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Kuhn's Evolutionary Social Epistemology.
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With the publication in 2012 of the fiftieth anniversary of Thomas Kuhn's *Structure of Scientific Revolutions* (1962) (SSR), one of the most cited books of all time, Wray's *Kuhn's Evolutionary Social Epistemology* (KESE) aims to defend Kuhn's later views on the epistemology of science and to clarify Kuhn's relationship with the sociology of science and science studies.

SSR avers that scientific revolutions are paradigm changes, which are incommensurable with other paradigms. Popper's criticism of SSR's notion of science is that it has lost its critical role. Other criticisms of SSR are that Kuhn's notion of science is either irrational or is guided by mob psychology. KESE notes that Kuhn's later view, which are based on essays collected in *Road Since Structure: Philosophical Essays 1970-1993* (2000) (RSS), tries to address criticisms of SSR through a linguistic turn to taxonomic lexicons, a revised notion of incommensurability, and the development of an epistemology of science and a social epistemology.

Of interest to the generalist is KESE's account of Kuhn's later view in terms of taxonomic lexicons, incommensurability, and epistemology of science and its relationship to sociology of science and science studies. Of interest to the specialist is KESE's account of Kuhn on the success of science in terms of specialization of science as both social and epistemic.

KESE has three parts with twelve chapters. Part One discusses revolutions, paradigms and incommensurability. On revolutions and paradigms, KESE notes that Kuhn's view developed from scientific revolutions as paradigm changes in SSR to changes in taxonomies or lexicons in theories. KESE examines Kuhn's *Copernican Revolution* (1957) to illustrate Kuhn's developed account of scientific change. On incommensurability, KESE discusses the epistemic significance of meaning incommensurability, topic incommensurability, and disassociation. For many of Kuhn's commentators, meaning incommensurability is problematic because it challenges the standards of science. KESE argues that meaning incommensurability is not a problem of shared standards, but it is a problem of difficulty to resolve disagreements. For KESE, meaning incommensurability has less epistemic significance than either topic incommensurability or disassociation.

Part Two explains Kuhn's evolutionary epistemology. KESE avers that in Kuhn's evolutionary epistemology the success of science is not explained by science getting closer to the truth but by increased specialization in science where scientists are focused on a smaller range of phenomena. KESE compares Kuhn's account of science specialization to sociological and historical accounts. Though those accounts provide explanations of the social dimension of change, Kuhn's account provides explanations of both social and cognitive or epistemic dimensions of change. KESE also discusses Kuhn's arguments against the Strong Programme.

Part Three addresses Kuhn's social epistemology. On the relationship between Kuhn and constructionism, KESE argues that Kuhn is an internalist: changes to scientific theories are based

on epistemic factors and not on external factors. KESE notes that Kuhn is not a radical nominalist, since mind-independent reality provides constraints on some hypotheses. KESE avers that Kuhn's social epistemology is based the research community, which is the locus of scientific change. Philosophers of science should use the social sciences to understand the type of social change involved in theory change. KESE discusses empirical research on older scientists who create new theories and are not resistant to theory change.

My criticisms are as follows: though KESE notes that it is unfortunate that philosophers of science have not taken seriously Kuhn's later work since they hold on to Kuhn's SSR view, KESE does not mention that Kuhn's later work is incomplete. RSS is a collection of Kuhn's later papers. KESE does not even mention Kuhn's posthumous unpublished manuscript, *The Plurality of Worlds: An Evolutionary Theory of Scientific Development*, which is being edited by James Conant and John Haugeland.

Although KESE aims to get philosophers of science to use the work of sociology of science and science studies, KESE falls short in its discussion of sociology of science and science studies since KESE does not specify which sociology of science or science studies philosophers of science should consult. Sociology of science and science studies are very broad and diverse fields.

KESE maintains that Kuhn proposes that philosophers work with sociologists of science to develop an epistemology of science as an extension to Quine's naturalistic epistemology (180). This is contrary to Kuhn's turn to Kant. KESE does not discuss Kant other than page 169's footnote, which states that Kuhn rejected earlier strands of his earlier career. Hence, KESE does not take into account the Kantian aspect of Kuhn's post-Darwinian linguistic turn to taxonomic lexicons. Kuhn's view is that a taxonomic lexicon and its structure function very much like Kantian categories of the mind, though they vary historically. This leads Kuhn to introduce the notion of synthetic a priori truths. The advantage of notions such as the lexicon and synthetic a priori truths is that they help to refine and unify Kuhn's notions of incommensurability and scientific revolution. KESE does not explain how the notions of the lexicon and synthetic a priori truths fit with KESE's discussion of the epistemic aspects of specialization of science.

Is KESE's explanation of success of science, which is the result of increased specialization of science, an empirical argument or a philosophical argument? For SSR, increased puzzle solving is an indicator for the success of science. Did Kuhn change his view to increased specialization of science? KESE agrees with Barnes and Fuller that Kuhn is an apologist for science because Kuhn does not want to challenge the "epistemic authority of science" (93). KESE goes further to argue that scientists should be in charge on their own research agendas. If this view is correct, then it leads to ethical problems in medical sciences and in biotechnology. If scientists should be in charge of their own research agendas, should scientists be allowed to clone other humans? There are many ethical issues arising from medical sciences and biotechnology that require input from medical ethicists, lawyers, and the public.

Though KESE notes that for Kuhn, scientific knowledge is produced by scientific research groups, which is an aggregation of similarly trained scientists, KESE does not discuss

how scientific knowledge is either justified or certified. Bird (in “Social Knowing”, *Philosophical Perspectives*, 2010) avers that a discovery becomes scientific knowledge through material ways of coordination and publication in a scientific journal.

As regards Kuhn’s social epistemology, though KESE notes that the training of scientists is critical in normal science, KESE does not discuss the role that tacit knowledge plays in the training of scientists. Tacit knowledge and background assumptions are very important to Kuhn in terms of puzzle solving and applying high-level theories to particular cases. Though KESE refers to social epistemologists such as Goldman, Kitcher, and Longino, KESE does not compare Kuhn’s social epistemology with their social epistemologies.

In KESE’s discussion of Kuhn’s instrumental rationality, KESE does not refer to Kuhn’s practical rationality. Though Rouse’s (“Kuhn’s Philosophy of Scientific Practice”, 2003), Bernstein’s (*Beyond Objectivism and Relativism*, 1983) and Irzik’s (“Changing Conception of Rationality”, 2003) have different views on Kuhn, what is common to all three of them is that Rouse, Bernstein and Irzik interpret Kuhn’s scientific rationality to be more than rule-following; it also involves practical reasoning of judgment and deliberation. This is an important issue because if Kuhn’s notion of scientific rationality is a form of practical reasoning, then scientists can make judgments on a case by case basis within a paradigm instead of following rules.

In conclusion, Kuhn wrote in his response in *Criticism and the Growth of Knowledge* (CGK) (1970) that there are two Kuhns. The first Kuhn is the author of SSR and of his response to criticisms in CGK. The second Kuhn, who has different concerns from the first Kuhn, is the one whom Popper, Lakatos, Feyerabend, Toulmin and Watkins criticize. KESE argues that there is yet a third Kuhn, who has different concerns from the first and second Kuhns. My criticisms above can be construed as questioning whether KESE has demonstrated sufficiently that there is a third Kuhn. Maybe with the publication of *The Plurality of Worlds: An Evolutionary Theory of Scientific Development* the real Kuhn will stand up.

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