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**Working Paper**

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CHOPE Working Paper, No. 2021-02

**Provided in Cooperation with:**

Center for the History of Political Economy at Duke University

*Suggested Citation:* Brackmann Netto, Arthur (2021) : Raised within government walls: The early years of program evaluation in the US, CHOPE Working Paper, No. 2021-02, Duke University, Center for the History of Political Economy (CHOPE), Durham, NC, <https://doi.org/10.2139/ssrn.3790608>

This Version is available at:

<https://hdl.handle.net/10419/231444>

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RAISED WITHIN GOVERNMENT WALLS:  
THE EARLY YEARS OF PROGRAM  
EVALUATION IN THE US

ARTHUR BRACKMANN NETTO

CHOPE WORKING PAPER No. 2021-02  
FEBRUARY 2021



CENTER FOR THE  
HISTORY OF POLITICAL ECONOMY  
AT DUKE UNIVERSITY

# Trapped within Government Walls: The Early Years of Program Evaluation in the US Government

Arthur Brackmann Netto<sup>1</sup>

## Abstract

Not much equates Michael Harrington, Dwight MacDonal, Orley Ashenfelter and James Heckman. But still history made a way to have, in one end of a story, a narrative journalist and a political scientist and, in the other, two economists. Harrington's famous piece, *The Other America*, made its way into government through MacDonal's extensive review in what could have been just another edition of the *New Yorker* in that January of 1963. MacDonal's lines ended up in John Kennedy's hands – or so the tale tells – and poverty made its way inside governmental concerns. "Poverty knowledge", then, spin and turned for 15 years for in one of its ramifications to conceive the field of microeconometrics. Poverty went from a campaign theme to an unconditional war, from unmeasurable to fashionable. It travelled through the Council of Economic Advisers and the Office of Economic Opportunity, to the Department of Labor and teaching rooms of universities. It finally reached the Office of the Assistant Secretary for Policy, Evaluation and Research (ASPER), where it became econometric policy evaluation and microeconometrics, embodied in the papers of Ashenfelter and Heckman in the late 70s. This paper intends to shed some light in this journey towards microeconometrics.

## 1) Introduction: Rediscovering Poverty in a Social-Scientific Government

On Saturday, the 13 of January 1963, someone wanting to have a pleasant morning could reach the closest newsstand and find an excellent selection of magazines. The *Time* stamped congressman Wilbur Mills and featured its cover story: "Will your taxes be cut?" *Vogue* latest edition brought one of its models dressed all in white, holding a white umbrella. Fresh in the newsstand was the recently arrived volume of the *New Yorker*, edition from that same day. Behind Birnbaum's cover depicting Ice Fishermen, the reader would be surprised. The largest-ever review of a book in the magazine was there to be read: MacDonal's "Our Invisible Poor" (MacDonal 1963). Reading the *New Yorker* was undoubtedly an honest way to spend that morning. Like this, Michael Harrington's (1962) *The Other America* pleased the morning of many Americans on that Saturday. Like this, numerous others noticed poverty for the first time in the following weeks.

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The tale from that time tells that among those Americans, President Kennedy was also enticed by MacDonald's lines and Harrington's book - as well as by Galbraith's (1958) book *The Affluent Society* and Homer Bigart's articles at the *New York Times*<sup>2</sup>. From his interest onwards, histories about the unveil of poverty abound<sup>3</sup>. The narratives are straightforward. The desire of overcoming poverty was listed on governments' inventory and would start receiving all the attention it needed to become a reality. Kennedy's assassination did not hamper the process; on the contrary. Lyndon Johnson, Kennedy's vice-president who rose to Presidency, would become the name behind America's unconditional War on Poverty.

Nearly one year after MacDonald's *Our Invisible Poor* and a month after Kennedy's death, in the same newsstand, Lyndon Johnson was stamped in *Life's* issue of the 31 of January 1964 sitting on the front porch of the Fletcher's – a typical low-income family of the Appalachia destined to be the symbol of American poverty. 'The Valley of Poverty' – heading of the main story of the magazine - reported: "In a lonely valley in eastern Kentucky, in the heart of the mountainous region called Appalachia, live an impoverished people whose plight has long been ignored by affluent America" (LIFE 1964).

Kennedy, Johnson, *The New Yorker*, and *Life* are symbols of what has been described as a more extensive transformation in America: a rediscovery of Poverty. After almost twenty post-war years worried about developing the third world, the 60s United States was finally looking to its own problems and (re)discovering that they were worse than expected. Fights against the Vietnam War and for Equal Rights inflamed the emergence of movements on the streets of the country during that decade and the next.

The government reacted fast. In July of 1964, Johnson signed the Civil Rights Act. A month later, on the 20 of August, the Economic Opportunity Act was also effective. Although the signatures were a crystal-clear change for equal rights, its effects on the social sciences were far from foreseeable<sup>4</sup>. Since the War, social sciences secured a vital role in American Government. The Council of Economic Advisers had been created, discussions for

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<sup>2</sup> See Shriver Oral History for a personal account and O'Connor (2001) for a secondary review.

<sup>3</sup> see O'Connor 2001 for an extensive review and O'Connor 2020 for a recent article on the subject.

<sup>4</sup> About the development of Social Sciences in the post-war, see: Backhouse and Fontaine (2010)



a Council of Social Advisers were not unusual and economists, sociologists, political scientists were gradually occupying more posts on governmental organizations<sup>5</sup>.

Still, having a place is not the same as having a role. Up to the 60s, the Council of Economic Advisers was mainly concerned with macroeconomic problems. They dealt with the few measurable social indicators of the time: product, productivity, employment, and so forth. In contrast, poverty, crime, and health were then intangible social problems. This was about to change with the arrival of the War on Poverty, or more precisely, with its development.

From 1960 to 1970, “poverty knowledge”<sup>6</sup> in the US government went from unknown and qualitative to fashionable and measurable. All that to, in the Nixon years, lose its fashion and put in question its measurability. It went from a social science problem to an economics problem and then to a multidisciplinary endeavor. Poverty knowledge woven for fifteen years inside government walls until disciplinary borders became clearer and the disciplines were set free. The following story demonstrates that, among these disciplines, economics’ program evaluation was born promoting microeconometrics to a new status<sup>7</sup>.

## 2) 1962-1964: Growth and Poverty into the Johnson Administration

“Mostly unmeasurable” seems a fair adjective for social problems in the 50s and 60s. It would be unfair with “Robert Lampman, Mollie Orshansky, Ida Craven Merriam, Herman Miller, Dorothy Brady, Helen Lamale, Selma Fine Goldsmith, Alvin Schorr, and many lesser-known bureaucrats and academics” of the 50s to say that poverty was unmeasurable (Huret 2018 p. 2). Someone had to supply Harrington with the data that backed his arguments. This lesser-known bureaucrats and academics that formed the “invisible network” of poverty did it (Huret 2018, 2010). And so, his book is filled with numbers: “There are 8,000,000 or more people living in the most miserable of conditions”;

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<sup>5</sup> Proposed in Bill #S.5 of 1969. See: Sheldon and Freeman (1970)

<sup>6</sup> Alice O’Connor’s (2001) introduced the term: “poverty knowledge”.

<sup>7</sup> Section 2 to 4 rely heavily on Alice O’Connor’s “Poverty Knowledge” (2001) and Alain Huret “The Expert’s War on Poverty” (2018).

"In New York City there are some 300,000 hard core Public Assistance cases". (Harrington 1962 p. 118, 142).

Still, these unknown figures were ill-equipped when compared to modern data and statistics. What amused Harrington's readers was (and still is) the palpable side of poverty. Trying to choose a single sentence from Harrington that highlights his way of describing poverty is hard; the book has uncountable examples. Adjectives follow numbers instilling feelings, just like his sentences above do with "most miserable" and "hard core". Poverty is not a problem of numbers in the book, it is a problem of human beings. Harrington's use of statistics is "impressionistic" as MacDonald noticed in 1964.

Anyhow, the topic received broad attention. For most Americans, it felt like suddenly poverty was everywhere: books, radio, magazines, television. The Vietnam War and racial inequality helped to show different sides of the problem. Imagine yourself a fan of boxing in the 60s, excited with Muhammad Ali, experiencing amazing fights, but also fierce criticisms to the War and racial problems. It was hard to not be aware of the issues of the day.

These were the early 1960s. A liberal, John Kennedy, had risen to the White House in 1962. With him, a new Council of Economic Advisers was formed to walk through the third floor of the Executive Office Building in Washington. Walter Heller, "the present-minded professor who tempers earnestness with cordiality and intellect with a touch of ambitious worldliness", became the chair. Kermit Gordon and James Tobin, "the twin rocks of Gibraltar", completed the "pragmatist" trio. Their task was direct, although complex: they had to make the economy grow (TIME Mar 03, 1961).

During his campaign, Kennedy repeatedly claimed that he would "get America moving again"<sup>8</sup>, and the economists did their best to achieve high growth rates for it. The previous Eisenhower's committee of Price Stability for Economic Growth became the Committee for Economic Growth. As advocates of the "new economics", the trio's main strategy would be to promote tax cuts as a Keynesian stimulus for the desired growth rate<sup>9</sup>.

From Kennedy's campaign to its initial governing years, new economics' policies were the Council's main commitment. Beyond the main trio, Robert Solow, Arthur Okun, and

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<sup>8</sup> This was one of Kennedy's campaign slogans

<sup>9</sup> See Tobin (1974) for a review of new economics. See Boianovsky and Hoover (2014) for a history of the development of growth as an academic subject at MIT during the 50s and 60s.

Kenneth Arrow joined the Council as advisers. During that period, Solow had just written his model on economic growth (Solow 1956), and Okun would soon discover the Okun's Law (Okun 1962). Those were economists with high credentials for a growth-centered liberal government - even though they advised only during Kennedy's debut year.

Growth, however, came with a dilemma for those economists: trickle-down effects were uncertain. Would growth hit the poorest? Harrington, Galbraith, and the media claimed that it would not. The poor needed specific policies in their views. The Council disagreed. Of course, growth was the best way to help the poor. In 1962, when the council looked for answers to the challenge, Walter Heller invited Robert Lampman to join the Council as an adviser on poverty issues. Lampman had been Heller's colleague in Wisconsin. He was specialized in poverty and also - probably not by chance - a believer in the power of growth for the poor (see Huret 2018 p. xxx). In 1963, when the pressure for governmental responses to the problem of poverty were on their peak, Heller relied on Lampman to start CEA's official analysis of poverty that culminated in the 1964 Economic Report of the President.

Lampman must have had a hard time in writing the report as science and politics are not always easily combined. In 63, poverty was no more than a "powerful campaign theme" that had to be integrated in his scientific predilection for growth (O'connor 2001, p. 152). At first sight, for him as for 1960s economists, poverty was not an economics problem.

Poverty as knowledge in the early 60s was unsubstantial (see Huret 2018, 2010). Until 1962, the word "poverty" appeared in less than 5% of sociology or economics papers indexed in JSTOR. The term did not appear in 97.5% of all paper indexed in the platform. It was like it did not matter. Poverty as a category was fragile, instead researchers focused on *individuals* or their *communities*. Without poverty being a reliable concept, psychological traits of poor individuals and their communities' characteristics dominated the discussions.

Although a small community, the nuances of dealing with individuals and communities created two different shades of poverty knowledge. The first was the "culture of poverty", which claimed that the poor had a culture of their own that trapped them in the lower classes. Poverty was a trait of individuals for those researchers. As a second research group - that is in many ways connected to the culture of poverty -, there was a community

researching delinquency and other everyday events in poor communities in the US. Their focus was not poverty per se, but the issues in poor communities like crime, delinquency, and lack of study. Poverty related issues were a trait of communities for this second group.

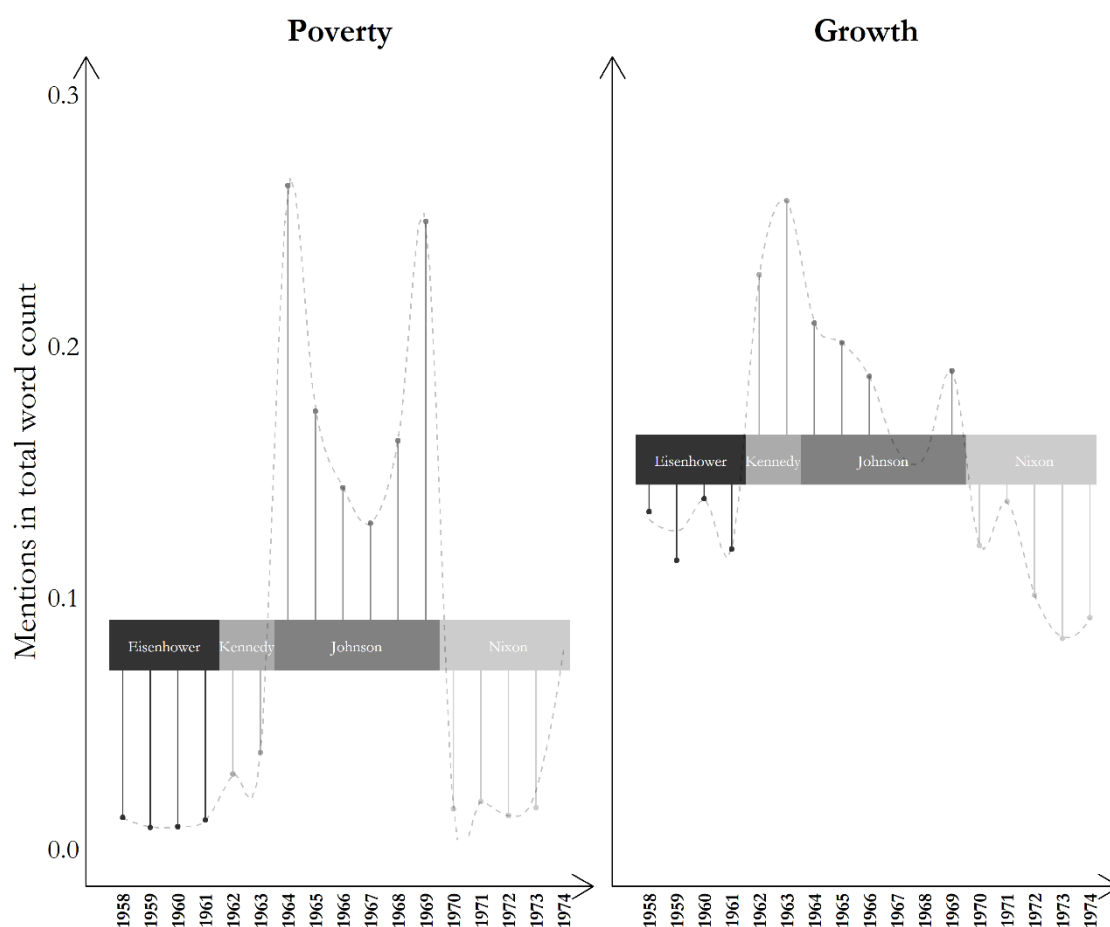
The most popular exemplars of the culture of poverty are Oscar Lewis' works (Lewis 1959, 1966, Streib et al. 2016). In 1959 the author published his book on poverty "Five Families - Mexican Case Studies in the Culture of Poverty" after a thorough examination of poverty families, using the most up to date surveys from behavioral psychology. "The culture of poverty" would soon become the theoretical frame that would define his career. Seven years after the book, he published an article in Scientific American delineating the main arguments in his theory. In his concise abstract, Lewis simply stated: "Does membership in a group that has been poor for generations constitute belonging to a separate culture? A study of Puerto Ricans in both Puerto Rico and New York indicates that it does" (Lewis 1966, p. 19).

On the other side, Cloward and Ohlin (1960) "Delinquency and Opportunity" is a study of gangs' subculture recalled as an influential work on delinquency. It did not achieve much theoretical influence though, but practical. Their book supported the creation of the Ford Foundation's Gray Areas project of 1962 and the release of Executive Order 10940 that established the President's Committee on Juvenile Delinquency and Youth Crime (PCJD) in 1961.

How could Kennedy's administration reconcile economic growth and these two branches of poverty knowledge was a question that had no immediate answer. The years of 1962 and 1963, then, were a time when economists put forth their views on economic growth reluctantly. The lack of trickle-down effects and media coverage about poverty saturated the air, and they revised their policies already in 1964.

In the economic report to the president of 1962 and 1963, economic growth was the main subject. Poverty, on the other hand, was suppressed from print in a consistent manner. This overturned in 1964, when the Government could not ignore the poverty roaring anymore. In the next figure, we can observe the mentions of poverty and growth terms in the economic reports from 1958 to 1974:

Fig. 1 – Economic Reports of the President<sup>10</sup>



Source: Economic Reports from 1958 to 1974<sup>11</sup>

Growth was of limited concern in Eisenhower and Nixon's conservative governments, but it widely appeared in the reports of Kennedy and Johnson. As Tobin remembers: "An important issue for me arose in the 1960 presidential campaign. Kennedy and the Democrats were accusing Eisenhower and the Republicans of letting the economy stagnate and not grow fast enough. There was a lot of discussion about growth, and there was complete confusion in the political discussion [...]" (Tobin Oral History 1999, p. 881). In a more contemporary comment, in 1965, he simply put: "At any rate, accelerating growth seemed to be a major theme." (CEA oral history p. 35).

<sup>10</sup> Named bar indicates average mentions to term during the period.

<sup>11</sup> Available at: <https://www.presidency.ucsb.edu/documents/presidential-documents-archive-guidebook/the-economic-report-the-president-truman-1947>. For more information see Appendix A.

Since WWII, America was in a constant run for growing, especially to grow more than its direct enemy, the Soviet Union (Holden and Biddle 2017, Collins 2000). Although Eisenhower had opted to a more conservative policy, putting Arthur Burns – a “sympathetic Republican loyalist” (Burton 2006, p. 177) - ahead of CEA and campaigning for stability, growth never left America’s interest. The late 50s were just a brief "republican interlude" in the growth mentality of US government, according to Collins (2000).

The 64 surprise then, is not in mentions to growth-related terms, but in mentions to poverty-related ones. In a pioneering manner, Heller’s first signed report to President Johnson mentioned more poverty terminologies than growth words. This could be interpreted as a single man effort: Lyndon Johnson rose to power to save the poor. This is imprecise. Kennedy's administration was aware of the poverty problem and, although it did not beat Johnson’s reports in mentions, it was ahead of the conservative governments in the issue.

This apparently fast change in the framing of problems and campaign themes was a gradual change underneath. Poverty would probably also been dealt with if Lee Harvey Oswald had not shot the president on that tragic November of 1963 in Texas. Walter Heller, in this regard, remembers struggling to convince Kennedy<sup>12</sup> about the relevance of a program on poverty but eventually receiving the approval:

"I got the green light from him [Ted Sorensen] on the poverty program. I had, after all, initiated that in May and June of 1963 in the sense of trying to get Kennedy interested. I had difficulty getting him on board, so to speak. Ted Sorensen had told me, "Keep at it, it's the kind of an issue we should sign on to, and it's a terribly important thing." I'd had sessions on it with Kennedy in October and again" (Heller Oral History, Interview I, p. 20)

Even before those sessions:

"I thought the only thing to do was to go directly to Kennedy and find out how he felt about it. And he said, 'Yes, Walter, I am definitely going to have something in the line of an attack on poverty in my program. I don't

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<sup>12</sup> Cherrier (2019) demonstrates Heller’s skills to inform and convince the president of his economic ideas

know what yet. But, yes, keep your boys at work, and come back to me in a couple of weeks." (Heller Oral History, Interview I, p. 21)

Lyndon Johnson played his part in boosting poverty programs as an "Unconditional War on Poverty". Walter Heller remembers that Lyndon Johnson was spontaneously in favor of a program for the poor. According to Heller, Johnson said the following when receiving the information about the poverty program: "That's my kind of program; I'll find money for it one way or another. If I have to, I'll take away money from things to get money for people." (Heller I, p. xx)

The previous name was something way less impactful: "widening participation in prosperity" (see Sundquist Oral History, interview 1, p. 4). Kermit Gordon remembers that poverty was initially a condemned term, and Johnsons' advisers tried their best to avoid it. They believed the word could sound bad internationally or even offend low-income people:

"An interminable amount of time was spent in thinking up euphemisms for the poverty program with all this high-powered talent around the table. I think the reason it was called the War on poverty, the poverty program, was that nobody around the table, despite a lengthy effort to identify such a title, could think of any euphemism which didn't sound silly." (Gordon Oral History IV p. 3)

The War theme was contemporary, and Americans were already used to it. Lyndon Johnson presented the additional War with grandeur, knowing that it was central for his approval. The idea stuck into Americans' minds. He was not Kennedy's substitute; he was Lyndon Johnson: the - hopefully - winner of the War on Poverty.

Campaign themes aside, as a reminiscence of Kennedy's administration, growth was still central in the economic reports from 1964 to 1970. Walter Heller was still the chair of the economic Council, but Kermit Gordon and James Tobin had already been substituted. Heller's opinion on the role of growth was unaffected, even though he was politically smart in understanding the need for a poverty embedding for his Keynesian aspirations. That was really a matter of political acuteness from Heller and the CEA. Economists, until the War on Poverty, overlooked social problems in a regular basis (see Fleury 2010). To put the topic on a governmental report was audacious and pioneer.

During the insertion of poverty in the report, both the culture of poverty and delinquency studies made their way into Johnson's administration. The technical and numerical nature of economic discourse was complemented by its counterpart from the sociology. In the economic report of 1964, there were paragraphs that without context could readily be confused with one of Harrington's famous piece:

"The poor inhabit a world scarcely recognizable, and rarely recognized, by the majority of their fellow Americans. It is a world apart, whose inhabitants are isolated from the mainstream of American life and alienated from its values. It is a world where Americans are literally concerned with day-to-day survival—a roof over their heads, where the next meal is coming from. It is a world where a minor illness is a major tragedy, where pride and privacy must be sacrificed to get help, where honesty can become a luxury and ambition a myth. Worst of all, the poverty of the fathers is visited upon the children" (US Economic Report 1964 p. 55)

"Poverty, as has been shown, has many faces. It is found in the North and in the South; in the East and in the West; on the farm and in the city. It is found among the young and among the old, among the employed and the unemployed. Its roots are many and its causes complex" (US Economic Report 1964 p. 77)

This is what O'Connor (2001) has named a Harrington-like language of the report. However, what is certain is that rebranding was easier on paper than in practice. Several agencies wanted to be the pillar of the War. The Department of Labor (DOL) and the Health Education and Welfare (HEW) department were the main contenders. But were they ready for mixing economics and action-oriented sociology? More important, were they prepared to put that mix in action right away? The answer at the time was: no. Johnson believed that current government agencies were unqualified to innovation once they would be divided between the new poverty effort and their normal activities.

The "Office of Economic Opportunity" was rapidly gestated to the responsibility of administering the "War on Poverty" then. In the lack of an adequate place, the new agency was born in a "rather dingy office" in Washington<sup>13</sup> with the charge of "understanding the enemy" and creating an "strategy of attack" - although nobody actually knew what that meant at that time. In haste, the Economic Opportunity Act of 1964 that established the Office

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<sup>13</sup> Probably Robert Shriver's Conference Room. See Yarmolinski Interview II p. 5-6.



was vague in numerous ways; where should it be its physical location was one of them, the definition of poverty and how to face it were another two.

Section 202 of the economic opportunity act, where the definition of community action programs can be found, was written in a very unclear manner. Point a.3 of section 202, reads as follows: "[The term "community action program" means a program] which is developed, conducted, and administered with the maximum feasible participation of residents of the areas and members of the groups served". "Maximum feasible participation" was, without hyperbole, an inadequate choice of vocabulary and it has been discussed for its vagueness (US Economic Opportunity act 1964, p. 516). It even became a pun in Patrick Moynihan's book title years after: "Maximum feasible misunderstanding" (Moynihan 1969). In any case, the OEO was born, and it had to in a short time be capable of encouraging "maximum feasible participation" of the poor as part of its "strategy of attack".

All the haste and vagueness became a recipe for conflict, but also the recipe that would displace economists from their macroeconomic "ivory towers"<sup>14</sup>. Poverty was on its way out of the Council of Economic Advisers. Poverty would soon become more than a campaign theme for economists.

### 3) 1964-1968: OEO born for conflict

The Office of Economic Opportunity is the symbol of an era. It was an assortment of war-like ideas. Many of the well-succeeded concepts of the cold War and WWII social sciences were there. The need for a different agency created an environment where every social scientist wanted space for its own worldview. As Haveman (1977) remembers: "Nearly every hypothesis regarding why the poor performed weakly in the labor market was reflected in some program" (Haveman 1977, p.6).

The agency, although having born in a dingy office, had a central role in the future war. The OEO was an action, coordination, researching, and evaluative facility. It was an immense innovative effort (March 1966, p. 115). Nothing like it had been tried before at the

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<sup>14</sup> It has already become standard to recall that when John Kennedy contacted James Tobin to join his team, Tobin answered: "I think you picked the wrong guy, I'm an Ivory Tower Economist".

US government. Johnson gave it cabinet status that offered them freedom for innovation and quicker movements in Congress. In a way, the Office should work as an omniscient entity able to end poverty in about ten years! Sociologists run OEO's action, economists commanded evaluation and research, and politicians coordinated the effort.

For Johnson, the central piece for the idea to work out was a single man: Robert Sargent Shriver. Economists and sociologists could have the ideas, but someone had to pass them through Congress. Shriver was a middle-aged man who knew his way in politics. He was married to Eunice Kennedy, and thus was John Kennedy's brother-in-law. Since 1961, "Sarge" was ahead of the peace corps, making world tours solving political problems and implementing "community development" programs. He had both the credentials and the personal connections for the job.

Johnson trusted the man, as did John Kennedy before him. On Thursday, the 2 of January of 1964, Johnson called Shriver to inform that he wanted to announce him as the head of the War on Poverty (see: Shriver oral history I). In fact, he wanted to announce Shriver in that same day. Shriver was caught by surprise. He had talked to Johnson at the Oval Office a few days before, but he had ineptly understood the president's hurry. He was still mourning Kennedy's death and had recently arrived from a world tour for the Peace Corps. Shriver asked for time. Johnson gave him what an urged president can give: few hours. In the fourth call that day, in late afternoon, Johnson did not want to hear any other excuse from Shriver and just said in more eloquent words: "That's it. It's going to be you. I'll announce you soon. Be ready."<sup>15</sup> That was Johnson's conviction in the capacity of Shriver.

Shriver became responsible for organizing a task force that would put the War on Poverty on paper. Everything went fast. His first pick to help him was Adam Yarmolinski, ex-RAND researcher and at that moment serving Robert McNamara at the Department of Defense (see Yarmolinski Oral history). They assembled professionals from government agencies and from outside to complete the task force. As the president wanted the program to be his first serious move in congress, Shriver, Yarmolinsky and their crew outlined the

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<sup>15</sup> Recording of the phone calls of that day are available at: [https://discoverlbj.org/solr-search?q=LBJ%20and%20SARGENT%20SHRIVER&facet=40\\_s%3A%221964-02-01%22](https://discoverlbj.org/solr-search?q=LBJ%20and%20SARGENT%20SHRIVER&facet=40_s%3A%221964-02-01%22)

“strategy of attack”, including its central unity, the Office of Economic Opportunity, in no more than six months.

In that brief period, they had lots of questions to face, but two stood out: What exactly is poverty? How should they face it? The task force came up with pragmatic answers for those questions. For a political reason, an absolute line of income defined who the poor were. Absolute poverty can have an end, while relative poverty can go forever. It would not be politically smart to combat relative poverty<sup>16</sup>.

Now, once they knew who the poor were, they had to decide how to help them. Johnson had one demand: action. Johnson wanted the War on poverty and the OEO to be action-oriented. It had to be an agency aiming at acting directly on the grassroots of poverty. On this, the task force came up with a - soon to be noticed - reckless solution: the pioneer activities of Ford Foundation Gray Areas Project.

In the late 50s, Ford Foundation Gray Areas Program was one of the earliest efforts to cope with the increasing awareness of problems surrounding juvenile delinquency and the urban crisis – inspired by Cloward and Ohlin (1960). It started as a series of grants provided to five cities: Boston, Philadelphia, Oakland, New Haven and Washington D.C. Almost everything about it was experimental as never something with its level of community engagement had been tried before (see O’Connor 1996). Still, participation of the communities in understanding and solving urban problems was its main goal and was demonstrating signs of success in the mid-60s.

The members agreed that the community action programs (CAPs) as embodied by Ford’s ideal were the best way to attain the action goal. At least three reasons backed the decision. Beyond knowing community action quite well, given that the peace corps were involved in “community development” projects that worked in a similar vein, Shriver was a friend of Dick Lee, New Haven mayor, where Ford’s Gray Areas had had its greatest success. In addition, among the task force recruits, the initial achievements of Ford’s program put Paul Ylvisaker - director of the Gray Areas program - in an important position inside the group. These forces were enough for the CAP’s pioneering trait prevail over its immaturity.

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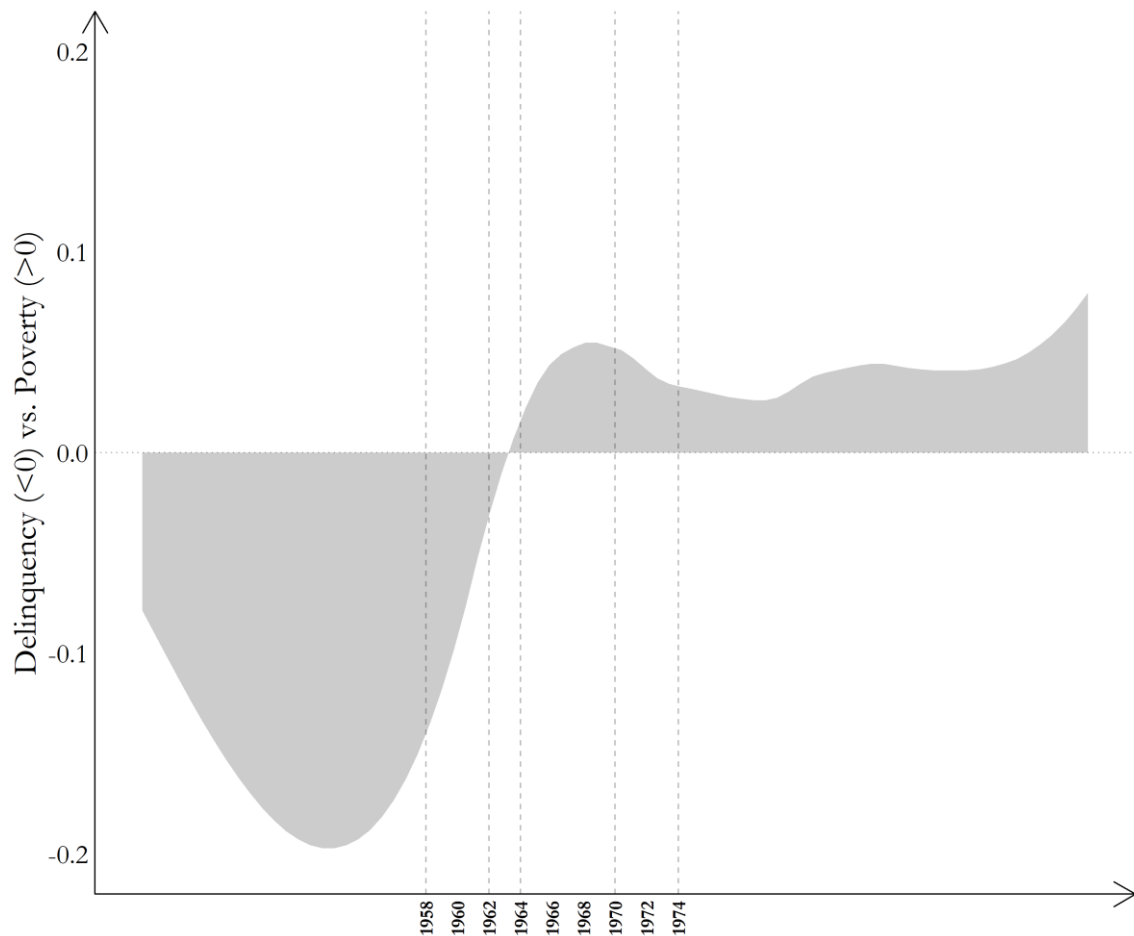
<sup>16</sup> Molly Orshansky’s research was behind the somewhat arbitrary definition of the line of income. See Huret (2018).

Conceptual clearness may have helped as well. In the paper, community action is a straightforward task of empowering a community and let it make its own decisions. The poor - or whatever community in question - should not be taken for granted, and more important, they should not be co-opted in any manner. In this sense, community action programs are small-localized efforts. Each community must develop its unique democratic system of defining problems and finding solutions. Community action researchers were hopeful about this aspect as can be seen in the first edition of the *Community Agencies Workbook*:

“The community action agency provides the means whereby a community can take a fresh look in a coordinated fashion at the problem of poverty. It enables local citizens, local organizations, and local government to plan and act together. From their joint experiences and diverse perspectives, they try to find more effective ways to reduce or eliminate poverty.” (US OEO 1965, p. 12).

What the War on poverty did was embrace this apparently simple idea and make it its own. The literature on “community action” that was of small scale and once concerned with delinquency, became in a brief period bigger and almost completely concerned with the broader problem of poverty. In JSTOR indexed articles, in the 60s, mentions to poverty started to dominate papers mentioning “community action”, while delinquency mentions lost space:

Fig. 2 – Poverty vs Delinquency mentions in “Community Action” Abstracts



Source: JSTOR<sup>17</sup>

It was all new and appealing. Although Shriver was very fond of the idea, he noticed that his budget was way over the necessary to implement hundreds – even thousands – of community agencies around the country (see Yarmolinsky I, p. 10). He had 500 million dollars to spend. His estimate was of spending 300 in CAPs. The task force had to find a way to use the leftover.

In his review of the 60s governments, Plotnick (1975) compiled the five options of poverty policy available at the time: cash transfers, in-kind transfers, direct services, human capital programs, and community development programs. We have seen that CAPs were in the last category and represented the major part of the bankroll. Another option was readily

<sup>17</sup> Data built with JSTOR data for research (DFR), searched term: “community action”. Analysis of abstracts done with tidyJSTOR, available at: <https://github.com/arthurbnetto/tidyJSTOR>. Poverty words accounted: “poverty”; “poor”. Delinquency words accounted: “delinquency”; “delinquent”. For more information see Appendix A.

discarded. Johnson was against hand-outs - he never supported major cash transfers - and this never became an option (Plotnick 1975). This left three options. In-kind transfers and direct services were used in a small scale. They were subsidies to help the consumption of certain goods or services, like food and health care.

The real competitors for the post of second major expenditure, then, were the remnant human capital programs. Without competition, it easily won the position. But human capital programs can themselves be divided into two other categories, train people or create jobs, and this division became a theme of heated discussions in the task force and afterwards. The secretary of Labor, Willard Wirtz, repeatedly contested training policies. He had been in Kennedy's administration and had the macroeconomic mentality of growth, employment and product. America needed more jobs. That was all for him.

Well, maybe that was not all. Wirtz has also been described as a "very jealous" bureaucrat. So, it must also be said that he knew that if more Jobs were America's future, the Department of Labor would be a central piece of the War on Poverty, whereas if training-programs were to be chosen, they would probably be divided among HEW and OEO, leaving the DOL aside (which actually happened). Jealous or not, he was a strong voice inside the government whose opinion had to be considered. It took Shriver and his fellows some effort to overcome his thought.

A conflict was ensured for the rest of OEO's lifetime. The Department of Labor would be in disarray with the OEO since the beginning. Community action and job training were not DOL's main responsibilities and would never be. What probably Shriver did not expect was that job training would also provoke buzz inside his agency. OEO's Community Action specialists also never approved training-programs. In their minds, job training programs were a top-down decision; there was no poor or community agency asking for that. This was purely a way of co-opting the poor in their scholar opinion.

Shriver tried to sell the idea for Community Action specialists that Job Training programs were a species of "department store" for community agencies. If the community noticed that the lack of training was a problem, it could check out OEO's catalogue and select one of its programs - like Job Corps or Head Start. In Shriver's view, job training

programs of national extension were a way to protect community action from criticisms (Shriver Oral History IV p. 30)

The specialists never bought the idea. The OEO had then one more conflict to deal with, but that was not all. To complete the disputes with CAPs, following McNamara's successful implementation of RAND's planning programming budgeting system (PPBS) in the Department of Defense, a Johnsonian presidential directive enforced OEO to include a branch of research and evaluation. The Office of Research, Programming, Planning and Evaluation (RPP&E) was created to obey the regulations.

Right away the evaluation division noticed that evaluation was implementable on job trainings, but hardly so on community agencies. Whether community action programs were delivering results would be a hard to answer question during the subsequent years. Being a hard question would not be a problem if RPP&E did not play a far-reaching role in the agency, but that was not the case<sup>18</sup>. Joseph Kershaw, head of the economic department of RAND from 1948 to 1962 and at the time a Professor at Williams College, became the first director of the Office. Since his arrival, he was responsible for signing an important document. RPP&E and its director were the main authors of the War on Poverty five-year plans – directly analogues of the five-year defense plans. This was no small responsibility. The plan was the document that defined all poverty actions for the following period, and had to go through Congress and the Bureau of the Budget.

Kershaw invited Robert Levine to help him in the task of implementing system analysis at the OEO. Robert Levine was also a RAND employee and was eager to start a new challenge. Adapting PPBS to the need of the OEO would be it. PPBS had been inserted in all government agencies in 1965 through Johnson's presidential directive. The analytical technique had been inserted in the Defense agency through Robert McNamara in 61 and since then was presenting impressive results (See Einthoven 2019), but its capacity of adaptation to different problems was still a mystery.

The assimilation of PPBS varied greatly among all government agencies (Harper et al., 1969). This was due to several reasons, but mainly because most agencies already had their workflow of analysis. It would take time and effort to implement PPBS adequately for

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<sup>18</sup> Forget (2011) discusses the clash between RPP&E and CAPS extensively

most of them. This was untrue for Kershaw and Levine's OEO. Recently created, it was fresh to use the most up to date metrics and tools from the Department of Defense and RAND. The choice of ex-RAND directors was a way of ensuring that. Harper et al. (1969) demonstrate that the OEO was the best agency in adopting the technique in the second half of the 60s when compared to the other 15 governmental agencies.

Thus, while in other agencies CAPs could have lived an easy life without worrying itself with quantifiable metrics and single outcomes, community agencies were submitted to a rigid regime of evaluations in the OEO. This emerges in analyses and reminiscences of the period as a feeling that measurability was too demanding at the OEO (see Aaron 1978).

Imagine it, if you will, Community Programs are scattered all around the US with the intention of enhancing the quality of life of the communities. They want to increase self-esteem, empower individuals, make institutional changes. These are not exactly measurable outcomes, right? It comes as no surprise that RPP&E could not deal very well with that. Robert Levine admits that they found what was the "best way so far" only in 1970, but still he did not seem satisfied with the tools he had in hand:

"[...] to make institutional change is inherently a qualitative aspect. That's to say – an apple is inherently not quantitative – but you can count apples. So you can count institutional changes in detail, kinds of changes, causes of changes, and so forth. They're doing this now, and that's the best way I think we've come across to try to really evaluate Community Action" (Levine oral history p. 13)

Until this point, things seem to have been rough during OEO's formative period. Conflicts were popping everywhere. As if technical problems were scant, CAPs faced an additional more humane concern: political distrust. Mayors of major cities despised the concept. They accused OEO of inspiring riots all over the country. Already in 1965, in Congress, Shriver was having to dribble political conundrums explaining that his agency was not against politicians nor behind any protest. In the court hearing for increasing OEO's budget, Senator Murphy, asked him the following:

"May I ask a question? There was a comment in Los Angeles, which is the largest city in my State, made by the mayor. He said: 'Mayors all over the United States are being harassed by agitation promoted by Sargent Shriver's speeches urging those he calls the poor, in quotes, to insist upon control of local poverty programs. The Shriver



organization can go ahead with the programs without the city having any voice in the programs at all and perhaps that is what is being planned, a real political boondoggle.' Would you care to comment on that? (US Expand the War on poverty 1965, p. 40)

Problems at OEO were enough for any agency to collapse. They were from all sorts of types: technical, ideological, political. And still, the agency survived. Years after the War on poverty ended, Shriver reflected on this. The point for him was that the programs were good. Time spoke for itself. Most of the War on Poverty programs had been living a long life when he was interviewed and Shriver did "believe that the longevity of them [spoke] well for the concept" (Shriver Oral History IV, p. 23)

Being proud and knowing that they were doing the right thing was powerless though, to avoid a restructure of the war and agency. The conflicts inside OEO were summed by an intellectual context that was aggressive towards community action. Well, not precisely towards community action but towards any sociological consensus about the way of combating poverty. Patrick Moynihan, assistant secretary of Wirtz's Labor Department and well-known figure in Washington politics, sparked a wave of social revolts and sociological debates after his 1965 report (US 1965), known as the "Moynihan Report" - but which the real name was "The Negro Family: The Case for National Action".

The report was written after Marquette Frye, an African American driver, fought with police near Los Angeles' Watts Neighborhood in August 1965. The report was a reflection on the subsequent days of the fight: The Watts Riots. Six days of uprising succeeded an acclaimed injury to a pregnant woman. Moynihan wrote about racial segregation and poverty in what can be said as an insensitive mode after those turmoiled days. Poverty was pathological in his report. The culture of poverty was taken to the extreme and it was received with distaste.

Moynihan's chosen tone concealed any underneath objective or good intention that the report could have had. The debate became public and the report was widely discussed inside and outside the academy. It was the spark to politicize the discussions about the culture of poverty. With this blatant insult to low-income families and African Americans, it did not take much to gradually bury any intention of applying the culture of poverty in governmental efforts.

While community action was not necessarily associated with the idea of dividing the poor under “deserving” and “undeserving” labels, it was closer to the culture of poverty than any job training program. The OEO had to be careful with its public appearances in an environment that was quick to bring academic ideas to politics and newspapers.

With every year that passed, it became clearer that the War on Poverty and the OEO had to be rebranded. Poverty was no more a mere campaign theme nor an exclusive specialty of sociologists, it was a complex and diverse phenomenon that deserved more than a macroeconomic attention from economists. The OEO would transform itself to abide to the modern times.

#### **4) 1969-1972: Governmental Think Tank for Experiments**

Those were intense years until time for a change has come. Lyndon Johnson, the name behind the whole project, was worn out. The War on poverty was not going as expected, and the real War, on the other side of the ocean, was going even worse. In 1968 the Vietnam War was consuming all government resources, and still, it did not look as it was going to end neither well nor soon.

On the Sunday, the 31 of March 1968, from the Oval Office, President Lyndon Johnson postponed prime-time programs to surprise the nation, appearing on television to inform that he would not run for re-election. With his Texan accent, set on the presidential chair, he said: “I shall not seek and I will not accept the nomination of my party for another term as your president”<sup>19</sup>.

Nevertheless, work still had to be done until the elections in November of that year. Four days after Johnson’s appearance on television, on the 4 of April, Martin Luther King was shot to death in a Memphis’ motel. The United States went on distraught, and the president had to deal with numerous riots over that year. A half-staff flag flew in the country on the 7 of April, signaling respect but also that change was over-due. With a promise of ending the War with honor and calming the situation at home, Nixon was elected at the end of that year.

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<sup>19</sup> Video available at: <https://www.youtube.com/watch?v=CJeLoMCF6Jo>

This brings us back inside OEO's headquarters. Shriver resigned from his post in late 67, when Bertrand Harding assumed his position. After the conflicts over community action and all the bad reputation of the culture of poverty that culminated from Moynihan's report, both topics silently gave their space inside and outside the Office's walls. There were no political reasons to be associated with those controversial ideas. No substantial evidence or results were supporting their theories either.

The first sentence in OEO's report of 1969 was Nixon's following quotation:

"I believe that the goal of full economic opportunity for every American can be realized. I expect the Office of Economic Opportunity to play a central role in that achievement. With new organizational structures, new operating procedures, and a new sense of precision and direction, OEO can be one of the most creative and productive offices in the government. For here much of our social pioneering will be done. Here will begin many of our new adventures." (US OEO Report)

But what exactly were these new structures and operating procedures? In a single page of the report all the answers could be read. They were direct. They were simple. Experimentation and testing were the new role of the agency:

"The risk of wasted public money can be reduced substantially through documented testing of new ideas for social programs. The President has assigned to the Office of Economic Opportunity the role of experimenting with innovative approaches to the problems of poverty." (US OEO report 1969, p. 1)

Action was no more OEO's focus:

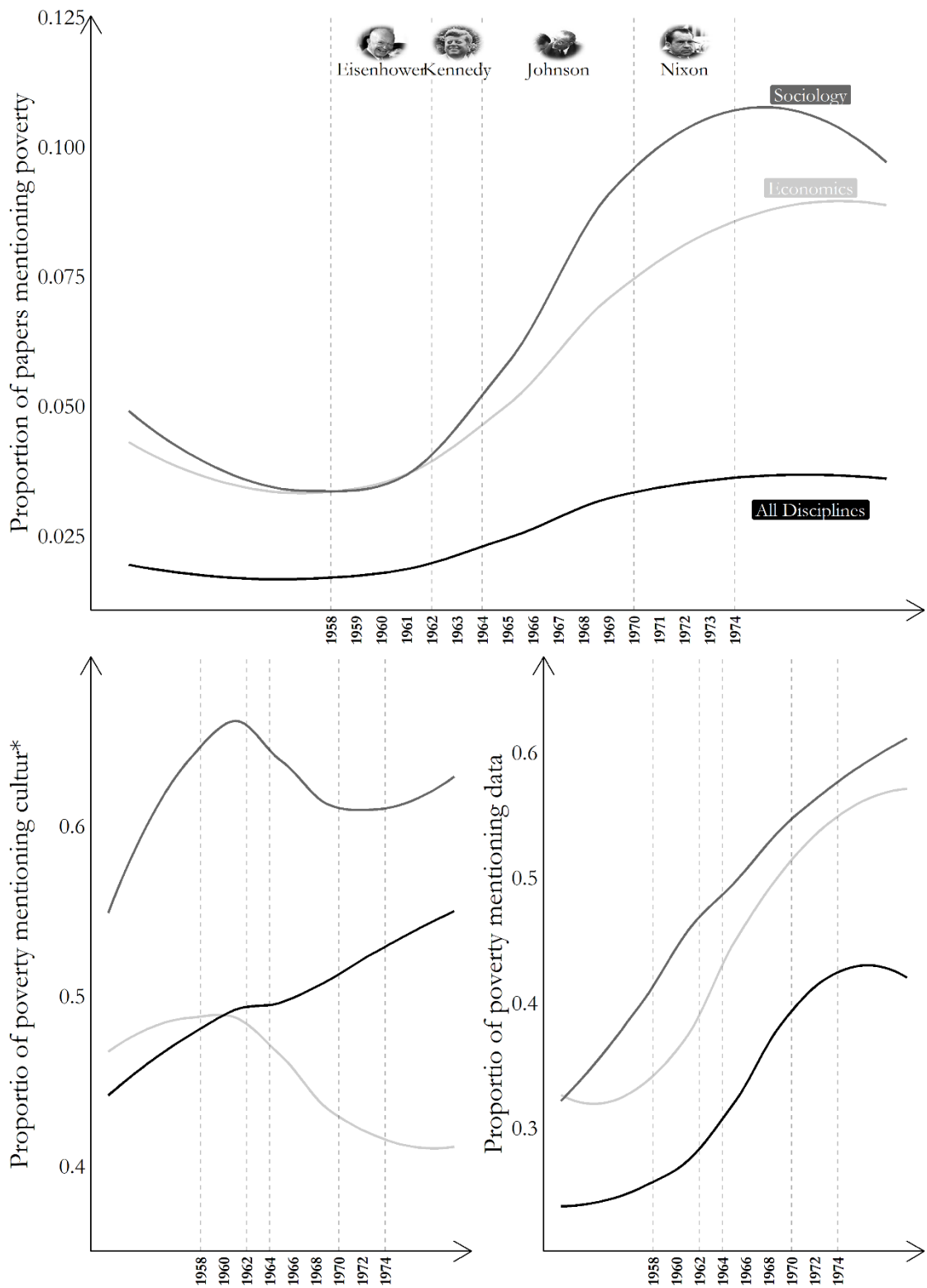
"mature operating programs run by the Office of Economic Opportunity have been turned over to other Federal departments and agencies, leaving the Office of Economic Opportunity free to develop and demonstrate different ideas" (US OEO report 1969, p. 1)

Job training programs were mostly divided among HEW and DOL. The previous Research, Planning, Programs, and Evaluation Office (RPP&E) became the Office of Planning, Research and Evaluation (PR&E) with an increased budget and new responsibilities. Leaving "program" outside its name and responsibility, OEO "was

strengthened” according to the report. Joseph Kershaw remained ahead of the evaluation and research activities.

As data from the poverty programs started to “materialize” in the late 60s (see Levine oral history), culture gradually lost importance in discourses. In JSTOR-indexed articles, mentions to poverty increased both in economics and sociology since the early 60s. However, they gradually diminished their user of culture-related terms and increasingly relied on data:

Fig. 3 – Cultur\* vs Data in papers mentioning poverty



Source: JSTOR<sup>20</sup>

<sup>20</sup> Searched terms at [www.jstor.org](http://www.jstor.org): poverty; poverty AND cultur\*; poverty AND data. Cultur\* is a wildcard that searches for all terms with the root cultur. This includes culture, cultural, cultures and so forth. For more information see Appendix A.

What can be hypothesized is that this was a natural route as data became increasingly available. PPBS strict format evolved inside the agency into a more embracing culture of data. The OEO was a highly specialized unit inside the government that attracted young and talented people. This was even more so during Nixon's term when the agency sought policy analysts with a Ph. D., especially in economics. Data and research were welcome there.

Since 1966 OEO had significant connections with universities and researchers. They were a few initiatives but had large-scale grants and important responsibilities. The contract with the Institute of Research on Poverty at the University of Wisconsin to support the advancement of the research on poverty exposes that. According to Evanson (1986), the institute started small at Wisconsin bolstered by the famous "Wisconsin idea", that impelled its economists to participate in politics and policy, and the influence of Robert Lampman, a strong name on poverty research affiliated with Wisconsin and government – remember he had advised Walter Heller in the early sixties.

Harold Watts was the first director of the institute from 1966 to 1971 and had to coordinate the task of being a "national center for study of the nature, causes, and cures of poverty." Lampman had his back as a second in command. The institute had 30 persons as permanent staff in 1969 but grew in the following years, becoming a place of training and community gathering. During the 60s and 70s, numerous PhDs were trained there, and several conferences were organized<sup>21</sup>.

Similarly, OEO supported other academic projects. Among them, the Panel Study for Income Dynamics (PSID) at the University of Michigan has to be under the spotlight. In 1966 and 1967, the OEO conducted a major survey of households to study the characteristics of poverty. Over 30,000 households had their doors knocked to participate in the effort. However, such a massive endeavor was unsupportable for the following years, which led the Office to contact James Morgan at the University of Michigan Survey Research Center (SRC) to continue the process, although with a sub-sample of the initial surveys (see McGonagle et al. 2012).

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<sup>21</sup> For more information on Ph.Ds and conferences see Focus special issue of 1986 (Uhr 1986)

Academic research centers such as the Survey Research Center and the Institute for Research on Poverty are a contesting force to strict governmental procedures. Academics evade the rules of bureaucrats. For instance, James Morgan was reluctant to accept the role as director of the Survey Research Center for not agreeing with the previous research methods (see Sherburne 2017). IRP only became a reality after imposing the “condition that the Institute exercise full authority in allocating grant funds to researchers, selecting research topics, and publishing the results” (Evanson 1986, p.2)

While PPBS lived its glory days in the early 60s, especially inside McNamara’s Department of Defense, in Nixon’s term, with its more academic face, was in dissonance with a broad implementation of the methodology. In the *Public Administration Review* of the late 60s, what could be read was a disappointment with PPBS and a foreseeable melancholic end (Botner 1970, Mosher 1969). New evaluation tools were on the horizon.

When Shriver left, space was more than enough for new tools to emerge. The context was favorable: new data was materializing, PPBS was falling, culture talk was avoided, action was not the focus of the agency anymore. Summing up to that, new programs were on their way. In contrast to Johnson, Nixon was not afraid of hand-outs; cash-transfers were a real possibility in his government. Nixon’s “Family Assistance Plan” was considerably discussed at the time and could be an aid for low-income families in the upcoming years. Still, Nixon and his agencies would not offer money without evidence that this would produce returns to society in a meaningful way. They were afraid of a negative effect. Maybe giving money to the population would be an incentive for them not to work. That, Nixon did not want.

This kind of discussion was already usual since the beginning of the 60s, when Milton Friedman coined the term: “negative income tax” (Friedman 1962). It lost traction during Johnson’s term, given his reluctance over hand-outs. Milton and Rose Friedman had been known as advocates of a cash transfer since the publication of *Capitalism and Freedom* in 1962, but economists more widely received the idea with caution. Most of them were worried about recoil effects on the job market. Still, no theory had proven sufficient to discern whether the preoccupation was valid until the late 60s. If cash transfers would be a reality, some tests or experiments would have to be made.

Inside government, HEW analysts had historically opposed to Community Action as a waste of money. The idea of guaranteed income had survived inside that department even during Johnson's years. In the view of Alice Rivlin – an influential analyst inside the agency responsible for revamping the budgetary process and trained by Guy Orcutt -, community action and other Johnsonian solutions were just random projects to deal with poverty. There was no “systematic thinking” before the choice of the programs (See Rivlin 1972). A guaranteed income was a unified solution in her view that could pass through the test of data (see Huret 2018 Ch 7, p 30-32). In 1968 she had already developed the idea of a program called BIG (Basic Income Guarantee) that could be submitted to a national test: a randomized controlled trial.

The OEO, as the new data think tank of the government, took responsibility over the guaranteed income issue. The debate was growing stronger, and answers had to be provided. To the like of Rivlin, time had come to spend the otherwise action money on finding answers. Large-scale social experiments were the most expensive kind of evaluation that one could think of and the kind of evaluation that was said to offer the most precise answer. As money was sufficient, that grew into a real strategy, and not a simple one. The realization of the largest randomized controlled trial ever done in America became the strategy of the Office to discover whether a negative income tax disincentivized labor.

The “Rural (North Carolina, Iowa) Income Maintenance Experiment” (1969-1973), the “Gary (Indiana) Income Maintenance Experiment” (1971-1974); and the “Seattle-Denver Income Maintenance Experiment” (1971-1978) were nation-wide programs of negative income tax estimated to cost up to 5 billion dollars in 1965 to the government. The experiments themselves cost around 110 million dollars. This was about a fifth of the whole budget of the OEO when it started with Shriver. But this time it was money for research, and not action.

Such project would not be completed alone. While in Shriver's term there was a reluctance of using outside contractors to evaluate government programs, this was different in Nixon's period. The OEO changed its operational format. It became a producer but also consumer of research. The OEO was the main contractor for outside evaluation firms in the



late 60s. The most significant story in this operational change is the birth of Mathematica Inc's branch of policy research.

Mathematica has a respectable place in the history of OEO. Mathematica was the main responsible for the Negative Income Tax experiments. However, its history starts earlier than that. Mathematica Inc emerged in 1958 as one of the several enterprises of the 50s and 60s that looked to provide external evaluations in business and defense problems. The success of RAND corporation leveraged the emergence of this kind of institution during the period. But it must be said that Mathematica was special in this milieu. Its initial fellows were Oskar Morgenstern along with several mathematicians from Princeton University: Albert Tucker, Ralph Gamory, Harlan Mills and Michel Balinski. Harold Kuhn, William Baumol and Richard Quandt also from Princeton joined soon after the foundation.

In 1968, Mathematica founded Mathematica Policy Research specifically to conduct the Negative Income Tax experiments. At that year, the Office of Economic Opportunity, led by Joseph Kershaw, made a Request for Proposals on a Negative Income Experiment after deciding the need for the experiment to be realized with the help of external agencies (see Wooldridge 2013 p. 28). Albert Rees, William Baumol – two of the most respected Princeton economists of the time - and Heather Ross, a brave young scholar, signed a proposal of the experiment under the auspice of Mathematica.

They triumphed and arranged a contract for the company. Once Mathematica was confirmed as responsible for the task, David Kershaw, Joseph's (OEO's director) son, became the president in charge of the freshly born policy research branch of Mathematica that managed the experiments. There were reasons for bureaucrats to be reluctant with Mathematica's first experiment being one of such enormous size. Thus, OEO opted to subcontract Wisconsin's Institute of Research on Poverty as co-responsible for the experiments. This would mainly avoid concerns about money flowing directly to a for-profit company.

IRP and Mathematica joined efforts to make the largest social experiment ever done in America: the New Jersey Negative Income Experiment. David Kershaw was the main director of the project, while William Baumol and Albert Rees from Mathematica and Robert Lampman and Harold Watts from IRP were the leading investigators. Heather Ross, one of

the initial proposers of the experiment to the OEO, was part of the investigation through her Ph.D. thesis - one the most expensive Ph.D. thesis ever done in economics, surpassing 5 million dollars at the time (Levitt and List 2009). Edwin Kuh was her advisor at MIT with Robert Solow and Michael Piore completing the committee.

Several NIT experiments followed the first one, and thus NIT, OEO, and poverty changed the course of the history of program evaluation towards economics. Poverty knowledge that was initially divided into Community Action and the Culture of poverty was suddenly Job Training programs and negative income taxes. Randomized Controlled Trial decided whether they were viable or not. Any qualitative judgments of the Shriver years were then gone.

But poverty knowledge was not the only idea being changed. The discipline of economics was also in movement. In the process of proposing and testing NIT, a network of research was being formed. Wisconsin was hiring staff in economics that had to be specialized in the evaluation of programs and was also training Ph.Ds. Princeton Economics department was providing researchers through Mathematica, and Harvard was involved through Heather Ross. Three of the greatest Department of economics of the 60s and 70s were in contact to discuss the development of the experiments. The Chicago Department, although indirectly involved in the experiments themselves, was also directly concerned with the results given that Friedman was considered the father of the negative income tax idea. The economics discipline had an eye over the whole thing.

This was unprecedented. Economists did not deal with randomized data up to that point. Randomized controlled trials or experiments were mainly a concern of other social scientists. Oakley (2000), in this regard, names few experiments during the first half of the 20th century in education, such as Hudelson (1928) and Pittman (1921). In criminology, in the 1930s a large-scale experiment was carried out for the first time: the Cambridge-Somerville youth study. And even in these fields this kind of research can be said to be unconventional at that time (Oakley 2000).

Econometrics was a whole other world that dealt with what Haavelmo (1944) named “Passive Observations”. Roughly, what that means is that, for economists, data is gathered externally, and they do not have any control over the collection of data or the data generating

process (dgp). Econometricians are solely consumers of data. They borrow data from institutions to their desks. According to Koopmans (1979), passive observations were already a tradition of economics among other sciences in the 1970s: “like the meteorologist, the economist has traditionally been confined to drawing inferences from passive observations, records of data generated by the turbulence of the atmosphere or the fluctuations and trends of economic life.”

Regardless of whether economists' involvement mattered or not, what followed from 1968 to the early 70s is known as the “Golden Age” of experiments (Oakley 2000, Rossi and Wright 1984)<sup>22</sup>. Large scale experiments were initiated all over the country. Beyond NIT, housing and health experiments were conducted.

There was a significant optimism about the programs and the evaluations. The experiments would polish negative income tax to its perfect form before implementing it to help the poor. It is not the point here to enter on the details of randomization and experiments. It suffices to know that experiments divide two groups equal in everything observable and unobservable that they make different giving a “treatment” to only one of them. This difference attached to a single factor allows researchers to make causal claims: “the treatment causes...”. With NIT experiments, researchers would be able to say things like “a cash transfer of x, will cause a reduction in employment of y%” – once “cash transfer” would be the “treatment”. Simple and precise, exactly the need of bureaucrats. No wonder researchers were optimistic about that.

But wait, there is a – not so - small puzzle here: there has never been a negative income tax in America since then. Something went wrong along the way with the Negative Income Tax experiments.

## 5) 1972-fwd: Evaluation beyond experiments – The birth of program evaluation in economics

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<sup>22</sup> Madaus and Stufflebeam (2000) name the period from 1958 to 1974 the “age of development” of evaluation, when large-scale experiments led the development of new knowledge on evaluation techniques.

Negative Income Tax experiments were huge endeavors. Thousands of individuals were being tested, hundreds of researchers were involved. Uncountable operational problems existed, starting with randomization. In some of the experiments, randomly picking individuals was not so easy as it seemed. And even if one could choose the right groups, some of the individuals would never show up or just disappear in the middle of the trial<sup>23</sup>.

Statistical methods evolved during the period and techniques were developed to handle minor problems in the selection of groups. However, politicians and bureaucrats did and do not want to know about this. They want results.

And results were something that neither IRP, Mathematica nor anyone could offer in a rapid manner. Their initial plans were of experiments that would last from 3 to 5 years. Imagine politicians waiting years to know the answers for something they were discussing yesterday in Congress. Well, they were, to say the least, unhappy about it. In the case of the Family Assistance Plan (FAP), discussions started in Congress in 1969. No major experiment was close to an end in that year. As Nathan (2000) pointed out: “As it turned out, the idea of a negative income tax as tested both in the New Jersey and the SIME/DIME [Seattle/Denver] studies seeped into the policy process long before the final results of the experiments were available.”.

To complement, researchers were spending a lot of public money on the experiments. To be clear, those were experiments where the treatment was a “cash transfer”. This “cash transfer” was occurring for the treated groups all months of the year over 3 to 5 years. In the eyes of politicians, it did not matter that it could save money in the future, this was money being spent right in front of their eyes without any practical return.

What researchers, bureaucrats, and politicians had then was a fascinating causal tool, but a very impractical one. It had a slow pace and cost a lot. It is not even necessary to consider that when results came out, they were inconclusive (researchers did not know whether cash transfers should be implemented after several years of research<sup>24</sup>). This happened in the mid-70s, and change had already come at that point.

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<sup>23</sup> This is what researchers call “attrition”. The bigger the experiment, the harder to cope with these problems.

<sup>24</sup> See US GAO report of 1981

Without achieving politicians' expectations, the OEO came to a sudden end in 1972. The programs and evaluations that remained active were delivered to other departments, mainly HEW and DOL. Evaluations were transferred to HEW's Office of the Assistant Secretary for planning and evaluation (ASPE) and DOL's Office of the Assistant Secretary for Policy, Evaluation and Research (ASPER). Those two were similar organizations that were responsible for evaluating the programs of their respective departments.

Social experiments became smaller and instead of testing new programs, they focused on analyzing incremental changes in existing programs (Greenberg and Robins 1986 and Greenberg et al. 1999). Long periods were unnecessary to produce results anymore; instead of lasting 3 to 10 years, experiments from 75 to 83 lasted 1 to 3 years.

Even with these changes, experiments for the most part fade out in government. Governmental agencies and departments could contract firms such as Mathematica to conduct evaluations, but they would be no more responsible themselves for the endeavor. The institute of research on poverty, for instance, - considered part of OEO – stepped aside from negative income taxes after OEO closed its doors and had to find new modes of functioning. At the same time, Mathematica resorted to a grant from HEW to maintain its operations.

A whole contract-driven market of evaluation had to reinvent itself. Nixon would not spend a dollar more on hazy experiments. This could have been the end of evaluation. After a long way from poverty research and PPBs to experiments, the lack of support could have been a strong blow into the young research agenda.

Time, however, had been enough for a safe network to be built. More precisely, policy evaluation had been professionalized. It had a life of its own beyond governmental demands. How to evaluate better had become a research question and would be part of universities from then on. Private institutions required evaluations for their programs and government was still able to hire minor evaluations.

This is not the most thrilling ending for such an intense journey, but it is a fact. Societies had been formed, academic journals released, graduate courses initiated (see Kimmel 1981, Rossi and Wright 1984). In the mid-70s, policy evaluation was a concern of

in economics departments. The War on Poverty and the data and methods that came with it had sparked a technical and theoretical research agenda.

It had not been long since economists worried solely with time-series. That was their specialty. Econometrics was macroeconometrics. Hood and Koopmans (1953)<sup>25</sup>, in the founding days of the field, were worried mainly with measurement errors and simultaneity, known problems of aggregated data and time-series - widely described as *endogeneity* nowadays. There was no *omitted variable* or *unobservable* immediately in their sight. Even measurement errors were regarded as a secondary topic once there was little that could be done to interact with data collection. Simultaneity dominated the heavily macroeconomic-biased literature.

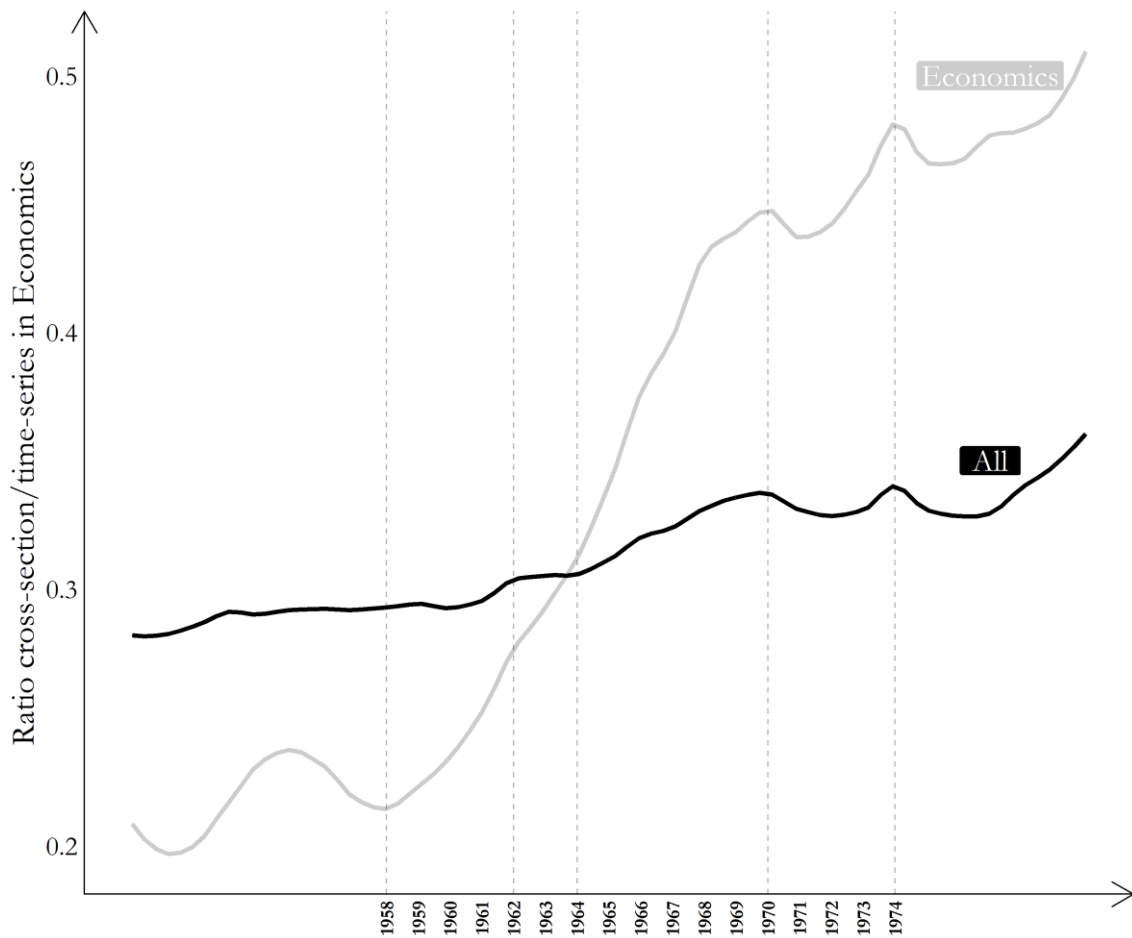
The first known uses of panels by economists were scant given the lack of data. The usually cited examples are Mundlak (1961), Hildreth (1949, 1950), Hoch's (1958) Ph.D. research, and especially Balestra and Nerlove (1966) (see: Nerlove 2000). These were either theoretical efforts (such as Hildreth's) or individual efforts for acquiring data (as that of Balestra and Nerlove). These first crusades in the realm of panel data had nothing to do with individuals. They were panels of states, cities, or countries (see Dupont-Kieffer and Pirotte 2011).

This means that randomization, unobservable and omitted variables were a new topic for economists in the 60s and 70s. You would never open an econometric textbook and find a section called endogeneity divided into simultaneity, measurement errors and omitted variables. This is a modern approach, something that could only emerge with cross-section and panel data. In JSTOR economics' indexed articles, mentions to time-series were almost five times higher than mentions to cross-section in the 50s. In a matter of 10 years, this changed dramatically. Cross-section was mentioned nearly one for every two mentions of time-series in the second half of the 60s:

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<sup>25</sup> Hood and Koopmans (1953), together with Haavelmo (1944), are two of the main references of the rise of econometrics and especially the "cowles commission approach" - the approach would dominate econometric theory until the 70s. For references on the history of econometrics and the cowles commission approach see: Morgan (1990), Qin (1993, 2010), Epstein (1987), Heckman (2001, 2010)

Fig. 4 - Cross-section vs. Time-Series



Source: JSTOR<sup>26</sup>

At this point, poverty knowledge, that had wandered through sociology and experiments, was crossing the realm of analytical policy evaluation. In economics, this is indistinguishable from the development of microeconometrics. The technical problems that lead to the failure of experiments and that came with the new set of data were the problems of young econometricians: How could one deal with panel data? How to avoid omitted fixed effects to influencing the estimators? Can we emulate randomization with econometric methods? What is the best way to deal with cross-sections? In which ways simultaneity is like the problem of unobservable?

Unlike money for evaluation, questions were plentiful. Microeconometrics came silently, but to stay. Economists understood that the data accumulated was perfect for solving

<sup>26</sup> Searched terms at [www.jstor.org](http://www.jstor.org): cross section; time series. For more information see Appendix A.

economic theory questions, especially in labor economics. PSID would fast become the go-to source for any labor economist in the 70s.

The new generation of economists faced the trial of having to solve the technical questions that came with the unavailability of experiments and an abundant number of panels and cross-sections. Cheaper, faster, and more reliable evaluations became the objective of researchers in government, private-sector and universities.

In the private sector, for instance, a few firms were established to conduct smaller-scale experiments. However, a gap in the market was also created for different evaluation approaches. Coming from the Office of Urban Affairs, in one interesting example, a host of young researchers created the Urban Institute. Microsimulation became its most well-known product (see O'Connor 2001, ch. 9). Microeconometric techniques were developing in the outdoors.

In universities, those with ties to the NIT experiments were quick to transmit the new questionings to their students. Albert Rees supervised or was in the committee of an extraordinary group of students in the late 60s and early 70s. The most well-known are John Pencavel in 1969, Orley Ashenfelter in 1970, James Heckman and Ronald Oaxaca in 1971. We are talking about people who were behind difference-in-differences (Ashenfelter 1978), Oaxaca-blinder estimators (Oaxaca 1973), a Nobel winning model, the sample-selection model (Heckman 1979) and the creation of the Journal of Economic Literature (JEL) Codes (Pencavel in 1988<sup>27</sup>). The connection NIT – Mathematica – Rees – Princeton was an active force in the development of microeconometrics and economics.

Albert Rees was also a friend of George Shultz (see Shultz Oral History), Nixon's secretary of labor in 1969-1970. He was responsible for restructuring OEO's evaluation inside the Department of Labor. Under Shultz, evaluation and research at DOL were revamped. Shultz, "an intellectual conglomerate"<sup>28</sup>, had been a noteworthy researcher himself before assuming positions in government. He had been part of the Chicago's Faculty of the 60s by the side of Friedman, Gregg Lewis, and Al Rees. Shultz and his friend Al had written an extensive research on labor markets that used the latest analytical tools emerging

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<sup>27</sup> See Cherrier 2017

<sup>28</sup> Shultz's description at the department of labor's official history. Available at: <https://www.dol.gov/general/aboutdol/history/dolchp07>



in the 60s with the War on poverty (Rees and Shultz 1970). At the time he was appointed secretary of labor, he was the Dean of the Chicago Graduate Business School. His view for ASPER was that of a prominent research facility.

ASPER was set on 14<sup>th</sup> and constitution in the same building as the Dep. of Labor in 1969<sup>29</sup>. It had “Very tall ceilings and was one of the first buildings built in Washington in the 1930s with central air conditioning.” recalls Orley Ashenfelter. Beyond its magnificent look, the building was also close to the White House and the center of the Washington Riots of the 60s. “And the fact is that the area around us impressed upon you that there was some urgency in trying to make those programs work.” (Ashenfelter Oral History III). Poverty was roaring one window away from ASPER researchers.

It was 1972 when Orley Ashenfelter was called to become the first director of the “E in ASPER” – evaluation more precisely (see Hamermesh in Krueger 2014). This was an academic choice, even though George Shultz had already left the Dep. Of Labor and James Hodgson had assumed his position. Orley had just finished his Ph.D. in Princeton under the supervision of Al Rees and was a professor at Princeton for a few years at the time. He had no experience in government though. Still, he was part of a younger generation of Ph.Ds trained in contact with cross-section and new labor economics tools. There were not many like him. Up to the 70s, labor economics was intertwined with labor relations and dominated by institutionalist approaches<sup>30</sup>. Analytical labor was trapped within government walls; few were the departments that had analytical training<sup>31</sup>. Princeton, Michigan, Columbia, Chicago, and Wisconsin were some of them because of their faculty’s connection to government.

Following Ashenfelter in the command of the evaluation division came George Johnson (1973-1974), Daniel Hamermesh (1974-1975), Frank Stafford (1976-77) and Alan Gustman (1978-79). All of them had connections to ‘analytical labor’ departments. Hamermesh was a 1969 Yale Ph.D. who taught at Princeton and was starting a new challenge

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<sup>29</sup> There are conflicting references to the year of foundation of ASPER. US ASPER 1980 points to 1969, while Krueger 2014, Ashenfelter and Hamermesh on interviews point to 1972.

<sup>30</sup> See Kaufman (1993, 2006) for accounts of the demise of institutionalism

<sup>31</sup> As its being told, analytical labor economics was a small field mostly constrained to governmental organs until the 70s. In universities it had a small reach that can be traced back to H Gregg Lewis in Chicago. For more references on the issue see: Rees (1976), Ashenfelter et al (1994) and Hamermesh (2020).

at Michigan State. George Johnson was a 1966 Berkeley Ph.D. with a longtime career as Professor of Michigan. Gustman made his whole career at Dartmouth and Stafford was a 1968 Chicago Ph.D. teaching at Michigan.

“We were all very young at that time. What we did was introduce and spend money on serious evaluation of labor programs.” Remembers Hamermesh (Hamermesh Interview I). That was impressive in a time when things were changing in government. “The experience was quite exhilarating. Much to my surprise, the office was left to do its work without political interference” (Ashenfelter 2014, p. xx)

In between serious evaluation, these researchers also found time to do what they were trained to do: research. The Office started a series of Technical Analysis Papers (TAPs). Those were technical papers in labor theory and microeconometrics. Most of them were also published as working papers for economics’ department and were later published by academic journals. They circulated freely and were intended to promote discussions. Their name though, was an inside joke:

“We called them technical analysis papers partly because at that time there were some unauthorized wiretaps and the Justice Department couldn't seem to keep track of. So when I would give up a talk on one of these papers. "this is TAP. Today I'm talking about TAP". this is true. I would say: "I would like the Justice Department, to know that the Labor Department actually know how many taps we have". (Ashenfelter Oral History II)

Jokes aside, the list of TAPs was impressive. Several researchers wrote them under grants from ASPER. Among them it is possible to find papers from James Heckman, George Johnson, Orley Ashenfelter, Lawrence Summers, Richard Freeman, Jacob Mincer and many more. Bobray Bordelon has made a preliminary list that counts more than 50 TAPs<sup>32</sup>.

A list of all projects and reports of ASPER is available as well (US ASPER 1980). In the document it is possible to find 27 reports focusing “on the economic, social, and policy background”; 85 reports about “the labor market itself—the nature and extent of labor supply and demand; the way in which workers, their unions, and their employers respond to the ebb and flow in the labor market; and the consequences of the operation of the labor

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<sup>32</sup> The full list can be found in Appendix B. I thank Bobray for his amazing librarian skills for the list.

market for various groups of workers”; 94 reports “concerning the nature and impact of the Department of Labor's programs”. And 41 reports about “how the Department could do a better job of administering its programs by developing better information for management and better methods for evaluating operations” (US ASPER 1980, p. X).

In the list it is possible to note that ASPER achieved a centrality that many economic departments could aspire but never have. Researchers from numerous universities and private institutes have contributed to ASPER research projects. All distinctive departments and researchers of analytical labor economics in the 70s received some form of support from ASPER.

The list has no large-scale experiments among the indexed projects though. On the contrary, most of the papers were technical developments of econometric techniques to deal with data in the absence of experimental data. The most well-known paper on the subject is: “Estimating the Effect of Training programs on earnings with longitudinal data” by Orley Ashenfelter in 1974. The “study dealt with the effect of training programs on earnings and the difficulty of implementing an adequate experimental design to obtain a group against which to compare trainees reliably.” (US ASPER 1980, p. 52)

The name may sound familiar, especially for labor economists, because it was published four years later in the Review of Economics and Statistics with the shorter: “Estimating the Effect of Training Programs on Earnings” (Ashenfelter 1978). The paper is widely known to be the first presentation of the difference-in-differences estimator.

The difference-in-difference estimator is an exemplification of the transformation that economics and econometrics were facing. Without the money and fanciness of large-scale experiments, econometrics came back to the table as an unexpensive way of evaluating programs. However, the previous macroeconomic-laden theory was precarious to solve problems of panel data and cross-sections. Economists had then to embark on a journey to solve new technical problems.

Economists knew the capabilities of randomization. They had seen it in the previous years. “My own conclusion was that randomization was the only transparent and credible cure for this problem [dealing with unrandomized data]” said Ashenfelter (2014) when

recalling about the time. The question then was how they could apply the same standards of randomized trials to unrandomized data. Diff-n-diff did that successfully.

Inside ASPER, then, researchers were enlightening and solving a concealed feature of passive observations: omitted variables. They were transforming their careers and their field in doing it: “This time in Washington led to a lot of stuff that we did later on and a lot of people paid attention to. So I would think about ASPER being this sort of breeding ground, if you will, for an awful lot of researchers in labor economics in that and subsequent periods.” (Hamermesh Interview I).

ASPER is, thus, the end of the long story of how econometrics made its way into policy evaluation and how it was shaped by it. That is the long story of the birth of omitted variables in econometrics and the emergence of microeconometrics. At the point where this story ends, microeconometrics, evaluation of programs and omitted variables had already become the everyday of economics department all over the US.

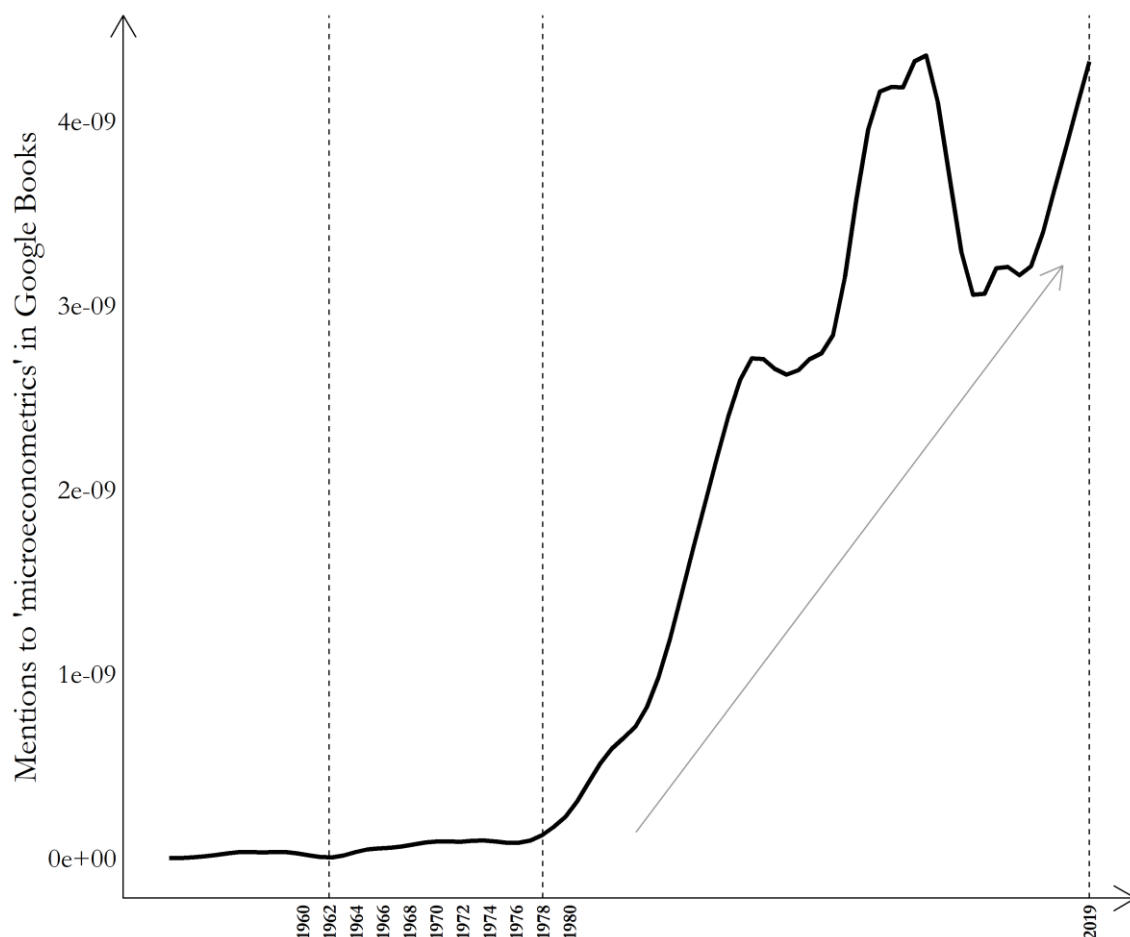
## 6) Concluding remarks

One year after Ashenfelter’s paper was published, in 1979, James Heckman identified a specific type of omitted variable, self-selection, and presented his solution as an *Econometrica* article named “Sample Selection Bias as a Specification Error” (Heckman 1979). Instead of simulating randomization, though, Heckman modeled out the selection bias. Economics had changed. It had come a long way from its initial unknown incursion on the problem of poverty as a “campaign theme”. Economics was dealing with cross-section, panel data, social issues, omitted variables, unobservables, and self-selection. Microeconometrics was a new area of research that enticed young researchers. It was the study of complex econometrics estimators that emerged from the necessity of researchers making evaluations in the absence of experimental data.

Governmental walls enclosed the formative years of microeconometrics, but when it was set free - being published in important journals with Heckman and Ashenfelter, finally receiving its deserved token of an academic field - it grew fast and strong. Mentions to

“microeconometrics” in google books broke out after 1978/1979, marking the beginning of a new economic field:

Fig. 5 - Mentions to ‘microeconometrics’



Source: Google Books<sup>33</sup>

ASPER’s research is the end of a history about how randomization became the gold standard for economists and how they learned to rely on their econometric methods instead of strict experiments. Labor economists on the 70s and forward would face the same problems over and over again. Solving omitted variables and self-selection became the task of the microeconometrician while dealing with simultaneity stood the job of the macroeconometrician.

<sup>33</sup> Data for fig. 5 available at: [https://books.google.com/ngrams/graph?content=microeconometrics&year\\_start=1950&year\\_end=2019&corpus=26&smoothing=3](https://books.google.com/ngrams/graph?content=microeconometrics&year_start=1950&year_end=2019&corpus=26&smoothing=3). For more information see Appendix A.

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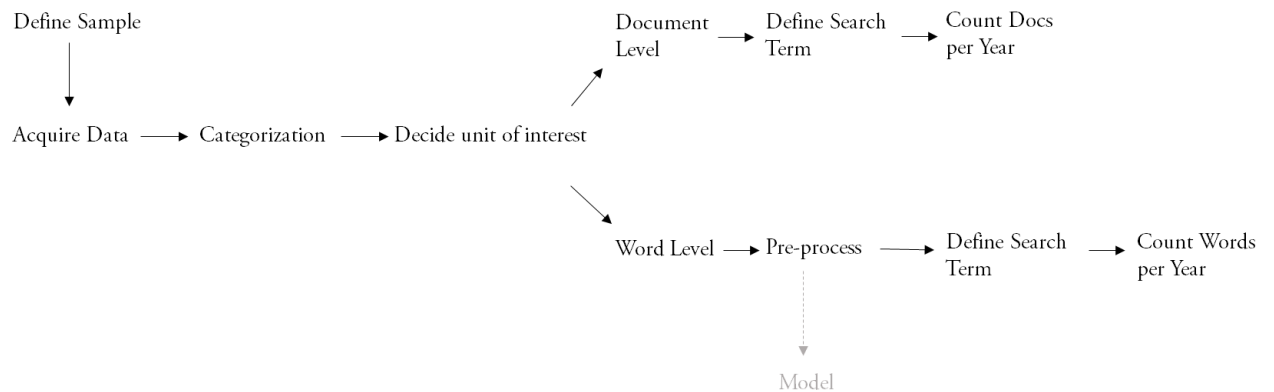
## Appendix A – Text mining

The use of computational methods in the history of science, and especially in the history of economic thought, has seen an upsurge in the last decade. Text mining and bibliometrics have been the two most used sources of information in this period. Researchers have applied mostly machine learning tools but also simpler methodologies. Overviews of the methodologies can be found in numerous places. Wilkerson and Casas (2017), grimmer and Stewart (2013) and Nguyen et al (2020) provide interesting introductions to computational methods for text mining. The *Journal of Economic Methodology* special issue of 2018 has a long discussion about the upsurge of computational methods in the history of economic thought and their impacts and constraints.

This appendix deals with the application of simple, even though enlightening methodologies. Namely, word counts, and document counts over time (see Hoover 2004 for an early and leading reference on the issue). Every day, researchers perform searches in scientific databases such as google scholar, JSTOR and ideas.repec. Their results are normally presented by relevance (according to the algorithm calculation of best match for the search). This makes sense, given that researchers are normally concerned with finding references. But what if the researcher is interested in the evolution of that search over time?

Well, it is totally possible to organize the results per date, although in any of those databases the researcher would be able to view how many papers are available for each year. Is this transformation that this appendix is concerned with. How is it possible to use research databases for analyzing time-series of words and documents? The following workflow extends the data science strategy for the creation of the time-series of interest.

Fig. 6 – Text mining time-series workflow



Source: done by the author

The first step, then, is the simple step already common to every researcher: defining a broad sample of documents for search. More practically, this means choosing a database like JSTOR for social scientists or philpapers for philosophers and, if necessary, constraining it to search terms. Even though this may be overlooked, this is an important step. No database is complete, and they define the representativeness of the search. A search for the term “poverty” has different meanings in JSTOR, philpapers and Google Scholar, for instance.

Moreover, it is the defined sample that will normalize the results. The number of documents and words change over years, normally because of the private nature of this institutions. JSTOR, for instance, holds contracts with publishers that hampers it from providing access to recent research. This means that the sample is biased for final years. Normalizing the result by dividing by total document count helps with the issue, but it is still important to realize exploratory data analysis (EDA) for knowing the characteristics of the sample and avoiding unknown biases.

In the sequence, the researcher must acquire the data through web scrap (manual or computational), APIs or online services. This paper uses four different samples, acquired in different forms: Economic Reports of the President, JSTOR abstracts for the search “community action”, Google Ngrams and JSTOR database as a whole. How each data has been acquired will be explained briefly soon, but first it is important to not skip the third step: categorization. After defining the sample, it is necessary to categorize it accordingly to

the interest of the historical research. Normally this means grouping them by years, but this could be also different grouping strategies.

Finally, with a categorized (divided by year) sample, it is possible to define the unit of interest, words of documents. This might sound confusing, but it is simple. When realizing a search in a database, researcher see the number of documents that fit the search criteria. A search for “poverty” in JSTOR yields 527.822 results<sup>34</sup>. This means that JSTOR found 527.822 documents that have the word poverty at least one time on them, not that the word was counted that amount of times.

If we were interested in a word count, we could then download all papers - or all abstracts - to count how many times a specific term has appeared. In the case of “poverty”, we could be interested in “malnutrition” or “school” for instance. When counting words, differently from when dealing directly with document count, data needs to be pre-processed. This means that texts have to be tokenized: transformed in unigrams, bigrams or trigrams through the method of “bag of words”. In R, programming language utilized for this paper, `tidytext` and `tm` are two of the most used packages for this task. For technical reference see: <https://CRAN.R-project.org/package=tm> and <https://CRAN.R-project.org/package=tidytext>. An interesting technical introduction for text mining and text analysis in R is provided by: Silge and Robinson (2017) and Jockers (2014).

In more technical analysis, after pre-processing the data, researcher may follow the route of modeling for different insights. Those would be mostly machine learning models such as topic models and supervised models. In this paper, these methods were not used.

### *Samples, acquisition of data and treatment*

#### *Economic Reports of the President – Fig. 1*

All reports from 1958 to 1974 were downloaded as PDF files from: <https://www.presidency.ucsb.edu/documents/presidential-documents-archive-guidebook/the-economic-report-the-president-truman-1947> [accessed on 11/19/2020]. PDFs were read using R version 4.0.2 (2020-06-22) with the help of the following packages:

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<sup>34</sup> Search realized on 11/19/2020

tm, tidyverse, tidytext and pdftools. Visualizations were produced using ggplot2, patchwork and extrafont. All packages were available at the The Comprehensive R Archive Network (CRAN). Code is available under contact with the author.

The time-series were created by counting the number of hits in the following string arrays: poverty, poor, opportunity and community; growth, development. Both counts were normalized by the total number of words for each year.

#### *JSTOR Abstracts – Fig. 2*

Figure 2 was built using data from JSTOR data for research tool. [jstor.org/dfr](http://jstor.org/dfr) provides access to .xml files containing metadata, with abstracts, for a given search. The searched term was “community action”. No quotation marks were used when realizing the search at JSTOR. XMLs were read using R version 4.0.2 (2020-06-22) with the help of the following packages: tidyJSTOR and tidyverse. Visualizations were created using ggplot2 and extrafont. tidyJSTOR is an R package created by the author for reading JSTOR dfr data. Available at: <https://github.com/arthurbnetto/tidyJSTOR>.

The time-series were created by counting the number of hits of the following string arrays in the abstracts: poverty, poor; delinquent, delinquency. Both counts were normalized by the total number of words for each year.

#### *Google ngrams – Fig. 5*

Figure 5 is a simple reproduction of the plot available at [https://books.google.com/ngrams/graph?content=microeconometrics&year\\_start=1950&year\\_end=2019&corpus=26&smoothing=3&direct\\_url=t1%3B%2Cmicroeconometrics%3B%2Cc0](https://books.google.com/ngrams/graph?content=microeconometrics&year_start=1950&year_end=2019&corpus=26&smoothing=3&direct_url=t1%3B%2Cmicroeconometrics%3B%2Cc0). For more information about Google Ngram Viewer see: Lin et al (2012) and Michel et al (2010). The plot was reproduced with R version 4.0.2 (2020-06-22) and ggplot2.

#### *JSTOR documents – Fig 3 and 4*

Figures 3 and 4 were created through the count of documents for searches in the JSTOR website. Terms were searched for individualized years and annotated. For instance, “poverty” for from 1950 to 1950 in the economics discipline, then from 1951 to 1951 and



so forth. Results were normalized by the total number of documents in JSTOR for the discipline in question. This was acquired through a search for an empty string in [jstor.org/dfr](http://jstor.org/dfr). A gentle web scrap was behind the acquisition of data [number of hits for a certain term each year], with a request time of 30 seconds to 60 seconds, with a mean time of 45 sec.

Plots, web scrap and data treatment were made using R version 4.0.2 (2020-06-22) with the help of the following packages: tidyverse, rvest, reshape2, lubridate, data.table, extrafont and ggplot2. All packages were available at the The Comprehensive R Archive Network (CRAN). Code is available under contact with the author.

## Appendix B

Measuring the Effect of the Federal Government on the Change in the Labor Market Position of Black Male Workers Relative to White Male Workers: 1966 to 1970	1973-04
Using Estimates of Income and Substitution Parameters to Predict the Work Incentive Effects of Various Income Maintenance Programs: A Brief Exposition and Partial Survey of the Empirical Literature	1973-06
Minimum wage legislation in the United States	1973
Minimum Wage Legislation in the United States: Comment. II. Minimum Wage Legislation in the United States: Reply	1976-05
Compensating Wage Differentials and Hazardous Work	1973-08
Estimating the Benefits of Job Banks as a Computerized Record Keeping System	1973-08
Effect of unemployment insurance laws and administration on unemployment rates	1973
Neighborhood Youth Corps: An Impact Evaluation	1973-09
Economic considerations for manpower revenue sharing	1973
Progress Report on the Development of Continuous Performance Information on the Impact of the Manpower Development and Training Act	1973-10

Effect of Manpower Training on Earnings: Preliminary Results	1974
Predicted Impact of the Black Lung Benefits Program on the Coal Industry	1973-12
Evaluation of the WIN 2 program	1974
Further Evidence on the Impact of the WIN II Program	1975-01
Revised Estimates of WIN Job Placements in FY 1973 and FY 1974	1974-10
Revised Estimates of WIN Job Placements in FY 1973 and FY 1974. Appendix: Sampling Procedures	1974-10
Impact of the WIN 2 program on welfare costs and recipient rates	1975
Economic Incentives and Occupational Safety	1974-04
Some evidence on the effect of manpower training programs on the Black/white wage differential	1974
Economics of job satisfaction	1974
Economist's View of Job Satisfaction and Worker Alienation	1975-01
Land Reclamation Requirements and Their Estimated Effects on the Coal Industry	1975-01
Statistical Theory of Discrimination in Labor Markets, Appendix A	1975-01
Indexing for Inflation: Some Formal Models and an Informal Policy Proposal	1975-05
Econometric Studies of Labor Demand and Their Application to Policy Analysis	1975-06
Stability of the racial unemployment differential	1975
Unemployment insurance, duration of unemployment, and subsequent wage gain	1975
Unemployment Effects of Minimum Wages	1976-02
Critique of tax based cost/benefit ratios	1976
Labor market displacement effect in the analysis of the net impact of manpower training programs	1976
Constant-Utility Index Numbers of Real Wages	1976-06
Effect of direct taxes and other factors on money wage changes in U.S. manufacturing	1977

Costs of Defined Benefit Pension Plans and Firm Adjustments	1977-04
Influence of Fertility on the Labor Force Behavior of Married Women	1976-04
Bias in the Estimates of Treatment Effects in Quasi-Experimental Evaluations	1977-08
Tax Base of the U.S. Unemployment Insurance Tax: An Empirical Analysis	1978-10
Unemployment insurance tax and labor turnover	1978
Three Paths from Disability to Poverty	1978-10
Distribution of Unemployment Insurance Benefits and Costs	1978-10
Labor Force Transitions and Unemployment	1978-10
Labor force participation--timing vs. persistence	1979-01
Demographic Composition of Cyclical Variations in Employment	1979-01
Compliance with Standards, Abatement of Violations and Effectiveness of OSHA Safety Inspections	1981
Study of the number of persons with records of arrest or conviction in the labor force	1978
Concepts and Measures of Structural Unemployment	1979-03
Implications for fiscal substitution and occupational displacement under expanded CETA Title VI	1979-03
Effect of Minimum Wages on the Youth Labor Market: An Expanded Model	1979-07
Person and organization characteristics involved in CETA program effectiveness: issues in need of research	1979-08
Nature of the youth employment problem: a review paper	1980-03
Economic Benefits from Manpower Training Programs	1976-11
Effect of the UI System on Labor Force Behavior	1978-01
Effect of the UI System on Labor Force Behavior (superceded)	1977-08