QUESTION (HD2206) In an email dated: 9-20-21, Dan Anderson wrote to me, and to David Anderson, that "Proof of Theorem 56 in Kaplansky has error. He orders by reverse inclusion but in the last line uses inclusion, so we don't contradict maximality(which is minimality)". I told him. "But it's not Kaplansky's theorem, the proof may be his. I seem to recall seeing the result in a book and I do not recall seeing the switching the order trick." Then from memory I wrote to him saying, "Isn't it Chevalley's extension theorem?" He did not respond. Later, when I had some strength, I looked up Chevalley's Extension Theorem in [Engler and Prestel's, Valued Fields] (which I had to buy), wrote the following note: https://lohar.com/researchpdf/Chevalley%20Theorem.pdf and circulated it among some of my "friends". I present below some of their responses and ask: What did I do right or wrong? If you have a comment on my note feel free to write to me/tell me off at: mzafrullah@usa.net

(1). One gentleman tells me: I have seen Corollary 2 and I probably used it at least once in that form in one of my papers. The proof that you have given of the theorem preceding it is not new to me – the assertion of survival in either A{u] or A[u^{-1}] (I use A here because I do not recall what you called the base ring) is in Bourbaki (perhaps as an exercise if not a full-blown theorem/proposition) and/or in Artin's Gordon and Breach (?) classic (!) and/or Artin's NYU notes. I saw something like it in a course on curves taught by Rosenberg in the spring of 1966; rumor had it that Rosenberg was using some notes of Weil (if memory serves) from Univ. of Chicago. On the other hand, Rosenberg may have been using Chevalley's not-so-well-known textbook. (In the earlier course that Rosenberg taught in fall 1965 (first year grad alg), I discovered later that Rosenberg's sources for his lectures and much of the homework had been Kap's soon-to-be-published "Rings and fields" (do I have the title backwards?) for field theory and the text of Chevalley for tensor products/algebras and exterior powers/algebras.) I recall seeing your main theorem in the form (if not the notation) that you stated several times around 1966-67 when I was learning the basics of comm alg and alg number theory. Dan was right that Kap made a mistake in his (Kap's) proof, by defining the partial order by using reverse inclusion and then applying ordinary inclusion to what had follow from using Zorn.. So, something of the kind that you drafted should be published, but please do not reinvent the wheel. I suggest that you publish as little new detail as is necessary (after checking Bourbaki and Artin and the other work of the same Artin). Math aside, I hope that you are well. Sorry for any typos that may remain.

(To it I responded with: "Thanks! The purpose was to "inform" and "not to re-invent the wheel"! I hope, I have done that adequately.") The response was immediate: You have certainly done that. To see some other places where it was noted or used that M survives in at least one of R[u] or R[u^{-1}], you might want to look at the text by Larsen and McCarty, also some references that were made in the papers in PAMS around 1973-74 by Ratliff (on the kernels of the two evaluation maps) and by me (On GD for simple overrings). I recall showing that R \subset R[u^{-1}] has GD if and only if R\subset R[u] does, after noticing the same result for flatness instead of for GD. (This result may be in OGDFSO II.) Over the years, I have down-sized, getting rid of many papers including my own, and many textbooks, so I am relying on 50-yer-old memories here and some of those memories may only be fantasies.

(2). I think that your approach is interesting, but I do not think that I agree that the proof is strictly in error. There is an unfortunate use of the "\supset" as opposed to "\subset" and the verbiage used is "...decreeing inclusion to mean both $R_a\supset R_b$ and $I_a\supset I_b$". I always interpreted this as the pair (R_a, I_a) being the "bigger" one. Admittedly, I think I would have made some slightly different choices in exposition, but I do not think that the theorem is in danger.

That being said, I think your approach is interesting. Although I have not checked the details, I think that it would make an interesting note.

(3). thank you for sharing your preprint with me. I knew of this inaccuracy in the proof of Theorem 56 in Kaplansky's book and had interpreted it exactly as Jim wrote in his message. Of course, if you publish your manuscript, it can be very useful as a new approach and reference. Furthermore, u, u ^{- 1} Lemma (Th. 55 in Kamplanski's book) which is a lemma for Th. 56, becomes a corollary of your statement in your approach. (I wrote back saying: "The purpose of writing that note was to inform researchers in the area that there's nothing wrong with that theorem, just in case Dan had shared that information with others. (Actually when I received that message from Dan, my immediate response to Dan was the result was not his (Kaplansky's) and that I had seen a similar result elsewhere.) When I finally saw that result, I jotted down the note. I am not reinventing the wheel, I am informing.")

(4). This fills a much needed gap in the literature. (When I sought explanation by saying: "Thank you, if you mean it is a commendable effort to fill a gap in the literature." He came back with a "Nope"!)

(5). I agree with J and M. It is not correct that Kaplansky's proof of Theorem 56 is incorrect, at worst it contains a typo. Note that Gilmer has essentially the same proof in MIT. Upshot: If you decide to write this up as a note, I would not say that there is an error in Kaplansky's proof, just that you are giving a (actually only slightly) different approach. (To this, I shot back with (I am ashamed to admit) "I quoted Dan on the error thing and I have given the result by Chevalley, of which Theorems 55 and 56 of Kaplansky are a (slight) variation."

Again, if you have a comment or an example of the use of all or part of Chevalley's Extension Theorem, I'd like to hear from you.