

## Dealing with plagiarism allegations: a case history

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**Summary.** In 2013, I was selected as a reviewer of a paper submitted by Prof. Thomas Weber (Ecole Polytechnique de Lausanne) to the *Journal of Mathematical Economics* (JME). To my surprise, I quickly realized that he used ideas, conceived and developed in many of my preceding published papers, without proper acknowledgment, and misrepresented my rate-of-return approach, one which was presented in a 2010 article that was awarded best article of the year in 2011 by the American Society for Engineering Education. As a reviewer of Weber's paper, I felt compelled to note Weber's inappropriate behavior in my report, but the then editor-in-chief, Prof. Felix Kübler, seemingly disregarded my comments and accepted Weber's paper for publication. Months later, I submitted a paper to the *European Journal of Operational Research* and, soon thereafter, a co-editor-in-chief, Prof. Immanuel Bomze, accused me of plagiarising Weber's paper, on the basis of Weber's review of my submitted manuscript. I quickly replied with detailed rebuttal notes and contacted the journal's coordinating editor, Prof. Roman Slowinski. As a consequence, the allegation was ultimately withdrawn. Later, I turned to Prof. Atsushi Kajii, editor of the JME, who invited me to supply a new paper for the JME correcting the "errors and omissions" in Weber's paper. This note recounts the events that made up this intricate affair.

1. In March 2013, I was invited by the *Journal of Mathematical Economics* (JME) to review a paper by Prof. Thomas Weber, entitled "On the (non)-equivalence of IRR and NPV". The paper deals with rates of return and investment decisions, topics on which Weber had not previously published. I assumed that the paper was sent to me because I had written a multitude of relevant papers on such subjects, published in respected, refereed international journals.<sup>1</sup>
2. In my first review, I described the problems with Weber's approach, with the intent of helping him arrive at the correct economic interpretation. Among other things, a discussion of the literature was lacking and I couldn't help but note Weber's obliviousness to a body of literature and to standard concepts (such as "NPV consistency") to which he was applying his mathematical skills for the first time. I suggested to Weber a list of papers which he should read and then discuss in a

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<sup>1</sup> In the inviting letter, the Associate Editor, Prof. Karl Schmedders wrote "Given your expertise in this area, I would appreciate your comments on the above paper" (March 2, 2013).

separate section. I also specifically suggested that his paper should address the proposals of Hazen (2003), Hartman and Schafrick (2004) and Magni (2010), all well-known contributions in the relevant rate-of-return and capital budgeting literature.

3. To my dismay, in his revised version, Weber dismissed Hartman and Schafrick's (2004) contribution with a passing comment. On the basis of his own ad-hoc definition of "NPV consistency", clearly having the intent of validating his own approach, he resolutely claimed that Hazen (2003) and Magni (2010)<sup>2</sup> were NPV-inconsistent approaches, notwithstanding the common knowledge among scholars that all the three above mentioned models are indeed consistent with NPV.<sup>3</sup> Most notably, I indicated in explicit terms, in my subsequent report, that Weber had misrepresented my approach and tried to ascribe to himself some of previously published concepts.<sup>4</sup> Despite this, I generally took the high road and tried to limit myself to comments which advised the author to delete the inappropriate Appendix and suggested, literally, a "(very) major revision" for other serious flaws as well, feeling confident that the editor-in-chief would intervene to warn the author against academic misconduct. Later, the Associate Editor, Prof. Karl Schmedders, informed me that he had recommended "major revision" as well.
4. I did not receive a revised version or a rebuttal letter or indeed any communication until March 17th 2014, when I received a brief letter from the editor-in-chief, Prof. Felix Kübler, who informed me that the paper had been accepted, with no other explanation.
5. The same day I wrote to Schmedders for some more information, noting that I had never received any further revised version. Schmedders wrote that he also did not receive anything and praised my very detailed reports: "Many thanks for your great work on this paper. The author should be very grateful. And let's hope that the author indeed addressed all your points."<sup>5</sup> Contrary to our hopes, it was immediately clear

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<sup>2</sup> Hartman and Schafrick (2004) and Magni (2010) won, in 2005 and 2011, respectively, the "Eugene L. Grant" award, granted by the Engineering Economy Division of the American Society for Engineering Education.

<sup>3</sup> Indeed, Weber (2014) claimed that any proposal existing in the relevant literature is NPV-inconsistent: "All of them are inconsistent with the NPV rule" (Introduction). Only his solution is NPV-consistent: it is "the only ... known NPV-consistent return criterion" (Abstract).

<sup>4</sup> From my report: "The author should not ascribe to himself results that have been shown by other scholars"; "the twofold scenario of either fixing capital and deriving rates or, vice versa, fixing rates and deriving capital is obvious and explicitly accounted for in Magni (2010, p. 155-6)"; "The author misunderstands the AIRR approach"; "the (past and) current literature is trivialized and deemed "NPV-inconsistent" by means of an ad-hoc definition"; "he aims at getting rid of other recent approaches, which are made to violate the author's definition of NPV-consistency"; "if the author does not feel at ease with finding relations with that strand of literature, he may give only some hints on the contents of the papers, but, as I repeat, without downgrading and without trivializing the results obtained by his colleagues".

<sup>5</sup> Anyone interested in reading my reports of Weber's paper can contact me by email at [magni@unimo.it](mailto:magni@unimo.it).

from the pre-print version, already posted by Weber on SSRN (and later from the published version), that Weber did not delete the Appendix, nor had he corrected his misrepresentation of my approach, so I expressed to Schmedders my astonishment at Kübler's decision.

6. Schmedders was dismayed: "Oh no, that sounds much, much worse than my worst fears" and stated that he did not know the inner workings of JME adding "I am no longer an associate editor for them. This paper was the last one that I had to handle" and suggested that the journal wanted to dismiss him for some reason ("Perhaps the journal just wanted to get rid of me for good. I don't know.")
7. In the acknowledgments of Weber's published paper, the author thanks me (among others) for "helpful comments and suggestions". Since I had never had any contact with Weber it seems that someone at JME had disclosed my name or confirmed Weber's conjecture about the reviewer's identity. Most notably, Weber, by adding my name in his acknowledgments without noting my reservations, may have suggested to some readers that I endorsed, partially or totally, his approach and his representation of my AIRR approach.
8. Schmedders suggested the possibility of writing a rebuttal paper. I intended to write it, as soon as I would have some time, so as to shed lights on the flaws of the paper, on the misrepresentation of my approach and on his incorrect self-attribution of some ideas, notions and expressions of mine.
9. Weber's paper was published online on March 22<sup>th</sup> 2014 and in print in the issue of May 2014.
10. On December 2014, while I was preparing the rebuttal note, I submitted a paper to the *European Journal of Operational Research* (EJOR). (I was a long-time author and reviewer for EJOR). The paper's results were entirely grounded on my AIRR approach (Magni, 2010). The AIRR is based on the idea that an equivalence class of capital streams (i.e., depreciation schedules) is uniquely associated with an average rate of return. I had conceived and fully developed my AIRR approach in the previous five years in various directions (accounting, economics, finance, mathematics), for various applications (project finance, investment performance measurement, real estate investments, engineering projects), and with several co-authors as well. At the time of my submissions, I had written more than 20 papers on AIRR (and its variant AROI),

including 11 papers published in refereed international journals (3 of which are in top rated journals, according to the rating established by the Italian Ministry of University and Research).<sup>6</sup> My paper submitted to EJOR showed that my AIRR approach enables, by means of Chisini mean, a unified perspective of several different metrics that are considered not connected in the literature.

My submitted paper had nothing to do with the *selective IRR* endorsed by Weber (2014). Indeed, my AIRR approach refutes the IRR approach, so there cannot be any intersection between the two papers (except for the fact that, in his Appendix, Weber's paper misrepresented and denigrated my approach, did not acknowledge my authorship on some results of mine, and used standard notions such as "NPV consistency" seemingly suggesting that his was a new definition). Weber (2014) searched for a rate of return associated with a given cashflow stream, whereas I had already shown, some years earlier, that a project is associated with multiple rates of return, each of which is uniquely associated with a given equivalence class of capital streams. I do not search for uniqueness of rate; rather, I guarantee the existence of multiple *economically significant* rates of return, each supplying a different piece of information and suitable for different purposes. Therefore, Weber's paper and my paper submitted to EJOR are incompatible.

11. On June 18<sup>th</sup>, I received a letter from Prof. Immanuel Bomze, co-editor-in-chief of EJOR, in which he accused me of plagiarism, saying that there is "considerable overlap" with Weber (2014). Also, he claimed that I had taken advantage of my "powerful position" of "anonymous referee"<sup>7</sup> by trying to eliminate portions of Weber's paper, which allegedly appeared in my submitted paper as claimed original contributions. In the letter, Bomze did not specify what parts I had copied. He also wrote: "The next steps will include dissemination of related information to relevant people". This would evidently result in a direct attack to my professional integrity and reputation, although he did indicate that he would grant me the opportunity for clarification and explanation by June 28<sup>th</sup>.
12. On June 19<sup>th</sup>, I wrote to Bomze asking for more details, because his allegation was not substantiated by any indication of the allegedly plagiarised parts. Bomze did not answer me until June 25<sup>th</sup> 2015. Even at that date, he failed to supply any information

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<sup>6</sup> Since December 2014, six other papers of mine have appeared in international, refereed journals (five of which published in top rated journals): three papers deal with AIRR, three papers deal with its variant AROI. (See [my homepage](#) for a list of my papers.)

<sup>7</sup> I was not anonymous, as seen.

about the plagiarised parts and merely repeated the accusation, also writing that “The Editors of JME agree with my impression, too”. In his email (sent three days before June 28<sup>th</sup>, the deadline he himself established) he also officially informed me that I would be banned from any interaction with EJOR for a time period of five years.

13. On the same day, he sent me the reports of four reviewers of my paper: One recommended acceptance, one wrote that the paper was worth publishing but was not suited for EJOR’s readership, one suggested rejection because s/he did not see any novelty with respect to other past papers of mine. A fourth reviewer (Reviewer 2) recommended rejection but the report boiled down to a strict comparison between my paper and Weber (2014), whose Appendix was apparently taken as the benchmark approach to capital investment analysis and rates of return.<sup>8</sup> That reviewer stated that I should have cited Weber (2014) for the following reasons:

- (i) “The notion of “NPV-consistency” was introduced into the literature by Weber (2014, p. 35) as follows: “a return criterion ... is termed NPV-consistent if, for a given return benchmark (...) it induces an excess return of the same sign as the net present value ... at the return benchmark. The authors use a similar definition (on page 11) without reference to that paper”
- (ii) “The notion of equivalence class in the context of determining the NPV or AIRR has been introduced by Weber (2014, pp. 36, 38) [...] The authors introduce an equivalence class for each value of  $\beta = C$  just in the same way ... without reference to the earlier paper.”
- (iii) “The long-standing multiple IRR issue the authors refer to on page 14 has been resolved by Weber (2014), which is not mentioned in the paper.”

14. On June 26<sup>th</sup>, I wrote Bomze that

- (i) NPV consistency and related expressions have been and are extensively used in the accounting, financial, and engineering economy literature long before Weber (2014), and have also appeared in

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<sup>8</sup> It was immediately obvious to me that Reviewer 2 was Weber himself.

many papers of mine since 2009.<sup>9</sup> Also, Weber's definition is incompatible with my definition;<sup>10</sup>

(ii) the notion of equivalence class of investment streams was formally defined by me since 2009 and used extensively in many subsequent papers of mine. I even devoted two papers to this notion, written in 2009 and 2010. This notion is foundational for my AIRR approach. So, it is the other way around: Weber (2014) plagiarised my previous works using the notion of equivalence class of investment streams without due acknowledgment;<sup>11</sup>

(iii) the multiple-IRR issue has no unique solution: there are tons of alleged "solutions" in the literature. Any scholar presenting a proposal may (and does) claim that he has solved the issue.<sup>12</sup> However, it is well-known that major recent contributions in this regard, predating Weber (2014), are Hazen (2003), Hartman and Schafrick (2004), and Magni (2010). So, again, there was no need for citing Weber (2014), as it was just one of many other papers in the literature devoted to the multiple-IRR problem<sup>13</sup> (and, as I repeat, my submitted paper did not deal with multiple IRRs).

15. On June 29<sup>th</sup>, I wrote to Bomze a "Note" containing extracts from my papers (predating Weber's submission to JME) and bibliometric evidence that gave overwhelming support for (i)-(iii) above, asking for immediate withdrawal of the allegation and the ban.

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<sup>9</sup> Synonymous expressions such as "NPV consistency", "NPV compatibility", "goal congruence" have been around for decades in the literature.

<sup>10</sup> Actually, "my" definition is not mine at all; it is currently used in the relevant literature and clearly distinguishes between investment and financing, as is obvious to any informed scholar. It is also worth noting that, in my last report, I explicitly supplied Weber with the correct definition of NPV-consistency, which is equal to the one presented in my submitted paper.

<sup>11</sup> In various papers, I have used various expressions such as "equivalence class", "depreciation class", "investment class", "PV-equivalent investment streams".

<sup>12</sup> Including myself: "This article offers a complete solution to this long-standing unresolved issue " (Magni 2010, Abstract); "The recent notion of average internal rate of return (AIRR) ... completely solves the long-standing problem of the internal rate of return (Magni 2011, Abstract); "the internal rate of return (IRR) approach is unreliable and the recently introduced average internal rate of return (AIRR) model constitutes the basis for an alternative theoretical paradigm of rate of return" (Magni 2013, Abstract). However, contrary to all other alleged solutions, my approach is *complete*; that is, it solves *all* the problems of IRR. (At the 2010 AMASES Conference I presented a paper with the title "On the long-standing issue of the internal rate of return: A complete resolution". See also Magni, 2013, for a compendium of all flaws of the IRR and their resolution, point by point).

<sup>13</sup> The misrepresentation of my approach by Weber (2014) and the clumsy plagiarism was itself a motive for not citing that paper, also because readers referred to Weber (2014) would be misled in interpreting the AIRR approach.

16. On the same day, I wrote to Prof. Roman Slowinski, coordinating editor of EJOR, sending along a chronology of events, to give him a clearer view of the situation and the unwarranted allegation. He answered me that he was reconsidering the case by turning to experts, which would take some time.
17. On July 16th Bomze wrote me that, based upon my messages, opinions were invited from experts; he admitted that the allegation was unwarranted and that the review was actually biased. He admitted that “even some of the JME editors may have been distracted by, or could also have overlooked this fact.” Consequently, the allegation was withdrawn and the ban was lifted. Yet, Bomze did not apologise for his error and even wrote “Needless to say, the picture would have been much clearer if Weber's 2014 JME paper were cited by you in EJOR-D-14-02840.”
18. On July 21<sup>th</sup> I wrote to Slowinski: “I am glad that EJOR can count on a balanced and thoughtful coordinating editor like you” and informed him that I intended to reply to Bomze expressing my dissatisfaction with the way he seemed to be suggesting that the affair was somehow my fault when it was pretty obvious that it was Weber who was responsible for bringing the EJOR (and Elsevier) into disrepute. Slowinski answered that such an email would not probably help our mutual understanding, so I dispensed with it.
19. In the course of July and August 2015 I sent my rebuttal note and the chronology of events to many respected scholars and received, in response, emails expressing disbelief for such an affair, which was variously considered “Kafkaesque”, “unprofessional”, “ugly”, “extraordinary”, “unusual”, “unfair”, “absurd”, “unethical”, “incomprehensible”, “unbelievable”, “terrible”, “messy”, “ridiculous”, “horrific”, “abuse”, “academic bastardry”, “living hell”, and the like.
20. In August 2015 a new paper of mine was published on EJOR itself (Magni 2015), one which had been previously accepted on February 4<sup>th</sup> and made publicly available online on February 15<sup>th</sup>, that is, several months before Bomze’s allegations. Even this paper was entirely devoted to AIRR and its product structure, as well as its NPV-consistency; even in that paper I did not mention Weber (2014). Yet, Bomze did not make any reference to that preceding paper.<sup>14</sup>

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<sup>14</sup> It is also worth noting that Weber’s first submission did not mention any of my papers on AIRR or its variants and did not acknowledge that I first solved the issue completely; yet, Kübler did not charge Weber with plagiarism, notwithstanding my reports. This resulted in a ‘double standard’ of editorial conduct by two editors who had the opportunity of communicating with each other (as Bomze testified).



21. On September 22<sup>th</sup> I wrote to Prof. Atsushi Kajii, editor of the *Journal of Mathematical Economics* (JME), asking for a retraction of the Appendix of Weber's paper or, alternatively, a correction note which should acknowledge my authorship of my ideas and a correct representation of my AIRR approach.
22. On November 16<sup>th</sup> I was invited by Roman Slowinski to review a paper on NPV submitted to EJOR.
23. After turning to experts, Kajii wrote me, on January 29<sup>th</sup> 2016: "I am convinced that Weber's paper contains errors and omissions" and "I do feel that I should do something in the journal to acknowledge your contributions correctly." Kajii proposed to settle the matter by publishing a paper by me on JME to provide an overview of the product method implicit in my AIRR approach and its NPV consistency.
24. On January 30<sup>th</sup> 2016 I accepted Prof. Kajii's proposal, who answered: "I am looking forward to such a paper, and in fact I am very thrilled and excited to have such a paper." On June 24<sup>th</sup> I submitted the paper, with title "Capital depreciation and the underdetermination of rate of return: A unifying perspective." (Magni 2016)
25. On July 14<sup>th</sup> 2016 Anthony Doyle, the executive editor for engineering at the UK office of Springer wrote me: "I am impressed with your paper, "Measuring the inadequacy of IRR in PFI schemes using profitability index and AIRR", in *International Journal of Production Economics* and I would like to invite you (and your colleagues and collaborators as appropriate) to write or edit a book for publication as part of Springer's list." On July 15<sup>th</sup> I accepted the invitation.
26. On September 19<sup>th</sup> 2016 the paper was officially accepted by Kajii. The report of an anonymous reviewer begins with the following sentence: "This was a most enjoyable reads - arguably the best "history" on rate of return and how the research has progressed over time. Additionally, it clearly places the deserved importance of AIRR in the argument. Most importantly, it lays out some future research." Kajii wrote: "I am very glad as the editor of this journal to have this paper." At the end of the paper, I acknowledge Kajii's and Slowinsky's academic integrity and competence as editors.
27. On October 1<sup>st</sup> I started work on the book. Its temporary title is *Rate of Return and Economic Analysis of Investments. A Link among Economics, Finance, and Engineering* and is entirely devoted to the AIRR paradigm.



28. The paper was published in the December issue of the JME.

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