Wesley C. Mitchell on Value Judgments in Economics

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Artigo Submetido ao XIX Encontro da Associação Keynesiana Brasileira
Área 9. Economia Institucional Original

Resumo*:* Este artigo examina a contribuição de Wesley Clair Mitchell ao debate sobre a dicotomia fato/valor em economia. Argumenta-se que Mitchell desenvolveu uma concepção sofisticada da relação entre valores e objetividade científica, antecipando, em termos substantivos, elementos centrais da filosofia da ciência contemporânea, particularmente os argumentos de Heather Douglas e Helen Longino. Mitchell reconhece que valores não-epistêmicos permeiam inevitavelmente a investigação econômica e propõe, contudo, uma solução procedimental: valores definem os fins da investigação, enquanto o método empírico, a verificação intersubjetiva e a colaboração interdisciplinar funcionam como salvaguardas contra a distorção produzida pelo "*wishful thinking*".

Palavras-chave: Wesley Clair Mitchell; valores em economia; objetividade científica; institucionalismo original; dicotomia fato/valor.

Abstract: This article examines Wesley Clair Mitchell's contribution to the debate on the fact/value dichotomy in economics. It argues that Mitchell developed a sophisticated conception of the relationship between values and scientific objectivity, anticipating, in substantive terms, central elements of the contemporary philosophy of science, particularly the arguments of Heather Douglas and Helen Longino. Mitchell recognizes that non-epistemic values inevitably permeate economic research, yet he proposes a procedural solution: values define the ends of inquiry, while the empirical method, intersubjective verification, and interdisciplinary collaboration act as safeguards against the distortion produced by "wishful thinking".

Keywords: Wesley Clair Mitchell; values in economics; scientific objectivity; original institutionalism; fact/value dichotomy.

JEL Codes: B15, B25, B41

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1. Introduction

The fact/value dichotomy in economics is a recurring and unresolved theme in the methodological literature. The historically dominant position within the discipline, inherited from logical positivism, treats value judgments as extra-scientific elements that must be excluded from rigorous economic analysis. On this view, economics is idealized as a value-free science, a conception traceable directly to the legacy of Lionel Robbins and, more influentially — if less rigorously grounded — to Milton Friedman's famous methodological essay, which argued that the scientific status of a theory resides purely in its predictive capacity, thereby isolating positive from normative analysis (Boumans and Davis, 2016, pp. 169–170).[3]

This aspiration has roots in Max Weber's influential essay on "Objectivity in the social sciences" (Weber, 1904/1949), which advocated *Wertfreiheit* as a methodological ideal. Weber's formulation, however, was considerably more nuanced than the versions later adopted by economists, since it acknowledged that values necessarily guide the selection of research problems, while insisting that the process of scientific investigation should be conducted without normative interference. The conceptions of Robbins and Friedman tended to flatten these nuances into a sharper positive/normative divide (Reiss, 2017, pp. 135–136).

Yet the contemporary literature has increasingly challenged this inheritance. Boumans and Davis (2016) observe that economists inevitably operate with methodological value judgments — in the choice of research object, the method of investigation, and the criteria of validity. Małecka (2025a, p. 215) deepens this critique, arguing that the standard narrative concerning the role of values in economics is reductionist. On her account, discussions within the discipline are far richer and more nuanced than the dominant view suggests. Moreover, Friedman's legacy, she argues, produced an undesirable separation between theoretical work and policy formulation, at odds with how earlier economists thought about the matter (Małecka, 2025a, p. 219).

Along similar lines, van Staveren (2024, p. 161) argues that economic science is permeated by "normative empirical concepts," in which facts and values are inseparable and frequently employed in applied economics without due reflection on their evaluative weight. Mongin (2006), in turn, rejects both absolute neutrality and absolute non-neutrality, proposing weak versions of each. In the broader philosophy of science, Hilary Putnam (2002) argued that the very distinction between facts and values rests on untenable philosophical foundations, while Heather Douglas (2009) developed a nuanced framework distinguishing between the "direct" and "indirect" roles of values in science. Helen Longino (1990, 2002), from a social epistemological standpoint, proposed that objectivity is not a property of individual cognition but of communities organized around shared critical norms.[4]

[3]This is also the view of Schumpeter ([1931] 1982) and, for present purposes, that of Blaug (1992, chap. 5). There is thus an almost continuous interpretive line according to which, although economics is inevitably permeated by values, such values should, and eventually can, be eliminated from economists' scientific production.

[4]The same points are emphasized by Małecka (2025b) and Boumans (2025). According to Małecka, the methodological discussion in economics should be more grounded in discussions in the philosophy of science, where it has been acknowledged for decades (unlike in economics) that science is value-laden and that the important methodological step is to identify and explain how values enter research practices. Boumans (2025, p.

This article draws on the contributions of Wesley Clair Mitchell to argue that he dealt in a singular fashion with the fact/value dichotomy. We seek to contribute to the understanding of Mitchell's theoretical and methodological work by showing that he offers an account of how (i) values structure scientific investigation, (ii) without reducing science to ideology, and (iii) without making objectivity dependent on the positivist illusion of neutrality. We argue that Mitchell's resolution of this apparent paradox stands remarkably close to the more recent descriptive approach to the normative dimension of specific practices of economic research (Małecka, 2025a, 2025b). Indeed, Mitchell anticipates, in substantive terms, key elements of what Douglas (2009, pp. 16, 86) calls the "objective and value-laden" conception of science — that is, the view that non-epistemic[5] values inevitably permeate scientific reasoning, yet without thereby undermining its epistemic credentials, provided they follow appropriate procedural norms.

The selection of Mitchell's texts that underpin this article warrants a methodological note. Mitchell was a prolific author, with a vast bibliography spanning research articles, lectures, presidential addresses to various academic associations, and institutional reports. Given this breadth, the selection adopted here prioritizes texts in which Mitchell explicitly addresses the relationship between values, objectivity, and scientific method. The core of the argument rests on three fundamental texts — "Human Behavior and Economics" (1914), "Feeling and Thinking in Scientific Work" (1940), and "Facts and Values in Economics" (1944) — which constitute Mitchell's most direct formulations on the fact/value problem. To these central texts, we added textual evidence from his presidential addresses to the American Statistical Association, "Statistics and Government" (1919a) and "Need for Systematic Development of Social Statistics" (1919b), which reveal the institutional and practical dimension of his conception of objectivity; the text "The Research Fellowships of the Social Science Research Council" (1926), which documents Mitchell at work in the institutional construction of social science; and, especially, "The Public Relations of Science" (1939), his valedictory presidential address to the American Association for the Advancement of Science, where Mitchell articulates the thesis that scientific research is a social process intrinsically interwoven with values, institutions, and democratic commitments. The inclusion of these supplementary texts makes it possible to demonstrate that Mitchell's position on facts and values is neither a late reflection nor an isolated one, but rather a thread running through three decades of intellectual production, from his earliest institutional engagements in the interwar period to his mature reflections on science and democracy.

71) shows how the norms and values that define scientific "rigor" are relative to time and the community of economists. Thus, he finds no fewer than five definitions of scientific rigor applied in economics, all of them carrying a particular notion of "objectivity": "It becomes clear from the practices discussed that each economist mentioned above was deeply concerned with objectivity and searched for a concrete interpretation of rigor that fitted their image of economics as an objective science."

[5]The distinction is drawn by Małecka (2025b, p. 215): "Epistemic values are understood as normative commitments that constitute grounds for choosing one theory over another (e.g., simplicity, internal and external consistency, explanatory power, predictive accuracy, breadth of scope). (…) non-epistemic values are normative commitments related to moral, social, and political life." This corresponds to the "methodological" and "evaluative" value judgments of Blaug (1992, p. 114).

To support this argument, the article is divided into two main parts. In the first (sections 2 and 3), we situate Mitchell within the pluralism of economic ideas of the interwar period and highlight his critique of excessive abstraction in economic theory. In the second (sections 4 and 5), drawing on extensive textual evidence, we show how Mitchell resolves the tension between retaining non-epistemic values and preserving scientific objectivity. Some concluding remarks follow (section 6).

2. The Philosophy of Science and Mitchell

Wesley Clair Mitchell (1874–1948) occupies a prominent position in the history of American economics. A professor at Columbia University, long-standing founder and research director of the *National Bureau of Economic Research* (NBER), and a leading figure in the institutionalist tradition, Mitchell worked at a time of profound transformation in the understanding of what economic science could and should be. As Dorothy Ross (1991, p. 313) observes, Mitchell labored in a period when history ceased to provide clear moral teleologies and society came to be seen as an unstable "natural process." Drawing on a passage from Mitchell's own "Social Problems and the Social Sciences," she illustrates how he theorizes from the lessons of history. According to Mitchell, "social conditions and social organizations are subject to a ceaseless process of change" — what we have called, elsewhere, a theory of cyclical economic change (Fucidji and Neris Jr, 2026).

This understanding of science as a socially situated activity finds its most explicit formulation in Mitchell's 1939 AAAS presidential address, "The Public Relations of Science." There, Mitchell states as a fundamental proposition that scientific research is a social process as much as business, political or religious activities are, and as such is interwoven with all other social processes, influencing them and being influenced by them (Mitchell, 1939, p. 604). This formulation constitutes the central axis of the address and unfolds into a historical analysis of the relations between science and society. For the purposes of our argument, this proposition has two implications: on the one hand, it roots scientific activity in the social fabric, denying that science can operate in an axiological vacuum; on the other, it provides the foundation for the procedural solution that Mitchell proposes to the fact/value problem. In other words, if science is a social process, then objectivity must likewise be a social achievement, attained through institutional procedures rather than through the suppression of the researcher's individual values.

The interwar period was marked by intense methodological debates, driven on the one hand by the perceived distance between economic science as then practiced and logical positivism, and on the other by original institutionalism's insistence on developing an economic theory that addressed processes of change (Veblen, 1898) — that is, a theory capable of moving beyond logical positivism. It was a period of methodological pluralism, in which different notions of "rigor" and "scientific objectivity" coexisted (Morgan and Rutherford, 1998).

Mitchell actively participated in these debates, but from a distinctive standpoint. As Ross (1991, p. 305) puts it, for Mitchell, statistics and empiricism became a scientific *ethos*, in which "to do the job well is to practice virtue." This *ethos* is deeply tied to a reformist mode of thinking that characterizes his scientific procedure: "It is not lack of will that impedes progress,

but lack of knowledge. We putter with philanthropy and coquette with reform.... What we need as a guide for all this expenditure of energy is sure knowledge of the causal interconnections between social phenomena."

Objectivity is therefore a procedural achievement, built upon self-control, disciplined methods, and the intersubjectivity of data. This procedural conception of objectivity, as we shall see, anticipates arguments developed decades later by Longino (1990, 2002), who likewise located objectivity in social practices and institutional arrangements rather than in the epistemic virtues of individual researchers.

Other economists during this period also engaged with the fact/value dichotomy. Clarence Ayres (1935) and Frank Knight (1935) dealt with the question of values, but both agreed that economic theory carries values — the debate revolved around which values were the appropriate ones. Gunnar Myrdal (1953) insisted that all social research is inevitably conditioned by values and that the only honest path is to make one's own evaluations explicit, a prescription that anticipates the "transparency" requirement later developed by Douglas (2009) and that Mitchell, as we shall see, independently articulated in remarkably similar terms. Joseph Schumpeter (1931/1982), from an instrumentalist perspective, sought to preserve the scientific credentials of economics by distinguishing between the "vision" that inspires a theoretical system and the analytical apparatus that can be subjected to logical and empirical tests.

Mitchell's contribution, however, is distinctive in combining a sophisticated awareness of the role of values with a practical program of empirical research that would minimize the distortions they introduce. While Weber (1904/1949) demanded that the scientist suppress his values during the investigative process, and while Myrdal insisted that values be made explicit at the outset of that process out of intellectual honesty, Mitchell proposed an architecture centered on statistical measurement, intersubjective verification, and interdisciplinary collaboration — an architecture that would allow values to motivate research while at the same time safeguarding the objectivity of results.

The philosophical foundations of Mitchell's position can be traced to the pragmatist epistemology of John Dewey. For Dewey, inquiry is a situated and purposive activity that arises from problematic situations and aims at their resolution. Knowledge, in this framework, is produced for action, and its validity is assessed by its capacity to resolve the problems that motivated the inquiry. This pragmatist conception dissolves the sharp dichotomy between facts and values that characterizes the positivist tradition, since the very identification of a situation as "problematic" — the first step in any inquiry — involves an evaluative judgment (Dewey, 1938, p. 107).

Mitchell absorbed this lesson from Dewey during his formative years at the University of Chicago, and it remained the philosophical foundation of his approach to economic science throughout his career (Hirsch, 1988; Rutherford, 1999).

3. Mitchell's Critique of Abstraction in Economics

Mitchell's dissatisfaction with the abstract character of economic theory is a persistent thread running through his career, from his earliest writings in the 1910s to his mature reflections in the 1940s. In general terms, Mitchell criticizes the psychological assumptions underlying neoclassical theory and the methodological consequences of relying on untested deductions. His critique, though expressed in the language of his own period, addresses problems that remain central to the philosophy of economics to the present day. As Mäki (2002, pp. 15–17) has argued, the question of the "realisticness" of assumptions is not merely an empirical matter but an ontological one — that is, it concerns the relationship between theoretical representations and the structures of the social world they purport to describe. Mitchell's insistence on psychological realism can thus be read as an early demand for what Mäki calls "ontological plausibility" — the requirement that the building blocks of economic theory correspond, at least approximately, to the actual mechanisms that generate economic phenomena.

In his 1914 article, "Human Behavior and Economics," Mitchell opens with a diagnosis that sets the terms for much of his later work. He observes that economic theory had been built upon psychological foundations that were already being repudiated by the leading psychologists of the day:

> A slight but significant change seems to be taking place in the attitude of economic theorists toward psychology. Most of the older writers made no overt reference to psychology, but tacitly imputed to the men whose behavior they were analyzing certain traits consistent with common sense and convenient as a basis for theorizing. By recent writers, on the contrary, non-intercourse with psychology, long practiced in silence, is explicitly proclaimed to be the proper policy (Mitchell, 1914, p. 1).

Economic theory, as Mitchell saw it, had been built on a naïve psychology — hedonism — and, when the inadequacy of this psychology became apparent, the discipline, instead of seeking better psychological foundations, declared that psychology was simply irrelevant to economic analysis. The tendency to treat consumer preferences as given data, which Mitchell associates with Pareto, Cassel, and others, was a retreat to a "more superficial level" of analysis (Mitchell, 1944, p. 215).

This retreat is significant from the standpoint of the fact/value dichotomy because, as Nancy Cartwright (1999, pp. 1–2) has argued more generally, the idealization of human agents in economic models is not epistemically innocent: it involves choices about which features of reality to represent and which to suppress — and these choices are shaped by theoretical commitments and, ultimately, by values.[6]

In his 1944 lecture, Mitchell traces this retreat historically, beginning with Marshall's system and the criticisms it provoked:

> Scatteringly before and cumulatively after Marshall presented this imposing edifice to the public, critics began questioning the solidity of its cornerstone. Darwin's emphasis upon instincts, the emphasis of William James and other psychologists upon habits,

[6]In the case of exogenous preferences, neoclassical theory chooses to represent agents as atoms isolated from social relations in their choices—methodological individualism, beyond being an epistemic resource, is a value judgment—the superiority of individual freedom—as Knight (1935, sec. III) well attested in his reply to Ayres.

> undermined confidence in the older conception of conduct as guided by calculation. The reliability of introspection was discounted as experimental methods spread. Thorstein Veblen made sport of the hedonistic psychology which was explicitly used by Jevons in his analysis of utility, and which was implicit in Marshall's tamer phrasing (Mitchell, 1944, pp. 213–214).

The reference to Veblen signals an intellectual debt. Veblen (1898) argued that economics could not become a genuinely scientific discipline as long as it remained bound to a "pre-Darwinian" conception of human nature — one that treated the economic agent as a "lightning calculator of pleasures and pains."

Mitchell shared Veblen's critique but sought to move beyond it by developing a substantive research program, rather than contenting himself with what he regarded as Veblen's brilliant but inconclusive theoretical performances (Rutherford, 1987, p. 66). Mary Morgan (2012, pp. 235–236) characterizes this movement from critique to construction as one of the defining features of Mitchell's career. On her reading, Mitchell's empirical work was not an atheoretical accumulation of data, but a deliberate attempt to provide the empirical foundations for a theory of economic behavior that the Veblenian critique had shown to be necessary.

Mitchell (1944) identifies three responses that economists adopted upon encountering the collapse of their psychological foundations: some sought better theories of value, others, such as Dickinson, attempted to defend the old position, and a third group — the one that prevailed — simply declared that price theory did not depend on any theory of value. This last strategy, Mitchell argues, carries a significant cost. The theorist who works with supposed facts, postulated rather than empirically determined valuations, operates in a field of speculation where almost any conclusion can be demonstrated:

> Convenient as is this modern fashion, it has serious disadvantages. First and most obvious, it means that economic theory moves on a more superficial level than in its more confident days. The theorist works with data he does not profess to explain (Mitchell, 1944, p. 215).

The same diagnosis had already appeared in his 1940 address on "Feeling and Thinking in Scientific Work," where Mitchell directly criticizes the assumption of rationality:

> The easiest way to develop economic theory is to assume that men are thoroughly rational creatures, intent upon maximizing their wealth, and aware of their opportunities to do so. […] The whole body of propositions can be made as rigorously logical as Euclidean geometry, though some of the problems are indeterminate within fairly wide limits. The conclusions are true in the sense that they follow from the postulates, and an investigator content with this kind of truth should be satisfied. But the empirical validity of the propositions depends upon the representative value of the psychological postulate (Mitchell, 1940, pp. 229–230).

For Mitchell, a theory built on unrealistic foundations generates conclusions that "can not be refuted by an appeal to actual experience" (Mitchell, 1944, p. 217). And conclusions that cannot be refuted also cannot be confirmed. This leaves the economist free to construct elegant deductive systems, but unable to distinguish between valid knowledge and wishful thinking. In contemporary terms, Mitchell is identifying what Julian Reiss (2013, pp. 96–97) calls the

problem of "evidential underdetermination" in economics — that is, when theories are shielded from empirical testing by the very nature of their assumptions, the theorist's values and preferences become the de facto arbiters of theoretical choice.

The 1914 article makes Mitchell's programmatic aspiration explicit. After surveying the works of Parmelee, Thorndike, Wallas, Veblen, Sombart, Lippmann, and Walling, he declares:

> It was because hedonism offered a theory of how men act that it exercised so potent an influence upon economics. It is because they are developing a sounder type of functional psychology that we may hope both to profit by and to share in the work of contemporary psychologists. But in embracing this opportunity economics will assume a new character. It will cease to be a system of pecuniary logic, a mechanical study of static equilibria under non-existent conditions, and become a science of human behavior (Mitchell, 1914, p. 47).

The expression "pecuniary logic" encapsulates Mitchell's view that the orthodox economics of his day was not truly empirical but rather a formal exercise. For Mitchell, such theory possessed a logic of prices disconnected from the actual processes by which human beings make economic decisions. The aspiration to transform economics into a "science of human behavior" is therefore both a methodological and an axiological commitment — that is, it implies that the purpose of economic science is to understand real people in real situations, not to elaborate the implications of fictitious postulates. This aspiration resonates with Dewey's (1922) analysis of human conduct as fundamentally shaped by habit and social environment rather than by rational calculation — an analysis that Mitchell, in a letter to J. M. Clark, claims to have absorbed during his formative period in Chicago.[7]

4. Facts and Values in Mitchell

Mitchell's most sustained and explicit treatment of the fact/value dichotomy appears in his 1944 lecture "Facts and Values in Economics," delivered at a symposium on "Value in a World of Facts" at the New School for Social Research. The lecture is structured around three themes: the history of value theory in economics, the problem of wishful thinking, and the relationship between economics and other sciences. Each of these themes contributes to an integrated view of how values enter scientific practice and how the dangers they pose can be mitigated.

4.1 The Non-Existence of "Pure" Economic Facts

In the final section of his 1944 lecture, Mitchell states:

> There are no simon-pure economic valuations, and there are no simon-pure economic facts. That condition of affairs has long been recognized formally by men of my craft; but we have only vague notions about the non-economic components in behavior and can gain clear ideas about them best from men of other specialties (Mitchell, 1944, p. 219).

That is to say, Mitchell denies that facts can be purely economic, detached from the social, psychological, political, and cultural contexts in which they are embedded. The economic

[7]Letter from W. C. Mitchell to J. M. Clark, August 9, 1928; reproduced in Clark (1931), pp. 675–680, esp. 676–677.

agent, as Mitchell insists, "has a neuro-muscular system, habits, emotions, an unconscious, a culture, a family, a political orientation, a *Weltanschauung*, ancestors, and duties" (Mitchell, 1944, p. 219). This holistic view of properly economic matters implies that the facts economists study are always already saturated with the values, institutions, and meanings of the societies that produce them.

In this regard, Mitchell's position anticipates Putnam's (2002, p. 28) argument that the very vocabulary in which economic "facts" are described (terms such as "unemployment," "inflation," "welfare") is already value-laden, embedding normative judgments about what counts as a problem and what constitutes an adequate description of social reality. It also resonates with Longino's (1990, pp. 62–63) concept of "background assumptions" — the set of often unexamined beliefs that connect observational data to theoretical conclusions and that are shaped by social, cultural, and normative commitments.

If there are no pure economic facts, then the separation between positive and normative analysis — the cornerstone of the Robbins-Friedman tradition — cannot be maintained in the straightforward manner its proponents assume. Every selection of data, every definition of a variable, every choice of a research question involves implicit normative judgments. Robbins (1932, p. 148) argued that economics, "as a science," deals only with "ascertainable facts" and that "questions of 'ought'" lie beyond its jurisdiction. Mitchell's denial of the existence of "simon-pure" economic facts strikes at the foundation of this demarcation: if economic facts are always already permeated by evaluative elements, then the Robbinsian distinction becomes a (arguably misguided) regulative ideal rather than a description of actual scientific practice.

4.2 The Influence of the Researcher's Values

Mitchell was deeply aware that the investigator's values shape economic knowledge in ways that are often invisible to the investigator himself. His analysis of this problem draws on the concept of "personal equation," a term borrowed from observational astronomy to describe the systematic biases that individual observers introduce into their measurements:

> In all scientific research, an investigator's personal equation influences his results. The role played by this factor varies inversely as the impersonal controls to which results can be subjected. The natural sciences have a great advantage over the social sciences in this respect. When reasoning is checked by experiments that can be repeated at will, the findings of different observers can be compared, and a high degree of probability attached to the conclusions drawn from their joint efforts (Mitchell, 1944, p. 216).

The social sciences, Mitchell argues, suffer from a double disadvantage. First, the opportunities for experimental testing were, in his time, severely limited, meaning that the investigator's personal equation can exert a far greater influence on results. Second, and more importantly, economists deal with questions "about which all men, including themselves, feel intensely" (Mitchell, 1944, p. 217).

The combination of limited experimental controls and intense personal involvement creates conditions in which self-deception is not merely possible but nearly inevitable. This diagnosis is strikingly parallel to Douglas's (2009, pp. 74–82) analysis of the "inductive risk" involved in scientific judgments: when evidence is uncertain and the consequences of error are

significant, non-epistemic values inevitably influence the standards by which scientists accept evidence. In economics, where evidence is almost always uncertain and the policy consequences of theoretical conclusions are far-reaching, the scope for such value influence is correspondingly greater.

Mitchell's analysis of the sources of bias is telling:

> I like to divide the blame for economists' self-deceptions between the unconscious hidden within each of us, and the semi-conscious that is developed by the conditions under which our minds are formed. Every economist can see in certain of his fellows the unhappy effects of a personal or a class bias. Of course it is harder to see these biases in ourselves — though we realize they must be present in our make-up, and at times we admit the justice of Simkhovitch's caustic remark that "our theories are fig leaves worn to conceal our passions" (Mitchell, 1944, p. 218).

The same theme had already appeared in Mitchell's 1940 address, where he applied this self-reflexive analysis to the economist's own professional practice:

> An economist who recognizes the emotional element in the behavior of other people ought to apply this observation to himself. Not only in his own efforts to make a living, but also in his allegedly scientific thinking, he is a creature of likes and dislikes. It is rather easy for him to sense temperamental biases in the thinking of other economists. He may see, for example, that one colleague enjoys playing up every defect he can find in the workings of capitalism, while another wishes to minimize these unfortunate incidents. What one economist thinks about others, others think about him (Mitchell, 1940, p. 230).

Mitchell developed this self-critique more broadly in his 1939 address, where he analyzed the structural difficulties scientists face when seeking to influence public opinion. He observes that scientists, precisely by virtue of their partial emancipation from the prevailing prejudices of their social groups, become creatures whose judgment most people mistrust outside of their specialties; that both the temperament that inclined them to research and the habits they form in research tend to make them awkward and reluctant in appealing to the emotions that are so potent in influencing men (Mitchell, 1939, p. 605). This analysis reinforces the thesis that, for Mitchell, objectivity is not a natural property of the scientist but a fragile and partial construction that coexists with communicative limitations and biases of professional socialization.

This passage shows that Mitchell's awareness of the value-ladenness of economic research is not confined to abstract methodological reflection. It is rooted in his observation of the actual practice of economists: how they choose their problems, how they interpret evidence, and how they present their conclusions. This attention to the actual practice of research, rather than to idealized norms of scientific conduct, anticipates the approach that Małecka (2025) and others have recently advocated. It also resonates with Mäki's (2002, p. 5) insistence that the methodology of economics should be, first and foremost, a descriptive enterprise — an investigation of what economists actually do — rather than a prescriptive one that imposes external standards on scientific practice.

Mitchell's analysis of researcher bias finds a more precise philosophical development in Douglas's (2009, ch. 4) argument regarding the moral responsibilities of scientists. Douglas argues that scientists bear the same moral responsibility as any citizen to consider the potential consequences of error in their work, and that this responsibility can be neither delegated nor eliminated through a putative "moral exemption" from scientific activity (Douglas, 2009, pp. 74–82). This insistence on the consequences of error as a moral — rather than merely technical — question resonates directly with Mitchell's concern with "wishful thinking": for both, the danger lies not in the scientist having values, but in the scientist's failing to take responsibility for the consequences of allowing those values to distort the analysis without awareness.

4.3 Wishful Thinking in the Social Sciences

Mitchell develops the theme of "wishful thinking" as the central danger that values pose to scientific objectivity. In his 1944 lecture, he distinguishes between "conscious and virtuous wishful thinking" — the explicit expression of hopes and ideals — and "vicious wishful thinking" — the unconscious substitution of desires for conclusions (Mitchell, 1944, p. 216). The problem is not that economists have values, but rather that their values can distort their analysis without their being aware of it. This distinction between explicit, self-aware value commitments and implicit, uncontrolled biases is remarkably close to Weber's (1904/1949, pp. 53–54) insistence that the scholar must be transparent about the value presuppositions guiding his investigation, and to Myrdal's (1953, p. vii) assertion that hidden valuations are far more dangerous than openly declared ones.

The structural conditions of the social sciences make them particularly vulnerable to this danger. Unlike natural scientists, Mitchell argues, economists typically cannot test their hypotheses experimentally. This means that speculative conclusions can be neither easily refuted nor confirmed:

> This situation gives social scientists as wide a license as any poet could ask. Since their conclusions are seldom subjected to trial by fact, their personal equations can run riot with a freedom unknown in the experimental sciences. Economists have the reputation of differing endlessly from one another, and sometimes exult perversely in their differences. By setting up assumptions chosen for the purpose, they can demonstrate on the speculative plane almost any conclusion they wish (Mitchell, 1944, p. 217).

Mitchell identifies a specific mode of failure: the confusion between hypothesis and theory. Economists routinely treat speculative conclusions as established knowledge, offering policy advice with scientific authority when their conclusions have never been subjected to empirical verification:

> One of the besetting sins of economists is failure to recognize the distinction between an hypothesis and a theory. […] Many an economist is ready to speak on practical issues as one having the authority of science, although he has made no serious effort to verify his conclusions inductively (Mitchell, 1944, p. 217).

Mitchell is describing, a decade before Friedman's methodological essay, the tendency of economists to confuse logical consistency with empirical validity. His insistence on inductive verification as a necessary condition for treating hypotheses as established theory represents a

direct challenge to the methodological position that would later become dominant. Kevin Hoover (2001, p. 74), in his assessment of macroeconomic methodology, makes a similar point: the mathematical elegance of a model does not guarantee its empirical adequacy, and the history of macroeconomics is replete with logically impeccable but empirically empty models. Mitchell's critique of "wishful thinking" can thus be read as an early warning against the methodological tendency — later systematized under the banner of "as if" reasoning — to treat theoretical convenience as a substitute for empirical justification.

5. Mitchell's Methodological Solution

The most original aspect of Mitchell's treatment of the fact/value dichotomy is his proposed resolution of the apparent paradox between retaining non-epistemic values and preserving scientific objectivity. Rather than choosing between value-freedom and value-commitment, Mitchell argues that both values and empirical discipline are necessary components of good science. His solution operates on two levels: values define the ends of scientific investigation, while the empirical method provides the means of controlling the distortions that values may introduce.

5.1 Values as Ends of Investigation

Mitchell categorically rejects the idea that the scientist should attempt to divest himself of his values. In the final paragraph of his 1944 lecture, he writes:

> Nothing I have said about the dangers of wishful thinking in the social sciences implies that we should, or could, divest ourselves of aims, ideals, or social aspirations. On the contrary, we should endeavor to make these notions as intelligent and as noble as possible. The more we learn about other aspects of behavior than those which concern us professionally, the better we are likely to succeed in forming serviceable ideals (Mitchell, 1944, p. 219).

This passage establishes that, for Mitchell, values are not contaminants to be eliminated from science but constitutive elements that define its purpose and direction. In this sense, the theoretical economist who studies unemployment, business cycles, or income distribution does so because he cares about human welfare — and this care is not a defect but a motivation that should be cultivated. Mitchell's injunction (1944, p. 219) is not to suppress values but to refine them: to make them "as intelligent and as noble as possible." This position is remarkably consonant with Douglas's (2009, p. 87) argument that the legitimate role of non-epistemic values in science is not to determine the content of scientific conclusions but to shape the direction of investigation and inform the standards of evidence that scientists adopt. It also echoes Dewey's (1939, pp. 40–44) insistence that values are not arbitrary preferences but intelligent responses to problematic situations, susceptible to rational criticism and refinement through experience — a view that directly guided Mitchell's pragmatist methodology.

Mitchell expresses the ultimate purpose of economics in explicitly evaluative terms:

> Our ultimate aim, if I may express a final value judgment, should be to contribute all that in us lies toward realizing in practice the highest social values we are able to conceive (Mitchell, 1944, p. 219).

Coming from an economist renowned for his empiricism, this statement is remarkable. Mitchell does not hesitate to express a value judgment about the purpose of economics itself. Science exists to serve human welfare, and the economist who denies this is either deceiving himself or impoverishing his discipline. Moreover, this explicitly evaluative conception of the purpose of economics aligns Mitchell with what Reiss and Sprenger (2020, p. 4) identify as the "legitimate" or "wish list" role of values in science: the role of guiding the selection and pursuit of research problems, in contrast with the "illegitimate" role of determining the content of scientific conclusions.

The same conviction appears in his 1940 address, where Mitchell discusses the role of social sentiment in the choice of research problems:

> Scientific men have a moral duty toward society, and that feeling should, I think, be given weight in deciding what problems they shall try to solve. When it comes to deciding how they shall seek the solution, they had best rely as strictly as they can upon thinking (Mitchell, 1940, p. 232).

Douglas (2009, pp. 122–124) draws a crucial distinction that illuminates Mitchell's position. She distinguishes between "detached objectivity" — the legitimate prohibition against values supplanting evidence — and "value-free objectivity" — the illegitimate prohibition of all value influence on scientific reasoning. The former is essential to the integrity of science; the latter is a vestige of the positivist ideal that Douglas rejects. Mitchell occupies precisely this intermediate position: he demands detachment (protection against wishful thinking) without demanding the absence of values. Indeed, the practical effect of masking the role of values is, as Douglas (2009, p. 123) argues, to render them "unexaminable by others" — precisely the danger that Mitchell identifies in unconscious bias, as distinct from the conscious and virtuous commitment to social ideals. In his 1939 address, Mitchell articulates this connection in even broader terms, linking it directly to the defense of democracy. He argues that science cannot flourish unless freedom of thought prevails, and that democracy and the scientific way of thinking grew up together, each nourishing the other: if one fails, the other will falter (Mitchell, 1939, p. 607). Citing John Dewey, Mitchell affirms that the future of democracy is allied with the spread of the scientific attitude (Mitchell, 1939, p. 607). For Mitchell, the values that guide scientific investigation are not contingent individual preferences but structural commitments to institutional conditions — freedom of thought, disinterested criticism, democracy — without which the very possibility of science vanishes.

5.2 The Empirical Method as Safeguard

If values define the ends of scientific investigation, the empirical method serves as the mechanism for preventing those values from distorting the process of inquiry. Mitchell's key formulation of this idea appears near the end of his 1944 lecture: "And the more clearly we think about what we should like to see happen, the less likely we are to confuse these conscious wishes with our conclusions about what is" (Mitchell, 1944, p. 219).

This sentence is the cornerstone of Mitchell's position on the fact/value dichotomy. Far from undermining objectivity, explicit reflection on one's own values actually enhances it. The scientist who is clear about what he hopes for is better equipped to recognize "wishful thinking"

in his findings than the scientist who suppresses his values and allows them to operate unconsciously. In other words, the danger lies not in being guided by values but in being unaware of them. This insight — that transparency about one's own commitments is a precondition for, not an obstacle to, objectivity — was developed by Myrdal (1953, p. vii) — inspired by original institutionalism (see Berg, 2023, ch. 3) — in his critique of the hidden "political" element in economic theory, and was elaborated with greater philosophical precision by Longino (1990, p. 76), who argues that the transparency of value assumptions is a necessary condition for the critical scrutiny that constitutes scientific objectivity.

The practical counterpart to this awareness is the empirical discipline that Mitchell advocated throughout his career. In his 1944 lecture, he prescribes a specific method for guarding against wishful thinking:

> The best way to guard against the danger of doing wishful thinking in economics is to frame assumptions in such fashion that either they, or the conclusions deduced from them, can be tested by appeal to factual records. This type of work requires careful advance planning of every investigation, knowledge of the observations that have been or can be made, and familiarity with the technique of interpreting such evidence — especially the methods of statistical inference (Mitchell, 1944, p. 218).

Mitchell is not merely advocating empiricism as an epistemological preference. He proposes a specific institutional practice: the formulation of assumptions in testable form, the systematic planning of investigations, and the use of statistical methods as mechanisms of control over subjective bias. Objectivity, in Mitchell's conception, is not a property of the individual scientist's mind but a characteristic of the procedures that the scientific community employs. It is, in short, procedural objectivity. This conception finds a remarkably close parallel in Longino's (2002, pp. 129–135) account of objectivity as a characteristic of scientific communities organized around four social norms: recognized avenues for criticism, shared standards of evaluation, community responsiveness to criticism, and equality of intellectual authority. Mitchell's emphasis on empirical testing, statistical controls, and intersubjective verification can be interpreted as an anticipation of Longino's first three norms, while his insistence on interdisciplinary collaboration (discussed below) extends the critical community beyond the boundaries of any single discipline.

Douglas's (2009, ch. 6) framework of objectivity as a multidimensional concept provides a taxonomy that allows us to situate Mitchell's contribution with greater precision. Douglas identifies seven normative bases for objectivity: manipulable, convergent, detached, value-neutral, procedural, concordant, and interactive (pp. 118–129). Mitchell's conception maps onto at least three of these: detached objectivity (his protection against wishful thinking), concordant objectivity (his emphasis on intersubjective verification among economists), and interactive objectivity (his insistence on interdisciplinary collaboration as a means of "cross-fertilization of minds"). Notably, Douglas argues that objectivity is always a matter of degree, not a binary property (p. 117), and that no single basis can guarantee it — it is the synergy among multiple bases that strengthens confidence in results (pp. 129–132). This gradualist conception corresponds closely to Mitchell's fallibilist position, which acknowledges, from "painful examples," that the empirical safeguard is "far from perfect" but insists it is the best strategy available. This procedural vision of objectivity finds a concrete and earlier

instantiation in the distinction Mitchell proposed in 1919, in his presidential addresses to the American Statistical Association, between "record statistics" and "planning statistics." The former are a record of what has happened; the latter are statistics oriented toward future planning, which must be strictly up to date, presented concisely, and simple enough to be compiled overnight (Mitchell, 1919a, p. 235; 1919b, p. 43). This distinction reveals, with practical clarity, how Mitchell conceived the relationship between values and method: values determine the purpose for which statistics are collected (social planning), while method determines how they are collected (with rigor and intersubjective control). Moreover, Mitchell explicitly contrasts the radical progressivism of science and industry — where the good is discarded for the better even without external pressure — with the conservatism that characterizes social organization, where only catastrophes force change (Mitchell, 1919a, p. 228). This comparison reinforces the conviction that, for Mitchell, extending quantitative methods to social organization is not an epistemologically neutral choice but a value commitment to cumulative progress.

Mitchell also treats intersubjectivity as a crucial component of scientific objectivity. If economists adopt the realistic method he advocates, they will be able to check one another's work, and the differences among their conclusions will progressively narrow:

> If my hopes and expectations are realized, economists will be able to check one another's work more effectively in the future, and the differences among their conclusions will shrink to a narrower range. I know from painful examples that this safeguard against wishful thinking is far from perfect; but as we subject ourselves to the hard discipline of dealing with actual experience we shall grow in ability to amend or confirm one another's findings (Mitchell, 1944, p. 218).

This fallibilist stance — the recognition that objectivity is an ideal to be pursued but never fully achieved — is characteristic of the pragmatist epistemology that guided Mitchell's thinking, and sharply distinguishes his position from both the positivist assertion that value-free science is attainable and the postmodern denial that objectivity has any meaningful content.

Douglas (2009, pp. 155–156) reinforces this position by arguing that, in contexts of scientific advising for public policy, scientists should make explicit the values they employ in their judgments: "scientists need to make explicit the values they use to make these judgments, whether in their own work or when drawing on the work of others." The requirement of transparency is not, for Douglas, a concession to subjectivity but a necessary condition for the democratic accountability of scientific knowledge. This position finds a direct parallel in Mitchell's formulation (1944, p. 219), according to which the more clearly we think about what we would like to see happen, the less likely we are to confuse those wishes with our conclusions. For both authors, transparency about one's own value commitments is a precondition for, not an obstacle to, scientific objectivity.

5.3 The Role of Interdisciplinary Collaboration

A final element of Mitchell's methodological solution deserves attention: his insistence on interdisciplinary collaboration as a means of broadening the economist's perspective and reducing the distortions introduced by disciplinary narrowness. Since there are no "simon-pure

economic facts," the economist who works in isolation inevitably produces an impoverished understanding of economic phenomena. Mitchell argues that engagement with other sciences is essential:

> When economists thus deal with problems of practice they gain a vivid realization of their need of help from other sciences, and of the contributions they can make in return. In my experience, social scientists do not achieve a real meeting of minds by coming together and talking at one another about scientific methods or any other topic of common interest. But when several of them begin working together upon a genuine problem of human behavior, each member of the group finds he has much to learn and something to teach. Cross-fertilization of minds is best effected "on the job" (Mitchell, 1944, p. 219).

This pragmatic vision of interdisciplinarity serves a dual function in Mitchell's view. It enriches the economist's understanding of the phenomena under study and exposes the economist's values and assumptions to the scrutiny of scholars trained in different traditions. The intersubjectivity that Mitchell considers essential to objectivity is thus extended beyond the boundaries of the economics profession. In Longino's (2002, pp. 131–132) terms, interdisciplinary collaboration multiplies the avenues for "transformative criticism" — critiques that can challenge not only the internal logic of a theory but also the deeper assumptions that shape the questions it asks and the evidence it considers relevant.[8]

6. Concluding Remarks

The textual analysis presented in this article shows that Wesley C. Mitchell developed a remarkably sophisticated account of the relationship between facts and values in economics. This account, elaborated over several decades of writing and teaching, can be summarized in three propositions.

First, Mitchell recognized that values are constitutive elements of scientific investigation, not spurious accretions to be eliminated. Values define the problems economists choose to study, guide the purposes of their research, and shape the conceptual frameworks through which they interpret evidence. The aspiration to a value-free economics is, in Mitchell's view, both illusory and impoverished. The economist who denies his values does not become objective; he merely becomes less aware of the forces shaping his analysis. This position, as we have shown, anticipates central claims in the contemporary philosophy of science — from Putnam's (2002) critique of the fact/value dichotomy to Douglas's (2009) analysis of the legitimate roles of values in scientific work.

Second, Mitchell insisted that the recognition of values does not entail the collapse of science into ideology. The crucial distinction is between values as ends and the empirical method as a means of controlling the distortions that values may introduce. The more clearly the economist articulates his values, the less likely he is to confuse his wishes with his findings. And the more rigorously he subjects his assumptions and conclusions to empirical testing, the more

[8]The insistence on practice, or on concrete research problems, reflects Mitchell's pragmatist influence as the best means of approaching methodological questions. On the "practical turn," see Boumans (2025) and the references cited therein.

effectively he can prevent his personal equation from corrupting his results. This insistence on transparency and empirical discipline as safeguards against bias places Mitchell in a tradition running from Dewey (1938) and Myrdal (1953) to the contemporary work of Putnam (2002), Longino (2002), and Douglas (2009); see also Walsh (2009), as well as Badiei and Grivaux (2022) and Davis (2024), part III.

Third, Mitchell proposed a conception of objectivity that is procedural rather than substantive. Objectivity does not consist in the absence of values from the scientist's mind. It consists in the use of methods — statistical analysis, systematic observation, intersubjective verification, and interdisciplinary collaboration — that allow the scientific community to check and correct individual biases. This is a social conception of objectivity, grounded in the practices and institutions of research rather than in the psychological state of the individual researcher. It is, as we have argued, strikingly convergent with Longino's (1990, 2002) social epistemology, which similarly locates objectivity in the critical practices of scientific communities rather than in the epistemic virtues of individual researchers.

Mitchell's contribution to the fact/value dichotomy in economics deserves to be better known in the contemporary methodological literature. His position differs from the dominant Robbins-Friedman tradition, which denies any role for value judgments in economics; it differs as well from the more nuanced positions of Schumpeter, Blaug, or even Weber, who, while admitting the presence of values, regard them as something to be, at least ideally, isolated; and, while acknowledging the presence of non-epistemic values, Mitchell's account maintains at the same time a robust notion of scientific objectivity. Mitchell occupies a middle ground that is, paradoxically, more realistic and more demanding than either extreme: he asks the economist to be honest about his values and rigorous in his methods, combining moral commitment with epistemic discipline. As Małecka (2025a) has argued, understanding the production of economic knowledge requires analyzing the actual practice of research and the kind of knowledge it produces. Mitchell was engaged in precisely this kind of reflection.

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