is fined between $200 and $1000 per day, for each day from the 16th day until the building is secured or demolished. All costs for the demolition, repair or enclosure of the building are recovered from the owner of the property.

Consequently, we can see that the process regarding a 311 call is strict, and the financial penalties can grow extremely quickly past the 16th day. As such, there is a strong incentive for the building owner to quickly secure the building if they receive a notice from the Chicago Building Department

(Chicago Code 13-12-125).

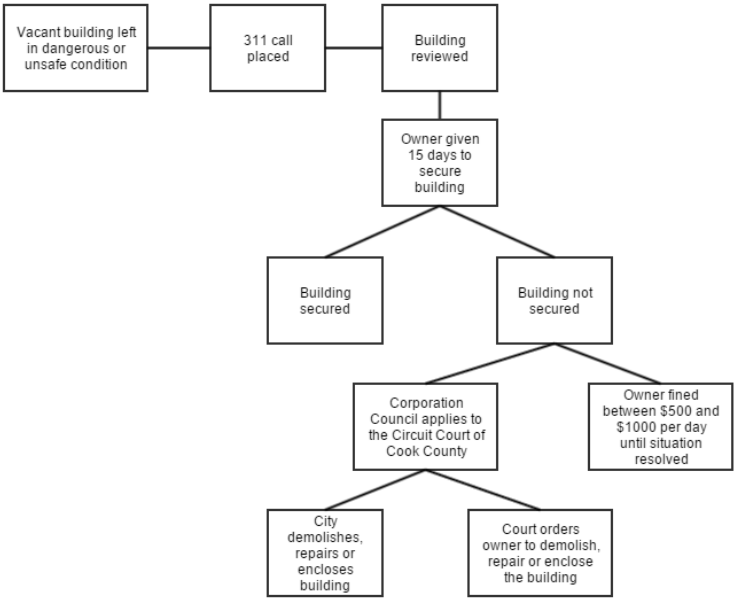

Figure 1: This diagram depicts the process of inspection and rehabilitation following a 311 call.

IV. Relevance of our Contribution

As already stated there is a significant lack of academic literature addressing the effectiveness of the governmental response to a vacant or abandoned building service call. Our paper aims to provide a first step in solving this paucity. However, it must be noted that the reader should be hesitant in generalizing our results for two reasons. First, since government response to a vacant properties is dealt with by localities who base their practices on State Building Codes the methods practiced by the Chicago departments may differ from other departments around the country.

Furthermore, there is a selection bias issue in regards to 311 calls. It is likely that a civilian will only report a vacant

property if it is imposing significant costs on the local community, e.g. crime. This has implications on the interpretation of our findings. In particular we expect crime rates to be higher than average in the time period preceding a service call than it would be for the typical vacant or abandoned property. The effects we observe should be understood as the change in crime around vacant buildings where crime is more likely to already be prevalent.

V. Model and Methodology

In this section we describe a model of the difference in crime rates from the time period before a 311 call is made to the time period after the call is made in some small region around the abandoned property. We estimate the model's parameters using the city of Chicago's crime records from 2010 to the present, block level demographics form the American Community Survey, and temperature data from the National Climatic Data Center. Using these estimates we quantify how much crime is reduced after a 311 call is made.

Description of Model

We consider the crime rate around an abandoned property in a given period of time. We model this as:

$$y_{i,t} = c + \gamma D_{i,t} + \beta^T \vec{X}_{i,t} + \alpha^T (\vec{Q} \times D_{i,t}) + \lambda^T \vec{R}_{i,t} + \eta_i + u_{i,t}$$

where $y_{i,t}$ is the crime rate around abandoned building i over time period t. $D_{i,t}$ is an indicator for whether the time period is post-call or pre-call. X is a vector of controls that remain fixed between the pre-call period and the post-call period. $(Q \times D_{i,t})$ is a vector of interaction terms describing the difference in the effect of some control variables in the pre-call period and the post-call period, and $\vec{R}_{i,t}$ is a vector of controls that evolve over time.

η_i contains unobserved characteristics that affect the crime rate and remain fixed over time. If the components of η_i are

correlated with values controlled for in the regression then the results of simply running a pooled regression of the pre-call crime rates and post-call crime rates would be susceptible to omitted variable bias. There is sufficient theoretical justification to suggest such correlation might exist; for instance, among the control variables in our regression is whether the building is boarded at the time the service call is made. There is reasonable justification to argue this could be correlated with fixed unobserved determinants of crime in the surrounding region. To account for this, we use a first differences regression.

Let time t correspond to the pre-call period and time $t+1$ correspond to the post-call period. We seek to measure $y_{i,t+1} - y_{i,t}$, the difference in crime rate between the two periods. Set

$$\delta_i = y_{i,t+1} - y_{i,t} \iff$$

$$\delta_i = \gamma + \alpha^T \vec{Q} + \lambda^T (\vec{R}_{i,t+1} - \vec{R}_{i,t}) + \epsilon_i$$

$\beta^T \vec{X}$ cancels out upon taking differences because it remains fixed between periods. γ measures the mean difference in crime rate after accounting for controls; if γ is negative this suggests crime rates decrease after the vacant building service call is made. α^T measures the effect of time-fixed variables on the difference in crime rates between periods, and λ^T measures the effect of the difference in time-varying variables on the change in crime rates. We now discuss the components of the model further.

The constant term, γ

The constant term measures the mean difference in crime rates after accounting for controls. We expect this term to be negative based on the literature. After a service call is made, an inspector is sent to the property, and the city has a right to force the building owner to board up or repair the property if it does not abide by housing codes. The city can also demolish the property under certain circumstances. Spelman argues that secured vacant buildings pose little to no greater risk than

non-vacant buildings, so succesful rehabilitation should lead to a reduction in crime rates (Spelman 1993).

Variables included in $\vec{Q}$

These variables effect the difference in crime rates but remain fixed over time. We include demographic characteristics, whether the property is boarded, whether it is occupied, whether the building is vacant due to fire, the police district to which the property belongs, and the number of housing units on the block.

Demographics and Number of Housing Units

Demographics included in our model are the median income, population, percentage of black residents, and percentage of females in the region surrounding the vacant property. The effect of each of these variables is theoretically ambiguous. The sign for median income, for instance, can reasonably be either positive or negative. It is perhaps possible that owners of vacant buildings in high-income areas will be more likely to carry out rehabilitation demanded by the city in a timely manner, or the opposite could be true if costs of rehabilitation are higher. Similarly the effect of the other demographics included in the model and the number of housing units is difficult to determine theoretically.

Whether the Property is Boarded

As the previously mentioned literature suggests, properties that are boarded do not seem to exhibit more crime than non-vacant properties. As a result we expect properties that are not boarded before an inspection is made to have a higher reduction in crime rate because the effects of rehabilitation should be greater.

Whether the Property is Occupied

We control for whether the vacant property is occupied at the time of the service call by gangs, children, homeless, or other individuals. We expect occupied properties will have a higher reduction in crime rate because these individuals will be dispersed by property rehabilitation, eliminating their safe haven for crime.

Whether the Property is Vacant Due to Fire

The change in the crime rate given that the property is vacant due to fire is ambiguous. It could be that the change will be smaller because it is more unlikely that the caller was prompted to notify the city because of higher crime in the time preceding the call. Crime rates might be higher in the region after the call is made because the block is in greater disorder, or the causation could act in the opposite direction; greater disorder leads to higher risk of fire. There is literature that suggests the latter is the case (Socioeconomic Factors and the Incidence of Fire 1997).

Police District

We include dummy variables for each police district in Chicago to determine if there is a difference in the reduction in crime rates across districts. The signs of these variables are theoretically ambiguous.

Variables Included in $\vec{R}_{i,t}$

$\vec{R}_{i,t}$ includes variables that change between the time period before the service call and the time period after the call is made. We include the difference in mean temperature between the time periods and the difference in total crime within the community area the vacant property belongs to.

Difference in Mean Temperature

Research suggests that temperature has a positive effect on crime rates because people spend more time outside of home, increasing the risk of criminal victimization (Field 1992). As a result we expect increasing the difference in temperature will lead to an increase in the difference in crime rates.

Difference in Total Crime within the Community Area

Controlling for the difference in total crime within the community area a property belongs to accounts for the effect of trends in crime within the community as a whole. We expect an increase in the difference in crime at the community level leads to an increase in the difference in crime around a vacant property. We discuss characteristics of a community area further in the following section.

VI. Data

We retrieved data on 311 vacant or abandoned property service calls from the city of Chicago's data portal. The data set contains up-to-date records of all calls dating back to January 1, 2010. At the time of our retrieval of the data in early March there were approximately 48000 data points. Relevant columns included in the data set are the date the request was received, the location of the building on the lot, whether the building is open or boarded, where the entry point is if it is open, whether the building is vacant due to fire, whether anyone is using it, its address, zip code, police district, community area, and latitude and longitude. We discuss some of these variables further in the context of our model, but we first note some important characteristics of how the data is collected. As mentioned in Section IV, there is a selection bias in whether a 311 call is made. Individuals are more likely to notify the city of a vacant property if its existence has a tangible negative impact on the community, for instance by being a safe haven for criminal activity. The effects we observe should be understood as the change in crime around vacant buildings where crime is

more likely to already be prevalent. While this distinction is important, the policy implications of our analysis remain. In particular, our results will determine if rehabilitation by the city leads to lower crime rates near properties where rehabilitation is especially necessary. The differences will likely be in greater in magnitude than they would be absent the selection bias, however. It is also worth noting that according to Spelman 85% of housing inspections in Austin were conducted after a complaint had been filed by a citizen and 15% had been conducted by an inspector on their own initiative (Spelman 1993). This would suggest that inspections resulting from service calls are largely representative of the total number of inspections, but this may have changed considerably over time. Property tax delinquency has been recognized as highly correlated with vacancy and is increasingly being used to identify vacant and abandoned properties (Alexander and Powell, 2011).

There appears to be a pattern in when service calls are made by month. A greater number of calls are made in summer months than in winter months. This is likely because the negative effects of a vacant property are more apparent in the summer, when crime tends to be higher. Figure 1 in the appendix shows a bar plot of service calls by month over the course of our study.This chart also shows that there is considerable variance in service calls by month. These patterns demonstrate the importance of including community-wide effects in our model. Since the number of service calls is not roughly uniform by month, not accounting for these trends in crime can skew results because crime in the pre-call period may be systematically different than crime in the post-call period for reasons unrelated to a 311 call being placed.

We do not currently include the location of the entry point for open houses in our regression because it is coded as a string variable and is difficult to categorize systematically. This could be an area for further research.

Table 1 presents summary statistics for whether the property is boarded, whether it is being used, and whether it is vacant due to fire. Note that there are a substantial number

variable	mean	N
fire	.0761196	39346
bdum	.1132294	39760
occ	.4904788	39596

Table 1: **Property Characteristics**

of missing values between the three variables. In our regressions these missing values reduce our sample size by several thousand.

Two other variables with important implications in our regression are the police district and community area to which the property belongs. We test for differences across different police districts on the difference in crimes between the pre-call period and the post-call period. Figure 2 is a map of community areas and police districts obtained from the city of Chicago. Community areas are groupings of neighborhoods with relatively homogeneous social characteristics. These demarcations were defined in the 1920s and have remained largely static over time. We control for the total number of crimes that occur in the community area over the relevant time periods, as described previously.

Estimating Crime Rates

In order to estimate crime rates we use a method similar to Spelman (Spelman 1993). We compute the number of total crimes in a square of side length 800ft centered around the vacant property for a 90 day time period before and after the service call is made. A 90 day window allows for ample time for the city to inspect and recommend rehabilitation for a property. We exclude 120 days from each endpoint of the sample in order to be able to compute the pre-call and post-call crime rates for each service call. This reduces our sample size to about 45000 calls. We use square boundaries instead of circular boundaries because we believe this better

captures the block structure of neighborhoods and because it was computationally simpler. Our method can also be interpreted as a time- and spatially-varying Poisson model with boxcar kernels in both dimensions. Our crime data comes from the City of Chicago's crime records, available through the Chicago data portal. Crimes include burglary, sexual assault, arson, weapons violations, drug-related crimes, homicide, and many others. Note that the crime statistics do not include cases of domestic violence.

Figure 3 displays the size of the region surrounding the vacant building over which we compute crime rates. Figures 4 and 5 display density estimates of the crimes and service calls across Chicago respectively. We note that service calls are far more concentrated in pockets of the city, particularly the south and west sides of the city. Though not evident in the density plot, there are service calls across the entire city, but the number of calls in the downtown area and north side are so small relative to the other regions that they have little density on the map. This could be because of differences in social and economic characteristics of the regions or because service calls are less likely to be made on apartment buildings, which are more concentrated in the regions with little density. A comparison of these maps mirrors the results of Branas, Rubin and Guo. The number of service requests appears to be highly correlated with incidences of crime (Branas et. al., 2012).

variable	**mean**	**N**
rates1	20.79	45523
rates2	20.44918	45523

Table 2: **Crime Around Vacant Buildings**

Table 2 shows summary statistics for the number of crimes in the region surrounding a vacant property in the periods before and after the service call is made. Note that the mean crime rates are similar, with the crime rate in the period before

the call slightly greater.

variable	mean	N
totalra 1	1967.205	45523
totalra 2	1941.995	45523

Table 3: **Crime Rates in Community Area**

Table 3 displays the mean crime rate in the periods before and after a service call is made in the community area.

Temperature, Demographics, and, Number of Housing Units

Demographics and the number of housing units are taken from 5-year American Community Survey estimates from 2013 at the census block group level. Census block groups are the smallest geographical units for which the bureau publishes sample data. Typically block groups have a population of 600 to 3000 people. This allows them to capture effects that are appropriately local to the regions we constructed around vacant buildings.

variable	mean	sd
medincome	34465.34	18033.11
population	1180.488	510.4444
percblack	.736939	.3661068
sexratio	.5353504	.0687156
numunits	37.26457	16.49339

Table 4: **Demographics Around Vacant Buildings**

Table 4 shows summary statistics for variables taken from the ACS across the locations of the vacant properties. Temperature data is obtained from the National Climatic Data Center. We retrieved the mean daily temperature in degrees Celsius as measured at Chicago Midway Airport for each day

in our sample and computed running averages of the temperature in the 90 day period before and after the service call.

variable	**mean**	**sd**
temper 1	11.05081	8.602828
temper 2	11.63109	8.321897

Table 5: **Temperature in pre-call and post-call periods**

Table 5 shows summary statistics for the mean temperature in the pre-call and post-call periods. Note that the mean temperature in the post-call period is roughly the same as the mean temperature in the pre-call period.

VII. Results

Results from our full model specification are given in Table 6:

Table 6: **Full Model Spec**

	(1) Difference in rates between the post-call period and the pre-call period
diff_tot	0.00857***
	(34.37)
medincome	0.00000478
	(1.86)
b_dum	-0.165
	(-1.29)
using	0.126
	(1.56)
fire	-0.145
	(-0.96)
temp_diff	0.0405***
	(7.71)
population	-0.000126
	(-1.21)
percblack	-0.147
	(-1.09)
sex_ratio	-0.574
	(-0.93)
num_units	0.00311
	(1.01)
Constant	0.130
	(0.34)
Observations	39289

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7 displays displays a regression to determine if there is a difference in effect when excluding the Loop.

Table 7: **Full Model Spec, No Loop**

	(1) Difference in rates between the post-call period and the pre-call period
diff_tot	0.00858***
	(34.39)
medincome	0.00000436
	(1.68)
b_dum	-0.176
	(-1.38)
using	0.125
	(1.55)
fire	-0.145
	(-0.96)
temp_diff	0.0404***
	(7.67)
population	-0.000127
	(-1.21)
percblack	-0.144
	(-1.08)
sex_ratio	-0.581
	(-0.94)
num_units	0.00265
	(0.86)
Constant	0.163
	(0.43)
Observations	39238

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8 displays a reduced form specification:

Table 8: **Reduced Spec**

	(1)
	Difference in rates between the post-call period and the pre-call period
diff_tot	0.00859***
	(34.43)
b_dum	-0.163
	(-1.28)
using	0.125
	(1.55)
fire	-0.141
	(-0.93)
temp_diff	0.0402***
	(7.66)
medincome	0.00000597*
	(2.57)
Constant	-0.360***
	(-3.57)
Observations	39289

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Tables 9 and 10 display regressions with robust standard errors:

Table 9: **Full Model Spec Robust**

	(1)
	Difference in rates between the post-call period and the pre-call period
diff_tot	0.00857***
	(32.85)
medincome	0.00000478*
	(2.19)
b_dum	-0.165
	(-1.32)
using	0.126
	(1.55)
fire	-0.145
	(-0.96)
temp_diff	0.0405***
	(8.24)
population	-0.000126
	(-1.31)
percblack	-0.147
	(-1.29)
sex_ratio	-0.574
	(-0.91)
num_units	0.00311
	(1.06)
Constant	0.130
	(0.35)
Observations	39289

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 10: **Reduced Spec Robust**

	(1)
	Difference in rates between the post-call period and the pre-call period
diff_tot	0.00859***
	(32.92)
b_dum	-0.163
	(-1.31)
using	0.125
	(1.54)
fire	-0.141
	(-0.94)
temp_diff	0.0402***
	(8.19)
medincome	0.00000597**
	(3.01)
Constant	-0.360***
	(-3.62)
Observations	39289

t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Full Specification

It is immediately apparent that many of our controls appear to have an insignificant effect on the difference in crime rates.

Boarded and Occupied Houses

One of the most interesting findings in this regression is that both the dummy for whether the property is boarded at the time of the service call and the dummy for whether it is being used by other individuals are insignificant. This runs in contrast to much of the literature and is somewhat counterintuitive. It would perhaps be expected that open or occupied buildings would exhibit greater reductions in crime rates by dispersing criminals from safe havens. One reasonable justification for why we do not observe this is selection bias. It could be that service calls are overwhelmingly made in cases where the vacant property has a direct negative impact on the sur-

rounding neighborhood, particularly through being a magnet for criminal activity. Consider a scenario where the probability that a service call is made is some increasing function of the number and severity of crimes surrounding the property. In particular if there is some threshold of crime incidence past which the probability of someone reporting the property increases dramatically, there may be little difference between crime rates for properties that are boarded versus open. Due to the selection bias in both scenarios crime might be comparable and building rehabilitation similarly beneficial. The same holds true for occupied houses versus ones that are not being used.

Another possible explanation for the insignificance of whether the property being used by other individuals follows from the broken window theory. It could be that the existence of vacant properties that people can enter freely is emblematic of general disorder in the surrounding area. In particular, it might be that in such cases dispersing people from the vacant property fails to reduce crime rates overall because there may be other safe havens in the area where they can conduct illegal activity. Alternatively, it could be the use of a property by other individuals is a sign of serious neglect by the owner and could be correlated with greater delay and delinquency in rehabilitation efforts. A lack of cooperation from the owner could stunt efforts to secure the property.

Finally, we note the summary statistics in table 1 again. There is not very high variance in these dummy variables, so more data may be needed to find significant results. Additionally a large number of values were missing. If the missing values are systematically correlated with other determinants of crime they could skew the results.

Fire

There does not appear to be a significant effect of whether the building was vacated due to fire on the change in crime rates. This conforms to our beliefs that the effects were theoretically ambiguous.

Demographics and Other Block Group Characteristics

Population, the percentage of black residence, the gender ratio, and the number of housing units on the block were all insignificant. There is little justification to believe these variables should drastically lead to changes in crime rates following a service call in one direction or the other, particularly after accounting for income. We later consider a model where several of these factors are removed from the regression in order to reduce the variance of key estimators.

Median Income

We find that median income is nearly significant at the 5% level and the coefficient is positive. A higher median income could possibly leads to an increase in the post-call crime rate relative to the pre-call crime rate. This could be because higher income regions exhibit less crime in general, so the returns on rehabilitation are lower. However, the results are ambiguous.

Difference in Crime in the Community Area

As expected, the difference in crime in the community area between the post-call and pre-call periods has a significant effect on the difference in the vicinity of the vacant property. An increase in the signed difference in total crimes in the community area by 100 leads to an increase in the expected difference around the property by .857 crimes.

Difference in Mean Temperature Between the Time Periods

The difference in mean temperature also a has a significant effect. A higher mean temperature in the post-call period relative to the pre-call period leads to a higher crime rate in the post-call period relative to the pre-call period. This is consistent with findings in the literature (Field 1992). An increase in the difference by 10 Celsius degree leads to an increase in the signed difference in crime rates by .405 crimes.

Constant term

The constant term is insignificant. After accounting for other factors there does not appear to be a significant difference in the post-call period and the pre-call period. This suggests that the city's response to vacant property service calls is either inadeuqate or that rehabilitation of vacant properties has little effect on crime rates. However, we note that many of the variables in the model were insignificant, so confidence intervals and significance tests may not be accurate. We consider several reduced form models to reduce the variance in our coefficients. Still, these results are particularly intriguing because of the presence of selection bias. The vacant properties in our sample probably have greater negative impacts on communities on average than the typical vacant or abandoned property, and even in these cases the city's response does not seem to reduce crime rates by a substantial amount in the full model specification.

Differences across Police Districts

We ran a regression under the full specification with added indicators for each police district. We examine whether the difference in crime rates varies by police districts. The excluded police district in the regression is district 1, which contains the Loop and downtown area. Nearly every police district is significant, but their confidence intervals all overlap. This suggests that the change in crime rates resulting from a service call in the loop area is significantly different than the change in crime rates in all other areas of Chicago. In particular, the signed difference in crimes in the Loop is roughly two crimes greater than in every other part of Chicago. This likely because the overwhelming number of properties in the Loop are high-rises and there is quite a bit of traffic and activity in the streets, so it is less likely a vacant building would become a safe haven for criminal activity.

This seems to have little impact on our full model specification because only a small number of service calls occur in the

Loop. Running the full specification again without the Loop leads to nearly identical results except that median income is no longer close to significant. The results are shown in Table 7. It stands to reason that excluding calls in the downtown region would have a significant impact on the median income coefficient since incomes in the Loop are on average much higher than in other parts of the city.

Reduced Form Model

We next consider models where we exclude the demographic indicators that had an insignificant effect on the change in crime rates. Removing these variables should decrease the variance in our estimates.

Table 8 shows the regression results when we exclude population, the percentage of black residents in the block group, the gender ratio in the block group, median income, and the number of housing units. We observe that the constant term is now significant. In the post-call period there is a mean reduction in crime rates of approximately .153 crimes compared to the pre-call period. This is rather small relative to the mean crime rates for each period, which is around 20. These results change very little if we exclude the Loop.

Heteroskedasticity

We use a Breusch-Pagan test to check for heteroskedasticity in our full model specification. The test is barely insignificant at the 5% level. We run the model with robust standard errors and display the regression results in Table 9. We note little difference in results. Table 10 shows the reduced form model regression results with robust standard errors.

VIII. Discussion

We note some limitations of our model. We do not account for other potential vacant properties in the vicinity of the property for which a service request is made. The presence of other

vacant properties likely has a significant impact on the change in crime rates. If there are many other vacant properties in the vicinity it may be less likely that crime will be dispersed from the area because there are other safe havens where individuals can evade supervision. If this is the case rehabilitation will have less of an impact. Furthermore, rehabilitation could have greater positive externalities in regions with a lot of vacant properties, so the net reduction in crime could be greater. Including other service calls made in the same region and relevant time periods would be a very imperfect proxy to account for these effects because it fails to include all vacant properties for which a service call is not made. Further, the vacant properties it does include also face inspection and rehabilitation, so the interpretation of the coefficients is somewhat unclear and likely not entirely relevant to addressing our concerns.

Another consideration is the choice of bandwidth parameters and the kernel function in estimating crime rates. Determining appropriate bandwidths nearly always poses challenges. There may be time frames that are more appropriate for capturing the effect of a service call, and there may be geographic regions better suited to measuring the impact on crime of a service call. Similarly there may be kernel functions more appropriate for modeling this problem than a boxcar kernel. There could conceivably be value in weighting crimes closer in time and space to the service call more heavily. Greater experimentation with these parameters is an potential area for further research.

Finally, we again note the selection bias in our results. It is unclear to what extent inspection and rehabilitation elicited by service calls is representative of the rehabilitation process in general. Inspections based on property rate delinquency could be systematically different. Examining these effects in such cases would be an interesting avenue for further research.

IX. Conclusion

A substantial body of literature suggests that the presence of vacant properties induces greater crime in a region. However,

there is little research into the success of city efforts to rehabilitate vacant properties in order to make them less susceptible to crime. We analyze the change in crime rates between the time period before a service call is made to the city to inspect a property and the time period after the call is made to determine the effect of rehabilitation on crime rates. We show that the rehabilitation has little effect on crime rates.

This raises the question of why there is not a greater reduction in crime rates as a result of a service call being made. Two possibilities are that the city's efforts for rehabilitation are inadequate or that rehabilitation does not have a significant effect on crime rates for properties for which a service call is made. The latter explanation does not seem to be well-supported by literature, which for instance shows that crime is much lower near boarded vacant properties than near unboarded vacant properties (Spelman 1993). One possibility is that for most service calls rehabilitation is not necessary at all, but it seems unlikely that there would be a large number of service calls for properties that pose no danger. Another possibility is that the process of rehabilitation takes far longer than the time period we consider due to noncompliance from property owners. This is certainly a possibility, but given the rights the city has to force rehabilitation on properties that pose immediate danger, we expected to find greater significance in our results. Finally, it could be the case that the resources of the city are too strapped to adequately deal with the large number of service requests. This is perhaps the most plausible explanation for why we do not observe greater reductions in crime rates.

While our results provide a first step in analyzing the success of municipal governments in addressing the problem of vacant properties, there is certainly further research needed both to verify our results and determine potential causes for our findings. Further inquiry into rehabilitation of vacant buildings is invaluable for determining the best ways for cities to allocate resources to fighting crime.

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Appendix

Figures and Plots

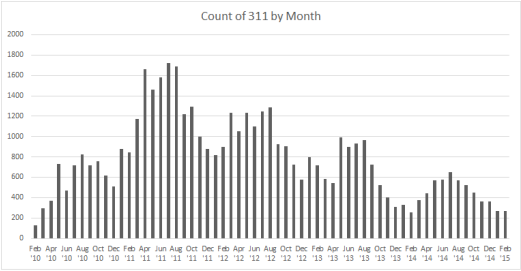

Figure 2: This plot shows the number of 311 service requests for vacant and abandoned properties for each month included in our study.

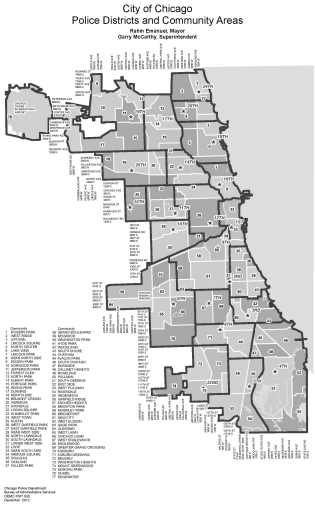

Figure 3: Map of the community areas and police districts in Chicago. Retrieved from the Chicago Police website.

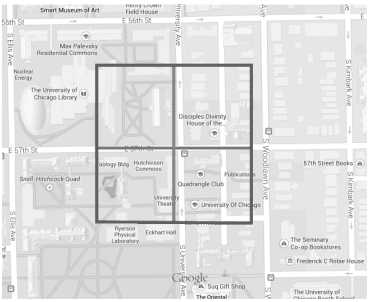

Figure 4: An example illustrating the dimensions of the region around a vacant property we use to compute crime rates.

Figure 5: A plot of the density of service requests for vacant and abandoned properties around Chicago over the time period of our study.

Figure 6: A plot of the density of crime incidence around Chicago over the time period of our study.

Does Trade Liberalization Lead to Declining Fish Stock Health? Analyzing the Causality

Erin K. Glenn

Abstract

This paper explores whether or not the level of international trade openness affects the health of fish stocks in national exclusive economic zones (EEZ). I look for an answer to the question if trade openness leads to a decrease in fish stock health. Additionally, since the level of national regulation is so important when considering the trade of environmental renewable resources, I also seek to examine whether or not there are differences in the effect of increased openness for countries with higher regulatory levels from countries with lower levels. Theoretical literature on international trade theory and renewable resources presents two popularized and conflicting predictions. Chichilnisky's (1994) "resource haven hypothesis" predicts that liberalized trade in countries with "open access" to the resource, or lower levels of regulation, leads to a decrease in its health and Brander and Taylor's (1997) "severe overuse hypothesis" that trade liberalization leads to the realization of a comparative cost advantage in these countries, which actually leads to a remedial effect on the resource's health. The results of my study find that trade openness has a positive effect on the health of national fish stocks, and does not find a statistically significant difference between countries with different levels of governmental regulation, thus refuting the prediction of Chichilnisky (1994) and generally supporting that of Brander and Taylor (1997).

I. Introduction

As the global population continues to rise, especially in highly concentrated coastal urban areas, the consumption, farming and trade of the world's fish supply becomes an increasing

concern. Since the 1960s the Food and Agriculture Organization of the United Nations (FAO 2014) estimates that the global per capita fish consumption has risen from 9.9kg to 19.2kg in 2012. Consequently, an increased demand for fish for consumption stimulated global fishery production to skyrocket. This phenomenon has coincided with the increase in international fish trade and the simultaneous decline in the state of the world's fish stocks.

The increase in global demand for fish, compounded by advancements in the way in which fishery products are caught, stored and marketed, has lead to significant expansion in fishery trade over recent decades. The FAO (2014) estimates that during the period from 1976 to 2012 the share of national catch entering the international market increased from 25% to 37%, and the total fish trade more than quadrupled. This increasing trend in both size and globalization of the fish industry has been coupled with a decline in the global health of fish stocks. Compared to estimates in 1974, when 90% of stocks were at a biologically sustainable level,[1] recent studies by the FAO (2104) estimate that number to only be around 71.2%. In other words, the proportion of overfished marine stocks in 1974 almost tripled from 10% to nearly 30% in 2011. When considered together, these trends raise an interesting question about their correlation and have many wondering if a causal relationship exists. This relationship provides the motivating question for this study: does trade openness negatively affect the health of fish stocks?

Unlike most environmental resources, fish stocks aren't grounded entities, but rather are migratory populations making it relatively difficult to measure their supply at a given point in time. Thus, in order to measure and regulate the open access issue of the world's fish supply, the United Nations Convention on the Law of the Sea (UNCLOS) grants either coastal

[1]The FAO (2014) defines this level to be only the surplus production is caught. A stock's surplus is the amount of biomass that exceeds the necessary replacement level occurring through natural death; the amount of fishing that can occur while still maintaining these levels is known as the stock's Maximum Sustainable Yield (MSY).

nations or regional organizations the sole right to exploit and manage resources contained within their regional basis and have divided the ocean into two different sectors in order to do so. These areas are commonly referred to as "EEZs" and "RMFOs." Exclusive Economic Zones (EEZs) are the areas spanning the world's ocean that lie within a 200 nautical mile range of coastal countries and are controlled by individual nations (OECD, 2016). 'High seas fish stocks' are those located outside of EEZs, in regions controlled by 'regional fisheries management organizations' (RMFOs).

In order to discuss management, catch practices, existing regulations etc. of fish stocks, most empirical studies considering the relationship between overfishing and international trade tends to focus on either EEZs or RMFOs. I focus on EEZs in my study because they include nearly all continental shelf area, account for 90% of all global catches, and provide the vast majority of natural resources, recreation, livelihoods and other benefits to humans (FAO, 2014). Additionally, since my study focuses on the relationship between international trade and fish stock health, using the data regarding the fishing practices in RMFOs, which are not under direct control of individual national governments, does not relate to my study.

Weighing the different effects of endogenous factors, and applying them carefully to the complicated framework of fish as a natural resource, is crucial; specifically, due to the importance of a country's degree of "open access" to renewable resources, I use a model allowing for the endogeneity of governmental regulation in my study. Although the effects of increased international trade on the environment have been well explored, the overwhelming majority of empirical papers have focused on how openness to international markets affects pollution levels to assess the environmental consequences of international trade. However, compared to this rapidly expanding literature, that on trade of renewable resources is a relatively new field of study, mainly consisting of theoretical papers. This literature is the result of rapidly declining fish stock health in the 1990s and has a special focus on compar-

ative advantages stemming from differences in property rights across trading entities; two prominent, yet, conflicting theories emerged. The theoretical hypotheses of Chichilnisky (1994) and Brander and Taylor (1997) offer two competing predictions addressing my motivating question.[2]

First, Chichilnisky's (1994) "resource haven hypothesis"[3] suggests that free international trade in nations with open access to fish stocks stimulates overuse, and has helped exacerbate the decline in the health of global fish stocks witnessed over the last several decades. Chichilnisky (1994) argues that countries with lax regulations for a common-property resource gain a "false comparative advantage" in the industry, and that liberalized trade will lead to overexploitation of the resource. Conversely, Brander and Taylor's (1997) "severe overuse hypothesis"[4] suggests that open trade in these areas has had a beneficial effect on the fish stock populations. Their argument being that in a closed economy, nations lacking regulation tend to overuse the resource, eventually causing the cost of fishing to become expensive enough such that these areas develop a comparative cost disadvantage. Thus, liberalized trade can dilute this overuse, as these countries become relatively unproductive in the open market, and become importers of fish.

[2]See Figure 1 for an illustration of the two hypotheses.

[3]This term is derived from the "pollution haven hypothesis" which captures the same mechanism for polluting industries relocating to less regulated countries as a result of trade liberalization and introduced in the theoretical paper of Antweiler et al. (2001).

[4]This term is the term that Brander and Taylor (1997) use in their theoretical paper.

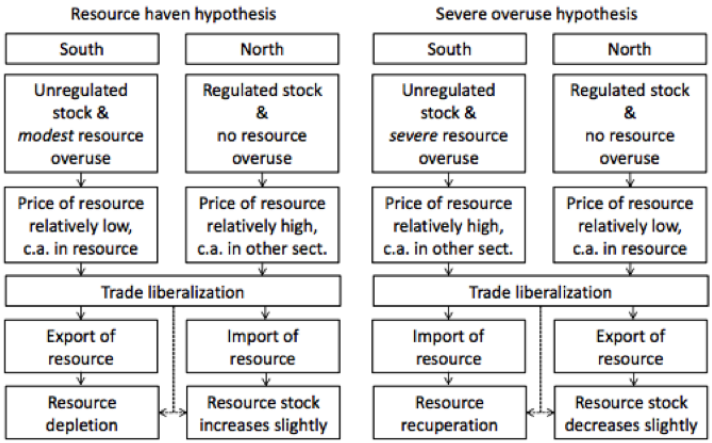

Figure 1: Theoretical Hypotheses of the result of Trade liberalization considering 2 economies, the North and the South. The "resource haven hypothesis" is the prediction of Chichilnisky (1994) and the "severe overuse hypothesis" on the right is that of Brander and Taylor (1997).

In this paper I test these theories with a fixed effects estimation model using calculated scores by the Ocean Health Index (OHI) to measure the health of fish stocks in national exclusive economic zones (EEZ). To my knowledge this is the first study to use a dependent variable that measures the state of a country's fish stock in "beneficial" terms, rather than using the proportion of *over* used stocks.[5] As a result the interpretation of my results is completely different than that of existing studies, as any variable with a negative coefficient in my model can be interpreted as "bad" for the environment, and vice versa. Additionally, as far as I know, my study is the first to account for both the catch in wild-caught fisheries as well as the harvest in mariculture,[6] which I believe to be

[5]McWhinnie (2009), Costello et al. (2008) and Erhardt et al. (2014) conduct similar studies however they use the share of collapsed and overused fish species-stocks as a dependent variable.

[6]The empirical works mentioned above only include catch from wild-caught fisheries in their analysis and do not incorporate mariculture catch in their data.

a far more accurate measurement of the health of national fish stocks because over 17% of the global yield of aquaculture comes from mariculture (FAO, 2014).[7]

Overall, the fixed effects estimations I use to capture the endogeneity of trade openness suggests that increased international trade does have a positive effect on a country's fish stock health. Additionally, when testing the competing predictions of the marginal effects of trade openness in countries with different levels of regulation, while controlling for any possible country-specific income, population and governmental effects, I found that that stock health does not significantly depend on the relative level of governance, refuting the prediction of Chichilnisky (1994).

II. Literature Review

Since the early 1990s there have been numerous empirical investigations considering the relationship between pollution and income; specifically, on how openness to international markets affects pollution levels, to assess the environmental consequences of international trade.

In their empirical study of the effects of increased openness to trade on a country's level of pollution, Frankel and Rose (2005) identify a variety of potential causal relationships that can exist between trade and the environment (Figure 2). Their paper uses geographical variables as exogenous determinants of trade to isolate the effect of openness and they find that trade does have a beneficial effect on three measures of air pollution. Additionally, they argue that the larger effect stems from income variables supporting the Environmental Kuznets Curve (EKC), introduced by Grossman and Krueger (1993), which argues that growth is harmful to the environment at low

[7]The term aquaculture refers to the cultivation in water of products for human consumption, which, depending upon the particular species may be done in freshwater, brackish water or salt water. Mariculture refers specifically to cultured food production in marine or brackish waters using floating cages, net enclosures, natural or artificial ponds, or closed circulation water systems.

levels of GDP and beneficial at high levels, supporting their "gains from trade theory" that openness to trade accelerates the growth process. Holding constant for income, openness as measured by the ratio of trade to income is estimated to reduce air pollution for most pollutants, but the opposite result emerges for CO_2: openness is estimated to worsen emissions, at any given level of income, with a moderate level of statistical significance. This suggests fears that competitiveness concerns will engender a "race to the bottom" in regulation are more justified in the case of carbon.[8]However, the analysis of increased trade of renewable resources and its effects on the environment differs from the well-explored area of study involving the environmental repercussions of international trade as measured by pollution levels.

[8]The contribution of that study is that it addresses the problems of causality that are likely to follow because trade is endogenous, and income too. It focuses on exogenous variation in trade across countries attributable to factors such as geographical location. When the statistical technique (Instrumental Variables) corrects in this way for the endogeneity of trade and income, it finds qualitatively that trade and growth (at higher levels of income) both tend to be beneficial for measures of national air pollution, but detrimental for emissions of CO_2.

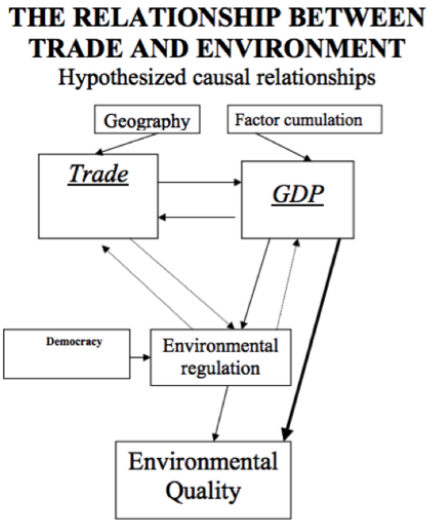

Figure 2: Hypothesized Causal Relationships occurring from international trade of environmental resources.

Barbier and Bulte (2004) provide a theoretical overview of the field of trade and renewable resources, and discuss the potential results of trade liberalization on welfare and resource conservation. They argue that analysis of trade and renewable resources is unique for three reasons: the importance of the stringency of resource management regulations; the environmental consequences extending beyond resource extraction (i.e. biodiversity, habitat conversion, etc.); and the intrinsic dynamism of natural resource management, with stock size changing overtime in response to over harvest or recuperation. The theories of the 1970s and 1980s followed the expansion of optimal control methods in the field of resource economics, thus, most of the literature on trade and resource management assumed there to be a "benevolent planner" with total property rights. From this perspective, open trade can only increase total welfare from a state of autarky, however, it may be harmful for the health of renewable resource stocks *locally*. As a result, Barbier and Bulte (2004) note that early literature on fisheries tends to find an inverse relationship between the

price of a specific species of fish and its stock health. Thus, from this point of view, trade liberalization negatively affects national fish stock health when global prices exceed domestic ones (and vice versa), and trade barriers are good for dwindling stocks.

However, generated by problems in fish stocks, literature on the effect of trade liberalization on renewable resources in the 1990s changed to incorporate a special emphasis on differences in the level of access to a resource across trading parties.[9] As a result, the unanimous conclusion that trade liberalization always improves welfare also changed, depending on the relative level of government regulation in the industry. Gordon (1954) introduced the popular "open access" hypothesis with his argument that overfishing is caused by the economic phenomenon occurring when a resource is of "open access," namely due to lax regulations. The majority of literature on this subject in the context of global fish stocks, considers "open access" to be the root cause in overfishing, implicating that increased trade liberalization only exacerbates this issue. As countries with poor regulations fish their stocks into overexploitation to fulfill international demand, envious well-managed fisheries of other countries will press for less regulation and be more likely to engage in less eco-friendly methods. Thus, many economists argue that the ocean's depletion is due to the lack of property rights to the resource.

According to international trade theory, countries specialize in industries based on comparative cost advantages that result from a variety of reasons such as a nation's "factor" or "natural" endowment, production technology and level of regulation in the industry. Specifically, resource regulations and endowments as sources of comparative advantages in international trade of renewable resources are presented by the theories of Chichilnisky (1994) and Brander and Taylor (1997). Chichilnisky (1994) focuses on the effects of trade liberalization between countries with different regulation policies, taking endowment of a resource as given. She argues that en-

[9] Barbier and Bulte (2004) and Nielsen (2009).

vironmental resources with "ill defined" property rights and liberalized trade lead to overexploitation of the resource, implicating that the increase in international trade should lead to the decline in fish stocks globally, thus supporting Gordon's perspective. Chichilnisky (1994) drew attention to the fact that the assumption of secure property rights is unrealistic for many resource-exporting developing countries. Chichilnisky (1994) shows that, despite the fact that neither the North nor the South has a real comparative advantage in producing the resource-intensive good, the lack of property rights for a common-property resource in the South leads it to produce and export resource-intensive goods in the steady state.[10] In other words, the country with weak property rights gains an apparent comparative advantage, but this advantage does not necessarily lead to greater welfare gains, and certainly not resource conservation, from trade.

Conversely, Brander and Taylor (1997) find that when analyzing the trade of an environmental resource with "open access," it is when the resource stock is in most jeopardy that liberalized trade in fact aids the recovery, rather than the overexploitation, of this resource. Brander and Taylor (1997) argue that in a country with an overused resource and poor regulations, the price on the closed market for the good must increase and the country develops a comparative cost disadvantage. Thus, if it is true that countries with lower regulation and lower quality of government have a tendency to overfish, opening trade in these areas will have a remedial effect on the fish stock. Brander and Taylor call this the "severe overuse hypothesis" suggesting that open trade can dilute this overuse as nations with dwindling stock levels specialize away from fishing these stocks, and thus become importers of fish allowing their national level to avoid complete exploitation or even allow for recuperation. Brander and Taylor (1997) demonstrate the potential welfare effects of trade liberalization with open access resources in both a partial and general equilibrium

[10]See the left hand side of Figure 1 for a visual depiction of Chichilnisky's (1994) theoretical hypothesis.

setting. Under autarky too much harvesting takes place and opening up for trade makes matters worse for those countries that are resource abundant and experience a rise in the terms of trade. In the long run, under certain conditions, a country that exports resources initially may experience declines in welfare compared to autarky.

The literature on the relationship between trade and the environment has varied in terms of approach and content over the past several decades from largely theoretical papers to studies more empirically based. Three basic categories of the impacts of liberalized trade on the environment have emerged that consider the interactions between scale, composition and technique effects created by different characteristics and trading opportunities across countries.[11] The scale effect of trade openness increases environmental damage through more intensive production; the technique effect increases cleaner production processes due to increased demands for environmental quality as national income level rises; and the composition effect, which is the effect my study focuses on, shifts production between environmentally damaging or beneficial goods depending on competitive advantages existing between trading partners.

My empirical study is similar to the recent empirical paper of Erhardt, et al. (2014), and is to my knowledge the only other existing empirical attempt to explain the state of the fish stocks in national EEZs by socioeconomic country characteristics like openness or national income. However, as mentioned earlier, my study uses more recent data, slightly different income controls, a new control for population pressure, a different index for the quality of governance and includes fishing in mariculture.

[11]This framework is used in the works of Antweiler et al. (2001) and Copeland and Taylor (2004).

III. Methodology and Data

Empirical Model (1):

In order to analyze the effect of international trade on the health of a nation's fish stock ($score_{it}$), I estimate a linear fixed effects (FE) model:[12]

$$\begin{aligned} score_{it} = & \beta_0 + \beta_1 open_{it} + \beta_2 \log(gdp\_cap_{it}) + \beta_3 \log(gdp\_cap_{it})^2 + \\ & \beta_4 \log(coast\_dens_{it}) + \beta_5 \log(pop\_dens_{it} + \beta_6 eu_{it} + \\ & \beta_7 gov_{it} + \varepsilon_{it} \end{aligned} \tag{1}$$

Interpretation:

Effect of Trade Openness on Stock Health:
$= \beta_1$

The dependent variable, ($score_{it}$), indicates the stock health in a given country's EEZs evaluated for 2014, as estimated by the OHI's current status score for both mariculture and wild-catch. The first variable of interest, ($open_{it}$), measures a nation's openness to trade as the sum of exports and imports of goods and services measured as a share of GDP. The variables $\log(gdp\_cap_{it})$, $\log(gdp\_cap_{it})^2$, $\log(coast\_dens_{it})$, $\log(pop\_dens_{it})$, gov_{it} and eu_{it} are explanatory variables controlling for potential country-specific income, population and governmental effects.[13]

The income controls are the natural log of per capita income and the natural log of the per capita income, squared, for each country (i) in 2014 (t). Per capita income is defined as a country's real 2014 GDP divided by its total population,

[12] For a detailed description of the purpose and source of my variables see Table 5 in the Appendix.

[13] Initially I included another control for governmental effects, the variable dem_{it}, as a measure of how democratic the structure of a country's government is, ranging from 0 (strongly autocratic) to 10.00 (strongly democratic). However after including a more extensive measure of governmental quality, which takes into account democratization, I believe that I was over controlling for country-specific governmental effects.

and measured in real purchasing power parity adjusted dollars (PPP GDP). The motivation for my population controls is to take into account the possibilities a higher overall population density decreases the health of a country's fish stocks for reasons unrelated to trade openness. My intention for including the log of the coastal density in particular is to allow for the possibility that high population density near the coast can negatively effect fish stock in a country's EEZs by means of increased litter, air and water pollution and habitat destruction, all of which are unrelated to trade openness. The variable gov_{it} is a proxy measuring the overall quality of governance within a given country. Other empirical studies have used different measures to separate democratization and governmental strength as explanatory variables.[14] However, the index I use for each region's EEZs, gov_{it}, which is provided by the OHI, includes scores for government strength and level of democracy, so I believe including an additional variable for democratization would lead to over controlling.[15] Again, using stock health, rather than the level of overfished or collapsed stock, will lead to different interpretations of my explanatory variables in this model.

Empirical Model (2):

$$dummy\_gov_{it} = \begin{cases} 1 & if gov_{it} > Median(gov_{it}) \\ 0 & if gov_{it} \leq Median(gov_{it}) \end{cases}$$

$$\begin{aligned} score_{it} =& \beta_0 + \beta_1 open_{it} + \beta_2 \log(gdp\_cap_{it}) + \beta_3 \log(gdp\_cap_{it})^2 + \\ & \beta_4 \log(coast\_dens_{it}) + \beta_5 \log(pop\_dens_{it} + \beta_6 eu_{it} + \\ & \beta_7 gov_{it} + \beta_8 dummy\_gov_{it} * open_{it} + \varepsilon_{it} \end{aligned} \tag{2}$$

Interpretation:

Marginal Effect of Trade Openness on Stock Health:

[14] Frankel and Rose (2005) as well as Erhardt et al. (2014) have used this control.

[15] See Table 5 for a complete description of how this proxy is calculated.

If $gov_{it} \leq Median(gov_{it}) = \beta_1$
If $gov_{it} > Median(gov_{it}) = \beta_1 + \beta_8$

The motivation of the interaction term, ($dummy\_gov_{it} * open_{it}$), looks for empirical support of the competing theoretical hypotheses presented by Chichilnisky (1994) and Brander and Taylor (1997).[16] The "resource haven hypothesis" assumes that highly regulated countries have healthier fish stocks and vice versa, and that relaxed regulation in a country creates a false comparative advantage, leading to more overuse. Conversely, the "severe overuse hypothesis" argues trade openness will lead to healthier stocks, and will have a larger effect in countries with poor regulatory policies.

Recognizing that interpreting coefficients on an interaction term between two variables of different scales can be misleading, like $gov_{it} * open_{it}$,[17] I generated a dummy variable which divided the data by the country's government strength, labeling countries with scores below the median as less regulatory than those with scores above the median. I then interacted the dummy variable with trade openness so that the coefficient on the open term, (β_1 when $dummy\_gov_{it} = 0$), can be interpreted as the effect of trade openness when governments are weak. Thus, for stronger governments the effect of trade openness can be interpreted as sum of the coefficients on openness and the interaction term ($\beta_1 + \beta_8$ when $dummy\_gov_{it} = 1$). I use an F-test, testing the null hypothesis that the sum of these coefficients equals zero, to measure the significance of the interpretation.[18] However, it is important to note that this test only implies statistical *difference* from weaker governments if the coefficient on openness and that on the interaction term are both significant in themselves.

Thus, according to Chichilnisky's (1994) "resource haven hypothesis," the coefficient on the open term in countries with

[16] See Figure 1 for a depiction of the competing hypotheses.

[17] Originally I had used $gov_{it} * open_{it}$ as my interaction term, however the effect of liberalized trade was difficult to interpret for different levels of government and highly sensitive to any changes to the control variables.

[18] See Table 4 for the results of these F-tests.

relaxed regulations (β_1 when $dummy\_gov_{it} = 0$) should be less than, and statistically different from, the effect of openness when considering all countries in the sample (β_1 in Empirical Model (1)), and that when considering countries with more stringent regulations ($\beta_1 + \beta_8$ when $dummy\_gov_{it} = 1$). Empirically this means that according to Chichilnisky (1994), when the interaction term is included, β_1 should be less than $\beta_1 + \beta_8$, and less than the value of β_1 when the interaction term is not included. This would indicate that an increase in openness stimulates increased over fishing. However if is not the case, and the marginal effects for countries with different levels of regulation are not statistically different or demonstrate higher stock health in areas with lax regulation levels, the results would support Brander and Taylor's (1997) "severe overuse hypothesis" that an increase in trade openness leads to more healthy stock, especially in developing countries.

Potential Econometric Issue: Endogeneity

The largest area of concern for the econometric identification of the effects is that the level of trade openness of country may be endogenously determined by the health of it's fish stock.[19] If a previously exported fish species is over fished, a significantly lower health of the stock may lead to a lower measure of openness, as exports decrease. Also, the GDP of each nation depends, although varying in degree, on the production and export of fish products. As a result, a decline in the health of a fish stock might, holding all else constant, also reduce a country's income.

[19] This is a common argument in the literature on trade's environmental effects addressed by Frankel and Rose (2005) and Erhardt et al. (2014) in their empirical studies.

IV. The Data

Measuring Stock Scores in Exclusive Economic Zones

The OHI evaluates the condition of marine ecosystems according to 10 human goals,[20] which represent the key ecological, social, and economic benefits that a healthy ocean provides. A goal score is highest when the maximum sustainable benefit is achieved through methods that do not compromise the ocean's ability to deliver that benefit in the future. The overall OHI score is the average of the 10 goal scores that are calculated using different dimensions. The current status of each goal accounts for one of these dimensions and is measured in relation to a specific target or reference point.

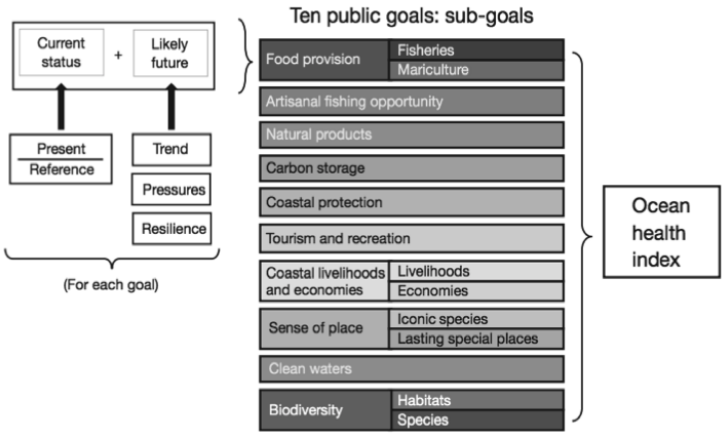

Figure 3: Conceptual Framework for Calculation the Index. Each dimension (status, trend, pressures and resilience) is derived from a wide range of data. Dimensions combine to indicate the current status and likely future condition for each of the ten goals.

To measure the level of a nation's stock health ($score_{it}$), I use the Ocean Health Index "food provision" current status scores; this score attempts to capture the level of sustainably

[20]See Figure 3 for a breakdown of the ten goals, and six sub-goals.

caught or raised fish in 108 countries and territories covering 221 different EEZs as a measure of the current health of a nation's fish stocks. The Food Provision goal score is the yield weighted average of its two sub-goals: Wild-caught commercial seafood and Mariculture, or ocean-farmed seafood. For my study I use the subset of data labeled the "Current Status" dimension of the 2014 Food Provision score. These values are calculated from the regions current value compared to a reference point, which is defined as the best condition for a goal that can reasonably be achieved; it is a relative measure of the current health of a nation's stocks as compared to what it "should" be. This is a far more inclusive measure of the stock health of fish in a country's EEZs than used in other empirical studies, because it not only considers wild-caught fish, but also determines a way to score and use the health of ocean farmed fish, or mariculture, of a country.

Measuring Mariculture Yield

This sub-goal measures the commercial harvest of seafood that is farm-raised along the coast and in the ocean. Since revised in 2013, the OHI Mariculture sub-goal uses the reference point of harvested tons per inhabitant within the 50 km coastal strip. Values for all countries are compared to the best-performing country. The reference point assumes that production is driven by socially related factors and does not assume that all coastal areas have equal potential for mariculture production. The new reference point assumes that two regions with an equal number of coastal inhabitants harvesting an equal tonnage of cultured seafood should score the same. This is a revision from the 2012 reference point assumed that all coastal ecosystems and countries have similar potential for productivity per unit of area and that all could be developed for mariculture at the same production density as in the most productive country. This unduly penalized countries with long coastlines but low population density.

Reported mariculture production comes from the FAO's Global Aquaculture Production Quantity dataset. Only pro-

duction classified in marine and brackish water environments was included in the analysis; all freshwater production was excluded. Total species produced within a country were summed to give a single production value per country for each year that production took place.

Measuring Fisheries Yield

The wild-caught commercial seafood sub-goal measures the ability to obtain maximal wild harvests without damaging the ocean's ability to continue providing fish for people in the future. For 2014 this sub-goal estimates the total population biomass relative to the biomass that can deliver maximum sustainable yield for each landed stock; I use the current stock status value, which is the average of those values, weighted by their proportional contributions to the total catch.[21] Additionally, as of 2013 new methods were used to allocate stocks into EEZs, which corrects the overestimation in the reporting quality for isolated locations such as remote islands, or countries with very extensive EEZs, as these were less likely to have stocks that overlap with neighboring countries. This update corrects for the potential problem with spatial autocorrelation of the error term that the study of Erhardt et al. (2014) identifies as a major econometric issue.

The Relative Stock Biomass is a new data layer, introduced in 2013, that gives the mean relative proportion of biomass represented by each stock within a given region. It is the proportion of a fish stock's biomass compared to the total biomass of all fish stocks present in an area. It is used in calculating the scores for the Fisheries sub-goal, as follows. Many fish stocks straddle the boundaries of EEZs, but scoring the Fisheries sub-goal for each country requires separate evaluation of stock status for the portion of each stock in each EEZ. Stock status (B/B_{MSY}) is estimated as the relation between biomass present (B) and the amount of biomass that would produce

[21] The 2013 analysis used five additional years of FAO catch data, 2007-2011. 2006 was the most recent year used for the 2012 analysis.

maximal sustainable yield (B_{MSY}). These updates make the data I use more recent and more spatially correct than that used in the study performed by Erhardt et al. (2014).

Measuring Openness to Trade

I use data from the WBO to measure a nation's degree of participation in international trade as measured by its total trade as percentage of GDP as a proxy for a country's level of trade openness, $open_{it} = (\frac{Exports+Imports}{GDP})_{it}$. This is the common practice of studies examining the relationship between trade openness and environmental quality.[22] Although a study conducted by Squalli and Wilson (2011) argues that this measurement may be a better indicator of a nation's size rather than trade openness, the model I use controls for country-specific income effects so a correction is not necessary.

Measuring Government Strength

Recognizing the lack of a precise measure for the stringency of a nation's fishery regulations gain, I will follow similar studies' use of government rating indices to do so. I use the measure of relative government quality provided by the OHI for each country. To achieve this score, Halpern, et al. (2012) used data from the Worldwide Governance Indicators (WGI), a composite of hundreds of different measures that assesses in very broad but comprehensive terms the social structure and functioning of countries as well as specific territories, scoring them along six component composite indicators: control of corruption, government effectiveness, political stability, regulatory quality, rule of law, voice and accountability. These indicators were then averaged for all six components of the WGI and then rescaled them 0–100.[23]

[22]This is a commonly used practice in the economics of traded environmental goods and utilized by researchers conducting similar studies: e.g. Antweiler et al. (2001), Frankel and Rose (2005) and Erhardt et al. (2014).

[23]Frankel and Rose (2005) and Erhardt et al. (2014) use the *International Country Risk Guide* (ICRG) to measure similar indicators, however,

Controls

Finally, in order to control for potential country specific demographic, economic and political indicators affecting stock health, I include similar explanatory variables as Erhardt et al. (2014) and Frankel and Rose (2005). However, rather than only using the population density of the entire country to capture pressures that high populations has on fish stocks, I consider effects that may be specific to coastal populations. My motivation in doing so is to take into account influences on fish stock health that are unique to populations that go beyond the scope of just a higher demand for human food consumption.[24] My income control variables are motivated by the EKC, the work of Grossman and Krueger (1993), which provides empirical evidence that national growth has negative environmental effects at low levels of GDP, as is the case in developing countries, and beneficial at high levels; plausibly, leading to countries with relatively higher PPP GDP to adopt more efficient or sustainable techniques for fishing. Another reason could be that countries with relatively higher GDP per capita have taste preferences for foreign caught fish, thus have healthier stocks in their own EEZs. In fact over 90% of the fish consumed in the United States today are imports.[25]

Accordingly, I use the natural log of each country's PPP GDP and its square. I use the 2014 data from the OHI's database, gathered from the WBO to obtain the PPP GDP as well as the coastal population density of each of the EEZ countries. PPP GDP is again defined as the gross domestic product converted to international dollars using purchasing

this database was last updated in 2008, while the WGI annually updates their online database. Additionally, Erhardt et al. only includes three indicators, while my study incorporates a more extensive, and what I believe to be a more accurate proxy for the level of regulation in a country's fishing industry.

[24]Refer to Methodology and Data Section 3.1 for a more detailed description of some of these potential influences.

[25]Greenberg (2014) discusses the increase in taste preferences for foreign caught fish that has occurred over the last few decades in the United States.

power parity rates. An international dollar has the same PPP GDP as the U.S. dollar has in the United States. I use data from the Center for International Earth Science Information Network (CIESIN) to obtain the coastal population density, defined as the percentage of the national population living within 10 miles of the coast.

The motivation for the dummy variable eu_{it}, is that considering there is a high level of trade within the Eurozone and that these countries all have identical and relatively stringent fishing regulations, the inclusion of a dummy variable allows for the possibility that healthy fish stocks is not caused by trade openness, but the two merely coincide in the Eurozone.

Differences in Models

The specifications in columns (1-3) in Table 2 incorporate different covariates to try to test the existence of a general effect of openness to international markets on national stock health. Regressions (4) and (5) consider potential explanatory effects between the interaction between trade openness and regulation in each country. The motivation in doing so is to test the predictions of the competing theoretical hypotheses of Chichilnisky (1994) and Brander and Taylor (1997) and to see if trade liberalization leads to a relocation of resource overuse from high to low-income countries.[26]

[26] This would be in line with most empirical studies as well as the theoretical prediction of Chichilnisky (1994).

Descriptive Statistics

Table 1 | Summary Statistics

Statistic	N	Mean	St. Dev.	Min.	Max.
$score_{it}$	108	53.39	21.21	1	96
$open_{it}$	108	89.21	44.78	25.71	358.0
gdp_cap_{it}	108	18,616	16,777	809.2	78,763
$log(gdp\_cap_{it})$	108	9.347	1.097	6.696	11.27
$log(gdp\_cap_{it})^2$	108	88.57	20.02	44.84	127.1
$log(pop\_dens_{it})$	108	4.303	1.314	1.029	8.951
$log(coast\_dens_{it})$	108	-2.164	0.486	-4.155	-0.635
gov_{it}	108	0.516	0.177	0.175	0.873
$open*gov_{it}$	108	47.48	35.52	4.511	292.0
eu_{it}	108	0.167	0.374	0	1
$dummy\_gov_{it}$	108	0.343	0.477	0	1
$dummy\_gov_{it}*open_{it}$	108	32.77	56.93	0	358.0

Figure 4: Note: The dependent variable is $score_{it}$, which measures the fish stock health in a given country's EEZs evaluated for 2014. The independent variables are, $open_{it}$, and its interaction term, $dummy\_gov_{it} * open_{it}$. Time-varying and country fixed effects are included in the remaining controls.

V. Results and Discussion

Fixed Effects Estimation for Model (1)

The specifications in columns (1-3) attempt to analyze if there exists a general effect of a country's openness to international trade on fish stock health. The result in each of my specifications rejects the null hypothesis that trade openness has no effect on stock health at the 5% level, demonstrating that trade openness has a significantly positive effect on national stock health, implying that openness reduces collapse and overuse

of fish stocks. My first specification (1) tests the general correlation between stock health score and trade openness, finding the relationship to be significantly positive at the 5% level.

The second specification in column (2) tests to see if there is any sign of an Environmental Kuznets Curve (EKC), which would show a negative coefficient on the measure of income and that on the parameter of the squared measure to be positive. Although I do not see significant signs of an EKC curve, this regression demonstrates that trade openness has a significantly positive effect on stock health, again at the 5% level.

Table 2 | Empirical Model (1) Fixed Effects Estimation, Dependent Variable: 2014 Stock Status Score

	(1) score	(2) score	(3) score
$open_{it}$	0.0749**	0.0858**	0.117**
	(0.0345)	(0.0400)	(0.0461)
$log(gdp\_cap_{it})$		-2.639	15.97
		(30.89)	(33.58)
$log(gdp\_cap_{it})^2$		0.0250	-1.192
		(1.685)	(1.886)
$log(pop\_dens_{it})$			-4.766**
			(1.918)
$log(coast\_dens_{it})$			1.511
			(5.554)
eu_{it}			8.006**
			(4.028)
gov_{it}			18.86
			(17.45)
Observations	108	108	108
R-squared	0.025	0.037	0.133

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10

Figure 5

The specification in column (3) tests for the effect of openness after controlling for all country-specific income, population and governmental effects. My results find a positively significant effect of trade openness on stock health that is greater than the effect in specifications (1) and (2). These results

imply that once country-specific characteristics, which may have effects on fish stock health unrelated to the level of trade openness, are controlled for, the effect of trade openness is significantly more positive than before. The high significance of downward pressures from population helps explain why this effect is effect is more beneficial than in specification (1), which tries to explain the broad measure of overuse by trade shares.

The log of the coastal population density is not statistically significant in any of the regressions, however the more conventional population control, the natural log of the total national population density, is negatively significant in each specification. This makes sense, as fish products are in higher demand when fresh, thus part of this may be explained by the nature of the fish industry and that countries with larger populations consume more domestically caught fish because individuals demand a short time frame from catch to plate. Part of the reason as to why the coastal population control is not statistically significant could be from the way in which the mariculture score is calculated. Since it uses a reference point that takes into account factors including coastal population pressures, some of these effects could already be controlled for in the dependent variable itself. Another reason is presented in Foster and Rosenzweig's (2003) study that provides an explanation for unexpected results achieved in the relationship between population level and resource health. They noticed that in closed developing countries, population growth stimulates demand for the resource, increases its price, which can result in the introduction of a resource management policy, allowing for the recuperation of the resource. If this argument, which is supported by Copeland and Taylor's (2009) theoretical argument, is applied to the health of fish stocks, it could help explain the variation in the data.

Fixed Effects Estimation for Model (2)

The fourth (4) and fifth (5) specifications test for an indication of the competing predictions of Brander Taylor (1997) and Chichilnisky (1994), by considering potential interaction

effects of openness and governance. These regressions report positive significant effects of the interaction of openness and regulation level at the 5%-level for countries with low governmental strength.

In these regressions the coefficient on open represents the effects of trade openness in countries with low level of governance ($dummy\_gov_{it} = 0$). When I take into account the possibility that the level of regulation or degree of "open access"[27] to the resource has interaction effects with the degree of trade openness, openness has a positive effect on fish stock health which is quantitatively greater than the effect when considering all countries in the sample (β_1 in regressions (4) and (5) is greater than β_1 in regressions (1)- (3)). Although these effects are not statistically *different*, the fact that there is no sign of a negative effect of openness in countries with lower levels of governmental regulation, indicates that even at the lowest possible value of governance ($dummy\_gov_{it} = 0$), trade liberalization has a significantly positive effect, refuting the prediction of Chichilnisky's (1994) theory and supporting that of Brander and Taylor (1997).

[27] I deem a country to have relatively open access to the resource when their quality of governance (gov_{it}) is less than the global median, such that $dummy\_gov_{it} = 0$ and thus, the interpretation of the coefficient on their measure of openness to international trade of trade openness is simply the effects of trade openness the open regression.

Table 3 | Fixed Effects Estimation, Dependent Variable: 2014 Stock Status Score

	(4) score	(5) score
$open_{it}$	0.125**	0.129**
	(0.0623)	(0.0636)
$log(gdp\_cap_{it})$		11.70
		(34.22)
$log(gdp\_cap_{it})^2$		-0.945
		(1.921)
$log(pop\_dens_{it})$		-4.533**
		(1.988)
$log(coast\_dens_{it})$		1.591
		(5.562)
eu_{it}		8.062**
		(4.059)
gov_{it}	18.28	23.21
	(16.04)	(20.44)
$dummy\_gov_{it}*open_{it}$	-0.0917	-0.0287
	(10.66)	(152.5)
Observations	108	108
R-squared	0.045	0.134

Figure 6

The interpretation of the coefficients in specifications (4) and (5) is different for countries with a higher level of governmental regulation ($dummy\_gov_{it} = 1$). For these countries, the effect of trade openness can be interpreted as the sum of the coefficients on the interaction term and on trade openness. I conducted an F-test to measure the statistical significance of this effect in countries with higher levels of regulation effect. Table 4 displays the F-statistics for specifications (4) and (5), testing the null hypothesis that $dummy\_gov_{it} * openit + openit = 0$, and demonstrates that, after controlling for different country-specific income, population and governmental effects unrelated to trade openness, the effect of trade liberalization in countries with higher levels of regulation is significantly positive[28].

[28]The null hypothesis, that $dummy\_gov_{it} * openit + openit = 0$, is rejected at the 5% level in specification (5).

Table 4 | F-Tests for Empirical Model (2)

	(4) dummy_gov$_{it}$*open$_{it}$+open$_{it}$=0	(5) dummy_gov$_{it}$*open$_{it}$+open$_{it}$=0
Marginal Effect (β_{8+} β_1)	0.0333	0.1003
F-Statistic	0.92	4.03**
P-value	(0.3384)	(0.0474)
Deg_Fr	1	1
Deg	104	99

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

Figure 7

However, as I mentioned earlier, it is important not to confuse this statistical significance as indication that the marginal effects in these areas are statistically different from those in countries with lower levels of regulation. Although is worth noting that the effect on the interaction term is negative, thus the sum of the coefficients, and the effect of openness in countries with a higher level of regulation is less than that in countries with a lower level (β_1 when $dummy\_gov_{it} = 0$), this difference would only be statistically significant if the coefficient on the interaction term were statistically significant. Therefore, although the effect of openness in countries with relatively higher regulation is statistically significant, (the null hypothesis is rejected with 95% confidence), the difference in these effects is not. Thus, even though it may seem like the effect is more positive in countries with lower levels of regulation, than in countries with higher regulation, since the coefficient on the interaction term is not significant, the marginal effects in these countries are not statistically different.

However, since the marginal effects in countries with a relatively lower level of governmental stringency is not statistically different from those in countries with more regulation, my data refute Chichilnisky's (1994) prediction, that the effects of trade liberalization should lead to a relocation of overfishing

to developing countries, and the implications of most other empirical studies, which argue that increased trade openness should lead to a decrease in stock health. My findings support the "severe overuse hypothesis" of Brander and Taylor (1997), as I find positively significant effects of trade openness for all levels of governance in each specification, suggesting that increased openness to trade has a beneficial impact on the health of fish stocks.

VI. Conclusion

Even though the period from the mid-1980s until 2014 has witnessed both an increase in international trade openness and a decline in the health of fish stocks, my study rejects the popular belief that increased trade liberalization contributes to overfishing. In fact, my results suggest that increased trade openness is a limiting factor on overfishing and may have positive effects on the health of fish stocks. These results suggest that in closed countries, regardless of their level of governmental regulation, the relative price of overused resources becomes high enough to develop a comparative cost disadvantage in the resource. This disadvantage could be due to a number of factors like rising costs associated with fishing a declining supply, or a higher willingness to pay due to a shrinking supply. Whatever the source is, my study suggests that introducing liberalized trade allows the country with an overfished stock to import the resource for a cheaper price, taking pressure off the resource. I provide empirical evidence that holds implications contradicting the theoretical hypothesis of Chichilnisky (1994) and other empirical studies like that of Ferreira (2004); generally supporting the "severe overuse hypothesis" of Brander and Taylor (1997).

In the context of broader studies on the relationship between trade and the quality of the environment, my results indicate that trade openness is more good than bad for the environment. However, this is not to say that openness always has good effects on the environment and it is important to be cautious about the promotion of free trade as a conser-

vationist policy. There are some cases where an international market has led to horrendous outcomes on environmental resource stocks, like the effects that increased trade of fins has had on sharks, discussed by Clarke et al. (2007), and the effects that a larger demand for buffalo has had on the American bison population, discussed by Taylor (2011). Thus, although the empirical results of my study conclude that the effects of trade openness are quantitatively beneficial to the health of fish stocks, especially after allowing for the interaction of trade openness and level of governance, this does not mean that opposite, and potentially larger, effects do not exist. As my data does not find a statistical difference between the effects of trade liberalization in countries of various regulatory levels, there is a need to confirm my results using additional datasets on internationally traded renewable resources in both developing and more developed areas.

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Notes

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