A REPORT ON "ELEMENTARY THEORY OF FREE NONABELIAN GROUPS" BY O. KHARLAMPOVICH AND A. MYASNIKOV

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This paper reviews approximately 30 revisions of the paper "Elementary theory of free nonabelian groups" by O. Kharlampovich and A. Myasnikov, starting with the original version of December 2000, and ending with the published version of June 2006. The paper describes the progress in these revisions, indicating mistakes, fatal mistakes, disagreements between the versions, and increasing similarities with [Se1]-[Se6].

Alfred Tarski had posed his famous problems on the elementary theories of the free groups around 1945. For many years these problems were considered among the major problems on the borderline between algebra and logic (see [ELTT] or [Mal]). Although quite a few people have been working in the general realm of these problems, little progress has been made until the beginning of the 1980's. In 1982 G.S. Makanin [Ma] presented an algorithm that can decide whether any given system of equations over a free group has a solution, and later used it to show that the positive and the universal theories of free groups are decidable. A.A. Razborov [Ra] used Makanin procedure to analyze (combinatorially) the entire set of solutions of a given system of equations over a free group. In other words, Razborov gave a combinatorial description of varieties over a free group.

The work of Makanin and Razborov enables to analyze sentences with one quantifier, but it turned out to be still far from analyzing general sentences and formulas, even sentences with only two quantifiers.

Shortly after we started working on these problems, at the end of 1994, we discovered that although geometric methods, in particular the JSJ decomposition, can be applied in studying Tarski's problems, previous ideas (in particular E. Rips' approach) are insufficient, and Tarski's problems are far more difficult than it was previously thought. Furthermore, it became clear that the main difficulties in tackling these problems were not previously formulated or explored, and a completely new structure theory had to be developed. We have spent six more years working entirely on these problems, before announcing a solution and posting it in six papers on our webpage in November 2000 [Se1]-[Se6]. As our work was rather long (500 pages) and complicated, it took five and a half years more before all these papers were finally reviewed and published.

O. Kharlampovich and A. Myasnikov announced a positive solution to Tarski's problems in June 1998 [KM1]. Although they claimed that they had a written manuscript they refused to distribute it. Following our announcement, in December 2000, they finally posted their work on O. Kharlampovich's webpage.

Their papers showed clearly that Kharlampovich and Myasnikov had not understood the main difficulties in tackling Tarski's problems (as these were not previously known), and in addition they had not developed any tools to tackle these difficulties (see section 1). At the time, we thought that there was no point in reacting to such papers, as the authors clearly had not understood the extent of the difficulty. After a few months (in April 2001) we posted a counter-example to the main claim of the authors (theorem 5 in [KM6]), which demonstrates that their approach could not possibly work.

Kharlampovich and Myasnikov wrote an answer to our counter-example (see section 2). To our surprise, following that answer an amazing process started to take place: for about 5 years (April 2001 to June 2006), a sequence of more than 40 revisions of the original papers of the authors was posted on Kharlampovich's webpage, and the original papers disappeared. These revisions have less and less to do with the original papers of the authors, while more and more constructions, definitions, notions, terms, notation, procedures, arguments, and at times even mistakes from our papers found their way into their revisions.

As one may understand, we found ourselves in a very strange situation. We submitted two formal complaints to the editor of the Journal of Algebra who handled Kharlampovich and Myasnikov's papers ([Ze1],[Ze2]), but these were never answered. Several people urged us to submit a formal complaint to various committees and institutions, and others convinced us that there was practically nothing that could be done or was worth doing.

The recent invitation of Kharlampovich and Myasnikov to the ICM, and the impending recognition of their work that might follow, convinced us that a sort of report on their work had to be written. We decided to concentrate on their last paper "Elementary theory of free nonabelian groups", as it contains the main body of their work, and was revised more than any of their other papers. As will be seen, there is very little in common between the published version of this paper and the original versions of Kharlampovich and Myasnikov's papers.

In this review we go over the sequence of approximately 30 revisions of the last paper of Kharlampovich and Myasnikov that we kept, starting with their original papers (December 2000) in section 1, and ending with the published version (June 2006) in section 15. For each version, we indicate the main changes and disagreements with the previous versions. We further specify the new similarities with our papers [Se1]-[Se6], indicating the precise line numbers in their revisions and in our papers, so that any comment can be verified.

About a third of our survey (pages 41-62) is devoted to the published version that appeared in the Journal of Algebra in 2006 [KM8]. All the versions of this paper contain serious gaps, and only partial definitions (as our definitions are often rather long), but in the final version the authors tried to add some details. However, in doing so, and probably because of lack of understanding, there are many mistakes, many of which are fatal, that appeared in the published version. Therefore, in addition to exact pointers to similarities between our papers and the published version, we include in our review of the published version (section 15) a long list of (crucial) mistakes.

From our point of view, the abundance of mistakes implies that any major claim made by the authors that does not appear in our papers should be considered as an open problem. These include the authors' claim of solving Tarski's problem on the decidability of the elementary theory of a free group, and its more recent generalization to torsion-free hyperbolic groups [KM9]. The proof of these decidability theorems still requires a careful and detailed analysis that cannot be found in the authors' papers (see section 15).

An appendix is added at the end of this paper in favor of the non-experts. It presents some of the bold (mathematical and literal) similarities between our papers and the authors' many revisions. We do not get into exact details in the appendix (these can be found in sections 1-15), but we hope that it gives an idea of the amazing process that we have been witnessing since April 2001 until June 2006, that ironically led to the invitation of Kharlampovich and Myasnikov to the next ICM in Korea.

§1. The initial versions - December 2000

Olga Kharlampovich and Alexei Myasnikov publically claimed to solve Tarski's problems on the elementary theory of a free group in a lecture at MSRI, June 1998 (see [KM1]). KM did publish two manuscripts on the structure of varieties over free groups in journal of Algebra in 1998 ([KM2],[KM3]), but since their announcement, and although they claimed to have written papers, they refused to present any manuscript that contained a proof of Tarski's problems publically.

In November 2000, in lectures at Utah and Princeton, I have announced a solution to Tarski's first problem, and uploaded a complete proof (500 pages) on my web page ([Se1]-[Se6]). My original version of the proof still exists on my webpage, it was revised once, and the final version is now published ([Se7]-[Se13]).

Following my announcement, on December 15th 2000, Kharlampovich and Myasnikov posted their papers that claimed to prove Tarski's problems. This section reviews the complete content of these papers ([KM4]-[KM6]).

"Implicit function theorem over free groups" [KM4] - in the first section (pages 1-7) the authors reduce a first order sentence over a free group to a sentence in which there is only one equation and inequation (this can be done only with coefficients, and is not really helpful for the general theory). In the second section (pages 8-16), the authors basically review some of their results from the papers in the Journal of Algebra regarding the structure of varieties (algebraic sets) over a free group. In the next section (pages 16-20), some formalities on lifting equations are presented, leading to a formulation of their implicit function theorem in the next section.

In the next section (pages 20-36), a version of the implicit function theorem is stated (theorem 2), and proved, for standard quadratic equations. In the rest of the paper (pages 36-39) a formulation of the implicit function theorem for NTQ groups is presented, with a sketch of a proof.

"Equations over fully residually free groups" [KM5] - the goal of this paper is to analyze sets of solutions to systems of equations over NTQ coordinate groups. This is done by modifying the Makanin-Razborov elimination process from free groups to NTQ coordinate groups. The basic result is presented on page 6. The modified Makanin-Razborov process is presented on pages 7-26.

In section 5 there is a discussion of what the authors call "projective images of NTQ systems". This means starting with a variety that corresponds to an NTQ system, and iteratively adding Diophantine conditions that are in NTQ form as well (this is not a loss of generality). They present a basic claim that in such a sequence of projective images, there exists a certain NTQ system that is a projective image

Q of the original system but this system doesn't necessarily appear in the sequence, so that for some index in the original sequence, the values of the original variables that are associated with the projective images, starting from that index, are in fact values of the NTQ system Q for appropriate values of its terminating free groups.

Important remark: We should note here that this claim is fairly elementary and not difficult to prove (the authors claim to prove it on one page), but it has not much to do with the main difficulty in solving Tarski's problem, namely the termination of iterative procedures for the analysis of sentences and formulas. In short, it is very far from any approach to constructing such terminating procedures, and doesn't really play a role in any procedure that is currently known. Also, note that the claim is general, and does not involve any particular sequence of projective images (in the terminology of the authors). In the sequel (using some examples that I presented in 2001) we will demonstrate that in general such systems of projective images do not terminate, and that very particular sequences do terminate (and it's difficult and unclear how to choose such and or even to decide if they exist at all).

Section 6 (pages 27-29) analyzes different solutions to the same system of equations: $w(x_1,\dots,x_n)=w(y_1,\dots,y_n)$. The authors use iteratively the structure of solutions to system of equations with one variable. It is important to note that the bound they obtain using this argument depends on the specific values of the elements $x_1,\dots,x_n$, and not only on the structure of the equation w (it is claimed differently in the paper), and that the analysis in this section has nothing to do with the study of system of equations with parameters that appears in our papers ([Se7] and [Se9]), and in particular the bound that they obtain has nothing to do with the bounds that are obtained in [Se9]. The analysis that the authors are doing here is valid for equations over free semigroups (where Tarski's problem has an opposite answer, arithmetic can be interpreted, and hence, there is no quantifier elimination, and the elementary theory is undecidable), whereas the bounds that are obtained in [Se9] are not.

"Elementary theory of free nonabelian groups" [KM6] - this is supposed to be the main and the last paper of the authors, that is supposed to prove Tarski's problems (it is only 14 pages). We'll summarize its content in detail. Page 2 introduces the JSJ decomposition. Page 3 repeats the claim that every fully residually free group can be embedded into an NTQ group. Theorem 3, which is the main claim in page 4, that is proved on the next pages, is false (it is true for shortest homomorphisms but not as stated). In any case, it is supposed to serve as a step in constructing a Makanin-Razborov diagram using the JSJ decomposition, somewhat similar to what is done in section 5 of [Se7]. The proof of theorems 3 and 4, and hence of the existence of a Makanin-Razborov diagram that uses the JSJ decomposition, is contained in pages 4 to the top of page 10.

Theorem 5 is the main theorem of that paper (and of the entire work of KM). They define a canonical embedding of a fully residually free group into an NTQ group as the one that requires minimal possible additional variables. This is not a mathematical definition, and it doesn't really make sense. Furthermore, there is no discussion of that notion or definition in any other part of the work. In the next line it is indicated that a homomorphism from a fully residually free group into NTQ system as described earlier using the JSJ decomposition, and the associated Makanin-Razborov diagram, is canonical (the two definitions of the same notion in two consecutive lines do not seem to agree).

Theorem 5 claims that any sequence of homomorphisms, in which the odd homomorphisms are canonical embeddings, and the even homomorphisms are proper epimorphisms, terminates. No further conditions on the sequences in question are specified. A counter example to theorem 5 was provided in [Se14]. The failure of theorem 5 is the main difficulty in approaching Tarski's problems. Most of my own work (papers [Se9]-[Se12]), and by far most of the time that I worked on Tarski's problem was devoted to overcome the failure of this theorem.

The authors give a proof of Theorem 5 that makes no sense (indeed, the theorem is false). In it they are using their notion of projective images, that by itself doesn't help in proving any similar statement (once again, the theorem is false). In the rest of the paper, the authors claim to apply (the false) theorem 5 to validate a general first order sentence. There is no point in getting into their argument (it takes less than 3.5 pages), except for mentioning that its main tool is (the false) theorem 5.

To Summarize the initial sequence of papers: the sequence of three papers that was posted on December 2000 by KM presented some basic tools that are needed in dealing with Tarski's problems, but contained no real strategy, or a real approach to the problems. In fact, the main difficulty in tackling Tarski's problems, the failure of their own theorem 5, was not noticed by the authors (and hence, no tools were even considered to overcome this failure). For comparison, all the tools that are developed in the initial sequence of papers of Kharlampovich and Myasnikov are strictly contained in sections 5-8 of [Se7] and 1-2 of [Se8] (in particular, they contained nothing from [Se9]-[Se13]). There is also no consideration of equations with parameters (the authors didn't see any need for it). Note that the theory of equations with parameters is essential in any attempt to prove quantifier elimination, any attempt to solve Tarski's problems, and probably in any attempt to overcome the failure of the authors' theorem 5 (that requires much more than that).

§2. My counter example and the answers it received.

In April 2001 I posted a counter example to the main theorem (theorem 5 in paper 3 "Elementary theory of free nonabelian groups") in the three papers of KM that aimed to prove Tarski's problems (see [Se14]).

The answer to that counter-example that was titled "On the non-terminating misreading" [KM7], already borrows notions, claims and arguments from my papers (that have already been available on the web since December 2000). Below is our analysis of that answer [KM7] of KM, as it was sent to Efim Zelmanov, an editor of Journal of algebra, who handled KM papers.

In their note, "On the non-terminating misreading", O. Kharlampovich and A. Myasnikov are aiming to explain the statement of theorem 5 in their paper "Elementary theory of free non-abelian groups", and given that explanation, they are explaining why the counter-example to theorem 5 presented in [Se14] is not really a counter-example to their theorem.

In the sequel, we will explain (with exact references) why the "commentary" of Kharlampovich and Myasnikov contradicts the formulation of their own theorem, why the example presented in [Se14] is indeed a counter-example to theorem 5 in [KM6], why the "commentary" they wrote and the main objects and notions it uses have no hint in the original text, and why it is completely based on notions and techniques borrowed from [Se7] and [Se10].

(1) Page 1 lines 1-2, "In this example NTQ groups are considered separately

from the fundamental sequences defining them". The statement of theorem 5 in [KM6] is clear (given the previous definitions of the authors), and requires no further explanations. Neither in the statement of the theorem, nor in the definition of canonical embeddings or the proper quotients the authors are considering, any fundamental sequences related to the NTQ groups are mentioned or discussed. These are also not mentioned in the proof of theorem 5 in [KM6].

(2) Page 1 lines 3-5, "a proper quotient of the initial groups is taken (in this situation our process stops because canonical homomorphisms restricted to the initial group must be embeddings)". Again, the statement of theorem 5 in [KM6] is clear, and nothing like that is hinted there. This condition appears in the iterative procedure presented in [Se4] (case (1) on page 74).

(3) Page 1 lines 5-7, "We always consider the pair: NTQ group together with the fundamental sequence defining this group, and there additional requirements on this fundamental sequence, naturally extracted from generalized equations". Once again, the formulation of theorem 5 in [KM6] is clear. Nothing of this sort is hinted. Besides, how should one interpret the sentence: "there additional conditions...naturally extracted from generalized equations".

(4) The two lines before theorem 5 (page 10 in [KM6]) "If G is a fully ... in such decomposition" do not exist in the original text (hard copy version, before the papers were put on the web). In fact, in all the 5 papers of Kharlampovich and Myasnikov there is no study of a fully-residually free group relative to a (general) subgroup (what is presented and called *graded* limit group in sections 9 and 10 of [Se1]). We should note that the authors do study a fully-residually free group relative to a coefficient subgroup in their text, but they never do it relative to a general subgroup (which is a rather different study), and of course, from their text such study does not seem to be needed anywhere in their approach towards Tarski's problems.

(5) Definition 3 in their note [KM7] is new (the notion of a sufficient splitting is never mentioned in their text). It was never hinted in the text. This is a basic concept in the entire note, and is connected to our analysis of graded limit groups in sections 9-10 in [Se1].

(6) "one can obtain a relativized version of the theory above *modulo* subgroup H" (page 2 lines 1-2 in [Kh-My4]), is completely new. This is the theory of graded resolutions that appears in section 10 of [Se1], pages 50-56.

(7) "essential abelian splittings of G modulo H may do not exist" (page 2 lines 4-5) - a new phenomenon, never mentioned or hinted at in the text. It is precisely the definition of a *rigid limit group* ([Se1], definition 10.2, page 52).

(8) "In case they exist sufficient abelian splittings of G with respect to H may do not exist" (page 2 line 5) - a completely new phenomenon. It is precisely the definition of a *solid limit group* ([Se1], definition 10.2, page 52).

(9) "They are obtained from canonical JSJ decomposition of G by collapsing some edges connecting non-QH vertex groups" (page 2, lines 10-11) - this is a false basic claim.

(10) Page 2 lines 12-15. Embedding and Canonical embedding trees are presented. These were never mentioned nor hinted in the statement or the proof of theorem 5 in [KM6]. In fact, these are not mentioned anywhere in

the text of the authors. The statement of theorem 5 in [KM6] deals with a general iterative process in which at each step one continues to a canonical embedding and then to a proper quotient of the corresponding (minimal) NTQ group (the terminology is the one used in theorem 5 of the authors). As one can learn in the sequel, the construction of these trees does not agree with the assumptions of theorem 5 in [KM6].

(11) "Denote by H the subgroup in G generated by all vertex groups which are neither QH subgroups nor abelian subgroups in D" (page 2 lines 22-23) - this is clearly not well-defined, since a vertex group is defined only up to conjugation. It is a variation of the multi-graded resolutions with respect to the non-abelian, non-QH vertex groups, considered in section 4 of [Se4], page 69. Multi-graded resolutions are neither considered nor hinted in any of the papers of the authors. In an earlier version of the note, the authors had used a definition that is closer to the original definition that appears in section 4 in [Se4], and changed it afterwards.

(12) "If the restriction on H ... , we do not continue" (page 2 lines 23-24) - something like that is never mentioned in theorem 5 in [KM6], it's proof, or the proofs of theorems 1 and 2 in that paper. A condition similar to that does exist in [Se4], parts (1) in the first, second, and general steps (see part (1) on page 74 in [Se4]).

(13) "Call these groups first type terminal groups of the first level" (page 2 lines 30-31). Nothing like this appears or is hinted at in any of the papers of the authors. These terminal groups are precisely the rigid or solid limit groups that appear as terminal groups of a graded resolution that appears in our construction of a graded resolution ([Se1], page 55). We should add that in our iterative procedure for validation of a sentence, such groups are called "terminal" ([Se4], proposition 4.3 part (ii), page 71).

(14) "Consider solutions ... Call them dimension increasing" (page 2 lines 33-35) - anything of that sort never appears or is hinted at in any of the papers of the authors. It is precisely the homomorphisms considered in [Se1], definition 10.3, part (i) (page 52).

(15) "Consider also solutions $\bar{U}$ that factor through quotients of $F_{R(\bar{U})}$ containing H as a subgroup, and having sufficient abelian splittings modulo H" (page 2 lines 36-37) - this is not a well-defined definition, which does not appear or is hinted at in the text. It is supposed to substitute (or to be an analogue of) part (ii) in definition 10.3 of [Se1] (page 52).

(16) "Solutions of these two types can be factored through a finite number of maximal quotients of $F_{R(\bar{U})}$" (page 2 lines 37-38) - according to the above definition, this is false. Note that the authors do not provide a proof of the statement. The claim is supposed to state lemma 10.4 and definition 10.5 in [Se1] (pages 52-53).

(17) Note that the authors call these objects "maximal quotients". This is precisely the notion used in [Se1] (definition 10.5, page 53), and is the notion used throughout our papers. The notion "maximal" is with respect to a partial order. Nothing of that sort exists in the authors' papers, and of course they don't use any partial order (Note that in [KM6] the authors use the terms minimal or canonical (with respect to some embeddings), but they never call such quotients "maximal" as they do here).

(18) "The group G is embedded into each group $F_{R(\bar{U},s_1)}$, where s_1 is a system

of quadratic equations with coefficients in H and equations of the form ... corresponding to D" (page 3 lines 11-13). This is precisely the construction of the graded completion of the graded resolution that appears in our iterative procedure for validation of a sentence (see [Se2], definition 1.12, page 25, for the construction of a completion). We should add that the notion of a canonical embedding of a fully-residually free group into a (minimal) NTQ group used by the authors in the text (page 10, line 10-12), is a different notion, as pointed out by the authors themselves, in an answer to a previous version of the counter-example.

(19) "We will include the fundamental sequence ... , which is the sum $k_1 + \ldots + k_k$" (page 3 lines 29-36) - - The authors consider fundamental sequences in their papers, but they never considered before such a partial set of homomorphisms anywhere in their text. This is precisely the definition of a taut homomorphism ([Se4], definition 2.4, page 25). These are the homomorphisms that are being analyzed along the procedure presented in section 4 of [Se4].

(20) The process of successively reducing the parameter subgroup presented in page 4, $< X_2, \ldots, X_n >, < X_3, \ldots, X_n >, \ldots$ is precisely the procedure presented in section 4 of [Se4] (see page 77 in [Se4]). There is no hint of such procedures in the papers of the authors (well, they never considered equations with parameters...).

(21) The conditions 1-3 on page 4 lines 10-14 are conditions used in the procedure presented in section 4 of [Se4] (See pages 74-78 in [Se4]. In particular condition 3 is a relaxation of a multi-graded resolution used there, and presented in section 12 of [Se1]).

(22) "We continue this way to construct resolutions" (page 4 line 20) - the notion "resolution" is a basic notion in our work, defined in section 5 of [Se1]. It was never used by the authors before. In fact, on page 3, the authors call $\bar{V}(S(b))$ a "fundamental sequence", which is the notion they use in their papers. We should note that the authors noticed the appearance of the notion "resolution" in this place a few days afterwards, and changed it to their usual notion "fundamental sequence" (that appears in few other places in the text) in a revised version of the note (we have a copy of the revised version from May 14, where the word "resolution" is replaced with "fundamental sequence").

(23) "Then either the natural image $G_{(p)}$ of G on some level p of sequence c is a proper quotient of G or can be replaced by a finite number of proper factors" (page 4, lines 29-30). This claim is the main concept on which the iterative procedure presented in section 4 of [Se4] is based (proposition 4.3 part (ii), page 71 in [Se4]). Nothing close to that appears or is hinted at in the text of the authors. We should note that according to the note, this is also the main principle used by the authors in the construction of the new tree $T_{AE}(G)$. Could it be that the main concept used in the entire process is not even hinted at in the text? that the notions and the objects that are needed to formulate the main concept are not hinted at, hence, do not exist in the text as well?

Finally, we should note that with the definition of the subgroup H used by the authors (the subgroup generated by the non-QH vertex groups), the claim is false. It is true only if one uses multi-graded resolutions that the authors do not define even in this note (see the proof of part (ii) of proposition 4.3 in [Se4], page 72).

(24) "Suppose that $G_{(1)}, \ldots, G_{(p-1)}$ are isomorphic... Consider a fundamental sequence c_3 obtained by pasting a sequence corresponding to b_2 to the bottom group $G_{(p)}$ of c_2." (page 4 lines 30-36). This construction that is the main one used in the entire process of the authors, is not hinted at anywhere in the text. It is based on the claim described in the previous two lines (29-30), discussed in (23). The construction described now by the authors is not a canonical embedding defined by them on page 10, lines 10-12, in [KM6], hence, it certainly does not agree with the notions, notation and assumptions of theorem 5 as it appears in the text.

We should note that in order to actually make the construction of the authors well-defined, one must introduce "induced resolutions" that never appeared in the text of the authors (nor in the note), that are presented in section 3 of [Se4], pages 55-66.
Finally, the construction presented by the authors in these lines (30-36) is precisely the constructions of the developing resolution and the anvil used in the iterative procedure presented in section 4 of [Se4], pages 75-76.

Some general remarks:

(1) The structure of the procedure presented in the note is essentially identical to the procedure presented in section 4 of [Se4], with some variations, mostly definitions that were changed by the authors, and cause some of the claims used by them in the sequel to be false (the same claims are true if one keeps the definitions presented in [Se1] and [Se4]).
(2) The procedure presented by the authors is not complete, mostly since they do not describe the general step, but rather the initial one. It certainly does not agree (in an essential way) with the notions and assumptions used in theorem 5 in [KM6].
(3) Finally, we should note that there is no "canonical" process for analyzing an AE sentence defined over a free group. i.e., such analysis can be conducted in many ways, even assuming that it is based on a generalization of Merzlyakov's theorem. Hence, we find it difficult to explain the close similarity between the procedure presented in section 4 of [Se4], and the process presented in the current note of the authors.

§3. Elementary theory of free nonabelian groups, May 17th 2001

On May 17th 2001 KM started a long list of revisions of their papers. The first was a revision of their 3rd paper. A report about these versions, their disagreements with the previous versions, and the notions, arguments and concepts it borrowed from my papers (that were all the time on the web and still are), was sent to Efim Zelmanov, as an editor of Journal of Algebra who handled these papers. The list below is the note that was sent to Zelmanov.

The new version of the paper "Elementary theory of free non-abelian groups", by O. Kharlampovich and A. Myasnikov, is supposed to be a revision of a pre-existing version, which describes the approach of the authors towards the solution of Tarski's problems. As we will see, there is only a slight connection between the pre-existing paper, and the current version (which is almost tripled in length

- the original length of the hard copy version was 14 pages, whereas now it is 38 pages). Although in the text the authors claim that the changes were made because of results they quote from previous papers in their sequence, most of the changes made by the authors are based on papers 3,4 and 5 in our sequence on the solution of the Tarski problems, which involve notions, claims and arguments that do not exist (or hinted at) in the pre-existing paper of the authors.

We should note that although the new text of the authors has major gaps, and the main difficulty in approaching Tarski's problems, i.e., the analysis of sentences that contain more than 2 quantifiers, is still totally false, it is certainly closer to a solution of the problems than the original text of the authors.

In this short note, we will explain (with exact references) how the new version is based on our papers, and how little it has in common with the pre-existing paper of the authors. We should note that part of the revision made by the authors is based on a previous note they had posted on O. Kharlampovich's homepage (our copy is from April 21st 2001), a note that we have addressed in a previous report, and explained how it is based on our work. Still, the current text is somewhat more detailed, hence, closer to our work, than the procedure presented in the note. We will go over the new version page by page, from its beginning to its end.

(1) Page 5 lines 2 and 3. The authors have never looked at decompositions of an ambient group, with a prescribed subgroup being elliptic (in the hard copy version before it was posted on the web). These are the graded limit groups presented in section 9 of [Se1].
(2) Page 5, Definition 9 is new. A sufficient splitting is never mentioned in the pre-existing text. Again it is based on the construction of graded Makanin-Razborov diagrams presented in section 10 of [Se1].
(3) Page 5, lines 26-32. This is all new. It is taken from our definitions of rigid and solid limit groups presented in definition 10.2 of [Se1] (we should note that the last 2 lines in this page, lines 35-36, are false).
(4) The new section, Section 2, Some auxiliary results (pages 6-7), is completely new. It is all based on our bound on the number of rigid and strictly solid families of solutions presented in [Se3] (cf. theorem 2.3 and 2.7 in [Se3]). Theorem 5 and Corollary 4 are precise reformulations of theorems 2.3 and 2.7 in [Se3]. Nothing of that sort appears in the previous text of the authors. It is also not related to anything discussed in any of the pre-existing papers of the authors.

    We would like to mention that in our papers, theorems 2.3 and 2.7, that are parallel to corollary 4, are used in the quantifier elimination procedure. In addition, the proofs of these theorems are modified to prove termination of the quantifier elimination procedure.

    Since the authors have no quantifier elimination procedure, the only place the authors use corollary 4 in this text, is the termination of the procedure they present that is supposed to analyze sentences with more than 2 quantifiers (page 37 line 8). However, the corollary itself is totally irrelevant in this place (as well as in any other place in this text). We modified the proof and used it to prove termination, and did not use the statement itself. But given the procedure suggested by the authors for analyzing sentences with more than 2 quantifiers, even the proof of the corollary is totally irrelevant.

    Finally, note that the new corollary 4, which is one of the central theorems in the entire approach to Tarski's problem, appears without any proof, or a sketch of a proof, or even a hint as to why it is true. In our papers these non-trivial and fairly difficult theorems are the main content of [Se3] (they take the entire (main) two sections of [Se3]).

(5) Theorem 6 on page 8 is supposed to be a variation of theorem 4 in the original text, but in fact it's completely different. In the previous text the authors defined a canonical embedding, as an embedding with "minimal number of variables" (of course, a completely vague definition). In this version, the authors added part (3) on page 8, that is taken from our notion of an auxiliary resolution, and auxiliary limit group (definition 4.1 in [Se4]).

(6) Page 13 lines 23-24. These two lines didn't exist in the original version (in print, prior to putting the papers on the web), and were added to the version that was put on the homepage of Kharlampovich in mid December 2000. The authors didn't consider any JSJ decompositions modulo a subgroup in the original paper (in print), nor in the version from December 2000. Now, it is strange that it appears here, since the authors talk about decompositions modulo a subgroup already in (the new) Definition 9.

(7) Page 13 lines 25-28. The trees $T_E(G)$ and $T_{CE}(G)$ are completely new - nothing of that sort is mentioned in the pre-existing text. They are mentioned for the first time in the note of April 21st, that is supposed to be an answer to the counter-example to (the old) theorem 5. We should note that the construction of these trees in the note of April 21st, although already based on the procedure presented in section 4 of [Se4], is still completely different from the construction of these trees presented in the current text, that is much more similar to the procedure presented in section 4 of [Se4]. e.g., Lemma 7 that is a main ingredient now (and in [Se4]), and on which the whole construction is based, does not appear at all in the note of April 21st.

(8) Page 13, lines -5 and -4. A completely new concept - no hint in the pre-existing text. The authors are defining the subgroup H as the subgroup generated that are non-QH non-abelian vertex groups in the JSJ of G. This is supposed to imitate the construction of the multi-graded resolution in section 4 of [Se4], that is taken with respect to these vertex groups (multi-graded resolutions are presented in section 12 of [Se1]). However, the way the authors modified our construction is wrong, and does not fit other claims and notions they "borrowed" in the sequel. For example, the statement and proof of lemma 7 is wrong if one uses this definition (it is correct if one uses our multi-graded resolutions).

(9) Page 13 lines -4, -3 and -2. A completely new concept - no hint in the pre-existing text (from December 2000). It is parallel to part (1) in the iterative procedure presented in section 4 of [Se4].

(10) Page 14 lines 1-5. A completely new construction. Parallel to our construction of graded Makanin-Razborov resolutions, presented in section 10 of [Se1].

(11) "Consider solutions ... Call them dimension increasing" (page 14 lines 7-9) - anything of that sort never appeared in the text of the authors. It is precisely the homo. considered in [Se1], definition 10.3, part (i).

(12) "Consider also solutions $\bar{U}$ that factor through quotients of $F_{R(\bar{U})}$ containing H as a subgroup, and having sufficient abelian splittings modulo H" (page

14 lines 9-11) - this is a totally false definition. It is supposed to substitute (or to be an analogue of) part (ii) in definition 10.3 of [Se1].

(13) "Solutions of these two types can be factored through a finite number of maximal quotients of $F_{R(\bar{U})}$" (page 14 lines 11-12) - according to the above definition, this is absolutely false. It is supposed to state lemma 10.4 and definition 10.5 in [Se1].

(14) Note that the authors call these objects "maximal quotients". This is precisely the notion used in [Se1] (definition 10.5), and is the notion used throughout our papers. The notion "maximal" is with respect to a partial order. Nothing of that sort exists in the authors' papers, and of course they don't use any partial order (Note that in [KM6] the authors use the terms "minimal" or "canonical" (with respect to some embeddings), but they never call such quotients "maximal" as they do here).

(15) Page 14 lines 20-24. The authors never considered this information in the "fundamental sequences" in the pre-existing text. This is part of our taut Makanin-Razborov diagram presented in definition 2.4 and proposition 2.5 of [Se4].

(16) Page 15 lines 4-9. This is all new. It is precisely the notion of a taut homomorphism and diagram presented in definition 2.4 and prop. 2.5 in [Se4].

(17) Page 15 lines 24- page 16 line 8. This is completely new. It is based on our notion of a well-separated resolution ([Se4], 2.2), and the taut Makanin-Razborov diagram (proposition 2.5 in [Se4]).

(18) Page 16 line -3. The existence of a finite number of "corrective extensions" was never mentioned in the pre-existing text. It is parallel to our notion of a covering closure of a resolution (Theorem 1.18 in [Se2]), and was added also to the new version of the authors on what they call "an implicit function theorem". Overall, this is a minor point.

(19) The process of successively reducing the parameter subgroup presented in page 17, $< X_2, \dots, X_n >, < X_3, \dots, X_n >, \dots$ is precisely the procedure presented in section 4 of [Se4]. there is no hint to such procedures in the pre-existing papers of the authors. Furthermore, the authors never defined graded diagrams, so they couldn't use them for any (terminating) procedure.

(20) The conditions 1-3 on page 17 lines 18-22 are conditions used in the procedure presented in section 4 of [Se4] (in particular condition 3 is a relaxation of a multi-graded resolution used there, and presented in section 12 of [Se1]).

(21) The continuation of the construction of the fundamental sequence on page 17 lines 23-30 is precisely the way multi-graded resolutions are constructed in the iterative procedure presented in section 4 of [Se4]. Needless to say - this is all completely new.

(22) Lemma 7 on page 17 is completely new. It is identical with part (ii) of proposition 4.3 in [Se4]. This lemma is indeed the key for the construction of the terminating iterative procedure for validation of an AE sentence presented in [Se4], and is also the basis for the procedure presented in the current text. This lemma does not appear even in the note posted by the authors on April 21st, as an answer to our counter-example. Needless to say, the procedure that appears in this note is completely different from what is presented in the current text.

The proof of lemma 7 is identical to the argument used in the proof of proposition 4.3 in [Se4]. However, it is true for multi-graded resolutions, and completely false if one defines the subgroup H the way the authors did, and looks at graded resolutions as they do.

Also, note that the proof of lemma 7 (which is precisely the proof of proposition 4.3 in [Se4]) is heavily based on the properties of the JSJ decomposition. Indeed, the JSJ decomposition is the main tool that is used in our approach to Tarski's problem. However, although the authors mention the JSJ decomposition in the initial version of their third paper, they have never really used any of its properties. i.e., the JSJ doesn't really play any role in any of the arguments that appear in their original papers, and is not really used there.

(23) Page 18 lines 16-41. This is completely new. It basically describes the general step of the procedure presented in section 4 of [Se4], including the same division into various cases, and the construction of what we call the "developing resolution", the amalgamated product described in lines 34-35. We should note that our procedure is by no means "canonical", i.e., there can certainly be other procedures for validation of an AE sentence, so there are no "objective" reasons for the fantastic similarity between the procedure presented in these lines and the procedure presented in section 4 of [Se4].

We should also remark that in order to define our "developing resolution", the amalgamated product described in lines 34-35, we use "induced resolutions" that are constructed in section 3 of [Se4]. Of course, the definition of the amalgamated product presented by the authors in these lines makes no sense without this notion, but it doesn't seem to bother the authors. Note that this entire procedure is completely different from the procedure presented by the authors in their note from April 21st (the answer to the counter-example).

(24) Definition 12 on page 19 is new. It is precisely our definition of the complexity of a resolution ([Se4], definitions 1.15 and 3.2). We should note that our complexity is not "canonical", i.e., there is no reason to choose precisely our complexity measure, but somehow the authors ended up with the same complexity measure. We should also note that the authors slightly modified our complexity measure by dropping the Euler characteristic of the surfaces associated with the various QH vertex groups - this is wrong, and contradicts some of their statements in the sequel (which are true when one considers the definition without the authors' modification).

(25) Page 19 lines 20-21. A new concept, which is precisely the notion of a taut homomorphism presented in definition 2.4 of [Se4].

(26) Page 18, lemma 8. A new claim. Identical to proposition 4.2 in [Se4]. Of course, this lemma is true with our original notion of complexity, and not with the slight modification made by the authors.

(27) The authors state lemma 7 in terms of the original limit group. This is good only for the initial step of their procedure for the construction of the tree $T_{AE}(G)$. To guarantee termination, with the complexity they use, they will need a similar lemma for the next steps that involve the ambient group, and not only the original limit group, i.e., they'll need lemma 4.6 from our paper. Of course, they will also need to work with induced resolutions that they had not defined (even in the current text). The way their procedure for the construction of the tree $T_{AE}(G)$ tree is defined, it is not guaranteed

to terminate.

(28) Page 23, Theorem 8. The last condition "The family of schemes can be taken ..." is new. It is taken from our definition of auxiliary resolutions (definition 4.1 in [Se4]).
(29) Page 26 lines 6-12. All new. This is precisely the construction of the auxiliary graded diagram presented in definition 4.1 of [Se4].
(30) Page 27 lines 4-5 and lines 6-7. This "pasting" operation is completely new. It is taken from the construction of the developing resolution in the iterative procedure presented in section 4 of [Se4].
(31) Page 27 lines 21-22. Completely new - not mentioned in the pre-existing text. It is also taken from the construction of auxiliary resolutions presented in definition 4.1 of [Se4].
(32) Page 27 lines -8 to -6. This never appeared in the procedures presented in the pre-existing text. It is taken from condition (1) in the general step of the iterative procedure presented in section 4 of [Se4].
(33) Page 28 lines 1-2. A new construction. Taken from the construction of the developing resolution in section 4 of [Se4]. To actually construct it, one needs the notion of an induced resolution, presented in section 3 of [Se4], that the authors don't have even in their new text.
(34) The procedures that are used to prove theorems 1 and 2 are very different from the ones presented in the pre-existing text, and even from the procedures presented in the note from April 21st (the answer to the counter-example), and are rather similar to the procedures used in [Se4] and [Se5]. However, to guarantee the termination of the iterative procedure for more than 2 quantifiers one needs much more. The authors use Corollary 4 (page 37 line 8). However, the corollary is not sufficient for proving termination; In fact, it's not clear why it is closely related. Indeed, in section 4 of [Se5], we imitate the proof of the existence of a bound on the number of rigid and strictly solid solutions ("parallel" to corollary 4), but the statement of the corollary certainly does not imply termination, and in the framework of the authors, even the proof won't help. This was and still remains the major gap in the approach taken by the authors, even with all the modifications they made, and the concepts they borrowed. Of course, this is the main difficulty in approaching Tarski's problems.

Some general remarks:

(1) The structures of the procedures presented in the new text are similar to the procedures presented in section 4 of [Se4] and section 4 of [Se5], with some variations, either because of lack of notions and arguments, or because the authors thought (mostly wrongly), that they can modify the procedures in order to prevent those notions and arguments.
(2) Even with the procedure defined by the authors in the note, it is completely not clear why the procedure terminates, especially in the case of more than 2 quantifiers.
(3) Throughout the new text, the authors use their "presentation" of results from previous papers to bring in new notions, techniques and claims that never existed, nor even hinted, in any of their pre-existing papers, and can be easily traced in our work.
(4) The procedures described in the new text are much closer to the procedures

presented in [Se4] and [Se5] than the ones presented in the note that was supposed to be a counter-example to theorem 5 in the pre-existing text. The procedures presented in the note are much closer to the ones presented in [Se4] and [Se5] than to the ones presented in the pre-existing text. We can only guess what the procedures presented in a forthcoming version will be.

Two particular bold similarities of the new version of the paper "Elementary theory of free non-abelian groups" and [Se4] are the following (both do not exist, nor hinted, in the old version of the paper):

(1) Definition 12 on page 19. "A characteristic of R will be the sequence:

$$ch(R) = (k, (g(R_{j1}), r(R_{j1})), \dots, (g(R_{jn}), r(R_{jn})))$$

where $(g(R_{ji}), r(R_{ji})) \geq (g(R_{j,i+1}), r(R_{j,i+1}))$... ... Denote by $ab(R)$ the sum of ranks of non-cyclic centralizes in R minus the number of such centralizers".

In our text (definitions 1.15 and 3.2 in [Se4]) it appears as: We set the complexity of the resolution $Res(t,a)$, denoted $Cmplx(Res(t,a))$, to be:

$$Cmplx(Res(t,a)) \;=\; (rk(Res(t,a)),$$

$$(genus(S_1), |\chi(S_1)|), \dots, (genus(S_m), |\chi(S_m)|),\; Abrk(Res(t,a))).$$

We should note that rk and $Abrk$ in our notation is the same as k and $ab(R)$ in the authors' notation. The rank $r(R_{j,i})$ in the authors' notation, is taken care of automatically by our taut resolutions (it is part of the definition). The authors have dropped the Euler characteristic $|\chi(S_i)|$, but they didn't notice that this is wrong, i.e., their following statements are true with our original definition but false with their modification.

Also, note that there is no "canonical" definition of complexity, i.e., one could have defined it in various other ways and still can get terminating procedures. Still, the authors have come up with a highly similar definition. Needless to say, no form of complexity (characteristic) appears in their pre-existing text.

(2) Lemma 7 on page 17 of the authors, which is a key point in the construction of the procedure is:

"Either the natural image $G_{(p)}$ of G on some level p of sequence c is a proper quotient of G or it can be replaced by a finite number of proper quotients."

Part (ii) of proposition 4.3 in [Se4] is:

"Let $Q_{term}(y,a)$ be the image of $Rlim(y,a)$ in the terminal (rigid or solid) multi-graded limit group of

$$MGQRes(s,z,y,Base^1_{2,1},\dots,Base^1_{2,v_1},a).$$

If the limit group $Rlim(y,a)$ is not the (coefficient) free group $F_k = < a >$, then the terminal limit group of the multi-graded resolution

$$MGQRes(s,z,y,Base^1_{2,1},\dots,Base^1_{2,v_1},a)$$

can be replaced by a collection of finitely many terminal multi-graded limit groups in which $Q_{term}(y,a)$ is a proper quotient of $Rlim(y,a)$."

Again, nothing similar to this lemma appears in the pre-existing text of the authors. It even doesn't appear in their note that is supposed to answer the counter-example to theorem 5 in their pre-existing text. We should note that the whole structure of the procedure presented in [Se4], and the one that is now presented in the new text of the authors are based on this lemma, and it is the real key for obtaining a terminating procedure for the validation of an AE sentence. Of course, this (among other things) explains why the procedure presented in the new text of the authors is completely different from the one presented in the pre-existing text (that was a much more general one, and usually does not terminate).

§4. Elementary theory of free nonabelian groups - May 21st 2001

Following the answer to the counter-example and the first revisions of the papers of KM, Eliyahu Rips asked KM to keep and post the various versions of their papers on the web. Indeed, Kharlampovich posted the original versions and the revisions on her webpage.

However, following my note to Zelmanov about the changes and the similarities to my own work (the note appears in the previous two sections), KM started to revise the original versions of their papers and post them as (fake) original versions. The sequel is a report on a revised version of paper 3 of KM, that was posted (on Kharlampovich's webpage) as the authentic original version (on May 21st 2001). The fake original version was 16 pages, 2 pages longer than the authentic original version.

(1) Corollary 1 on page 10 is new. Note the similarity between the notion "tight" that is introduced just before the lemma and used in it, and the notion "taut" resolution and diagram as appears in section 2 of [Se4], and used repeatedly and essentially in [Se4] and [Se5].
(2) The two lines between Corollary 1 and theorem 5 on page 10 are new. There was neither a discussion of decompositions relative to a subgroup, nor a relative JSJ decomposition in the original work of KM. My note to Zelmanov, prior to the change in this original version, indicated specifically the lack of such a discussion and its central role in the new revisions of paper no. 3 of the authors (that probably explains why the new (fake) "original" version was revised accordingly).
(3) The proof of theorem 5 (the main and crucial false statement) was changed.
(4) Page 13 from line -15 and on, is all new. In particular, the authors inserted the notion of splittings of a group (H) relative to a subgroup (R) that has not existed before, and is used extensively in my papers, in particular in the procedure that is described in section 4 of [Se4].
(5) Page 14 from the beginning until line -7 is completely new. At line -7 it continues as it was in the authentic version after line -15 in page 13.

Finally, we should point out that after the modifications in what was supposed to be authentic versions, I've pointed these changes in a note to Mark Sapir. Following my note to Sapir, KM removed the fake original versions, and since then the link to the authentic versions of KM papers became idle, i.e., the authentic versions didn't exist at all on Kharlampovich's webpage.

§5. Elementary theory of free nonabelian groups - July 22nd 2001

New versions of the paper with the above title appeared on June 24th, and July 22nd. We describe the changes in the version of July 22nd.

(1) Section 1 on algorithmic claims is mostly new (didn't exist in the previous version). We should note that we had not addressed algorithmic issues in our work.
(2) Lemma 8 is completely new - this principle was neither used nor appeared in previous versions. It is a simple but essential (geometric) observation that is used repeatedly in our work. The lemma is identical to lemma 1.4 in [Se10].
(3) Nothing like definition 7 appears in the original papers of KM. Indeed the authors never looked at decompositions, nor at free products relative to a subgroup (this is all related to the study of system of equations with parameters that is central in our work, but was missing and not studied by KM).
(4) Definition 8 is new. Its statement is wrong. It is supposed to be (and used later as) our notion of multi-graded decomposition that appears in section 12 in [Se7] (cf. theorem 12.2 in [Se7]).
(5) Theorem 7 and corollary 1 are new. Indeed, their statement uses definition 7 that is new as well (once again, we note that theorem 7 and corollary 1 are new algorithmic claims and we have not dealt with algorithmic questions).
(6) Definition 11 (page 9) is new. The authors never considered splittings or automorphism groups relative to a subgroup.
(7) Definition 12 (page 10) is new. This time definition 8 from page 7 (in the same version) is corrected. It's identical with our notion of a multi-graded free decomposition (cf. theorem 12.2 in [Se7]).
(8) Definition 13 (page 10) is new. It is supposed to describe multi-graded splittings (section 12 in [Se7]). However, for fully residually free groups, the definition, as stated, makes no sense (it can be used only after the multi-graded modular group (in our terminology) is defined).
(9) Theorem 8 is new. Again, definition 13 (sufficient splitting) doesn't make sense, so as it is, theorem 8 doesn't mean much.
(10) Corollary 4 on page 13 is new. This is meant to be theorems 2.3 and 2.7 from [Se3]. Note that this central theorem, that is new in the papers of KM, and takes the entire sections 1-2 in [Se3], appears with a rather short proof, that doesn't really have much connection to the statement of the "corollary". We stress that the statement of the corollary gives a bound that depends only on the pair of a fully residually free group and its subgroup, whereas the maximum that one can obtain from the previous arguments of KM is a bound that depends on the lengths of the values of a fixed generating set of the given subgroup. This is much weaker than what is needed, and requires a new argument (that will appear, not surprisingly, in one of the forthcoming versions after some false attempts for an original argument).
(11) Note the notation $T_{sol}(\Omega, X)$ on line -9 in page 13 (first line of the proof of

corollary 4). The notion *sol* doesn't appear in any other place in the text of KM. Could that be connected to our notion of a *solid* limit group, which is precisely what the authors study in these lines, and to which corollary 4 actually refers?

(12) Remark 2 on page 18 is new. It is a modification of our notion of a taut decomposition or resolution (definition 2.2 in [Se4]).

(13) Parts 1-6, lines 10-19 on page 21. These are all new. The formulation is far from precise, but it is supposed to describe our notion of well-separated resolutions (definition 2.2 in [Se4]).

(14) The tree $T_{AE}(G)$ has not appeared at all in the original papers of KM. Indeed, there was no need in it because of (the false) theorem 5 in [KM5] (hence, the whole section 5 in the paper is new). The construction that the authors are describing is identical with our iterative procedure for validation of a sentence, as appears in section 4 of [Se4].

(15) Lemma 12 on page 22 is new. It is identical with lemma 4.3 in [Se4].

(16) We should note that pages 21-24 are direct modifications of the procedure that is described in detail in section 4 of [Se4]. We stress that our procedure is by no means canonical, which means that surely other procedures could have been used for the same purpose. The description of that procedure by KM misses many essential details. For example, in line -8 KM talk about a resolution that is "generated" (their terminology) by a subgroup from a resolution of an ambient group. What they are referring to is the induced resolution, which is not an immediate object, and is defined using an iterative procedure, to which we devoted section 3 in [Se4]. Also, note that the construction of the anvil (in our terminology), which is the main object for this iterative process (together with the developing resolution), is not described at all by KM. So their description of the iterative procedure that appears in section 4 of [Se4] misses two of the main objects that are constructed along it: the induced resolution, and the anvil. We'll see if and how this will change in the forthcoming versions.

(17) Theorem 11 on page 24 is identical to theorem 4.12 in [Se4].

(18) Lemma 13 on page 24 is supposed to be a restatement of proposition 4.2 in [Se4].

(19) Section 9 pages 38-44. Note that the authors are still not aware that the iterative procedure that we've used for the analysis of AE sentences (section 4 in [Se4]), is not sufficient for analyzing definable sets and sentences with more quantifiers. In particular, they quote the finiteness of their tree T_{AE} (page 43 line -4), as the result that guarantees termination for an arbitrary formula. This is false, and in order to fix that one needs to borrow the tools of [Se5]. This will be done in the forthcoming versions of this paper.

§6. Elementary theory of free nonabelian groups - September 3rd, October 12th, and October 23rd 2001

The previous version of KM 3rd paper (of July 22nd) has 43 pages. The 3 new versions have 46,43 and 44 pages. We will only indicate the changes in these 3 versions, in comparison with the previous version of July 22nd.

(1) In the version of September 3rd 2001, the main change is in section 3 that

KM call "some auxiliary results". There is not much connection between the current content of this section and its main goal (corollary 3), and its original content in KM4 (we recall that corollary 3 and the notions it uses didn't appear at all in KM original papers. Corollary 3 is basically theorems 2.3 and 2.7 in [Se3]).

(2) The main goal of the change is to provide a proof of corollary 3. As this argument is going to change several times in the next versions, before it converges into the argument that appears in the first two sections of [Se3], we won't get into the details of the current (new) argument.

(3) Note that at this point KM still do not realize the big difference between sentences and formulas with 2 quantifiers, and those with more than 2 quantifiers. Hence, they claim to use their tree T_{AE}, that is borrowed from section 4 of [Se4] (for the analysis of sentences with 2 quantifiers), to analyze general sentences. This is a crucial mistake.

We continue to the version of KM's third paper of October 12th 2001. This version has 44 pages, shorter in two pages than the previous one of September 3rd. We only indicate the main differences between these two versions (October 12th and September 3rd) - the pages and numbers of claims refer to the version of October 12th 2001.

(4) On page 10 the last two lines are new. This is precisely part (i) in definition 10.3 in [Se7]. Note that the notion that is discussed in these two lines is parallel to flexible homomorphisms (in our terminology), and has not existed at all in the original papers of KM ([KM2]-[KM5]). It was added to previous revised versions, and although part (i) of the definition was missing, the corresponding class of (flexible) homomorphisms was used precisely as in [Se7]. In the current version, KM realized that their definition should be fixed to match with definition 10.3 in [Se7].

(5) Page 14 line -16. The author refers to shortest homomorphisms in their (graded) class. This concept doesn't appear at all in KM original papers. It agrees with the analysis of solid and strictly solid homomorphisms (see definition 10.3 in [Se7]).

(6) Page 14 lines -7 to -1. This is supposed to prove corollary 3 (which has not appeared at all in KM original papers, and is in fact theorems 2.3 and 2.7 in [Se3]). However, KM do not notice that the type of argument that they are trying to use (and which is similar to the arguments that are used in [Se1]) can give only finiteness of the number of exceptional (families of) homomorphisms and not boundedness as theorems 2.3 and 2.7 in [Se3] are claiming (as well as KM corollary 3 in this paper). This will change in forthcoming versions.

(7) Page 18 lines -25 to -1. This describes the construction of the graded Makanin-Razborov diagram as it appears in section 10 in [Se7]. Nothing like that appears nor is hinted at in any of the original papers of KM. Indeed, none of the notions that are required for the construction appears in these original papers.

On October 23rd 2001 KM posted a new version of their 3rd paper. The main difference in comparison with the version of October 12th 2001 is in the statement of theorem 12 (page 28) and its proof. As these changes are going to be modified in the forthcoming versions, we prefer not to get into their details.

§7. Elementary theory of free nonabelian groups - November 28th, and December 11th 2001

The previous version of KM 3rd paper (from October 23rd) has 43 pages. The 2 new versions have 42 and 43 pages. We will only indicate the changes in these 2 versions, in comparison with the previous version of October 23rd.

(1) The main difference between the version of November 28th and the previous one, of October 23rd, is in studying equations with parameters, namely the proof of corollary 3 on pages 13-15.

Note that KM have never studied systems of equations with parameters in their original papers ([KM3], [KM4], [KM5]), and corollary 3 is supposed to be a restatement of theorems 2.3 and 2.7 in [Se3]. These theorems claim for a global bound on the number of exceptional families of homomorphisms of such systems (these exceptional families of homomorphisms are defined in [Se1] and were never mentioned in KM original work).

In the current version KM probably noticed that the previous arguments that they have tried to use in order to prove their corollary 3 (i.e., theorems 2.3 and 2.7 in [Se3]) could not give a bound that depends only on the system, but at most a bound that depends on the lengths of the values of the parameters.

Therefore, in this version, KM are trying to imitate the argument that is used to prove theorems 2.3 and 2.7 in [Se3]. In particular, it is the first time that lengths of values of variables, and comparison between these lengths are mentioned by the authors, and the need to pass to a subsequence from a sequence that contradicts the statement of the theorem. This type of reasoning is foreign to the Makanin-Razborov elimination process, and is very natural when one uses R-trees and Gromov-Hausdorff convergence (notions like infinitely small on line 12 in page 14, short and long variables on line 13, and suitable subsequence on line 14 in page 14 are examples). Furthermore, fixing one solution in a family and looking at its perturbations (that the authors denote by the variables z in lines 7-8 on page 14 and in the sequel) is precisely the tool that is used in the proof of theorems 2.3 and 2.7. The notion of $infinitely$ *small* subsequence in line 12 of page 14 is precisely our *tame* subsequence as appears in definition 2.4 in [Se9] ([Se9] is the revised version of [Se3]).

These concepts have no hints neither in the original papers of KM nor in their previous versions. Of course, what they currently write is very preliminary, and will be revised (and get even closer to the arguments in [Se3]) in the forthcoming versions.

(2) The statement of theorem 9 appeared in the previous version of October 23rd, but it is very different from the statement of proposition 6 on pages 27-28 in [KM4] (the original work of the authors).

(3) The version of December 11th differs from the version of November 28th only in the last section, namely the analysis of general sentences. We won't get into a detailed description of the changes, as this part is going to be changed drastically in the forthcoming versions. Let us just note that at this stage KM still use the procedure that appears in section 4 of [Se4]

(their construction of the tree T_{AE} on pages 21-27 that has no hints in their original version), to analyze general sentences (see line 1 on page 42). This procedure is definitely insufficient for analyzing general sentences, and only later the authors will realize that they need to use techniques that appear in [Se5].

§8. Elementary theory of free nonabelian groups - January 6th, February 6th, February 18th, and February 20th 2002

The previous version of KM 3rd paper (from December 11th 2001) has 43 pages. The new versions have 43, 41, 41 and 41 pages. We will only indicate the changes in the new versions, in comparison with the previous version of December 11th 2001.

(1) The changes in the version of January 6th 2002 are in section 9, in which the authors analyze general sentences. These changes start on page 40 line -6. We already noted that at this stage, as in the previous versions so far, the authors are not aware that there is a big difference between sentences with 2 quantifiers, and sentences with more than 2 quantifiers. They are still trying to apply the procedure that appears in section 4 of [Se4] (that they encoded as the construction of the tree T_{AE} in section 5) to analyze general sentences (with more than 2 quantifiers). This is going to change in forthcoming versions, where they would realize that they would better borrow notions, procedures, and arguments from [Se5]. Hence, we don't see much point in getting into the changes in the current version in detail.

(2) The version of February 6th is shorter (41 pages), and differs from the version of January 6th, only in section 3 (Some auxiliary results), pages 11-13 (in the new version).

In the current version, theorem 9 of the previous version disappeared, and corollary 3 from previous versions is the new theorem 9. Now, the formulation of theorem 9 is precisely a reformulation of theorems 2.3 and 2.7 from [Se3], and has no hints in the authors' original papers ([KM4]-[KM6]). In particular, the current section, titled "Some auxiliary results", has nothing to do with the section with the same name in [KM5] (the authors kept the title, but completely changed the content).

The arguments that were used to prove corollary 3 in the previous versions got closer to the proof of theorem 2.3 in [Se3]. In the current version, the sketch of the argument that the authors present gets much closer to the proof of theorems 2.3 and 2.7 in [Se3], and, of course, nothing close to that line of reasoning appears in the authors' original papers (it is foreign to the Makanin-Razborov elimination processes techniques).

(3) The notions *short* and *long* in lines 9 and 12 on page 12 are precisely the notions of *degenerate* and *uncovered segments* that appear after definition 2.4 in [Se9]. the notion "infinitely small" in line 9, and the need to pass to a subsequence is precisely the Gromov-Hausdorff subconvergence that is used in the proof of theorem 2.3 in [Se3].

(4) The sentence "Remove all such subintervals" in line -16 is precisely the "uncoverings" that are performed for tame subsequences in the proof of theorem 2.3 in [Se3] (cf. the paragraph after definition 2.4 in [Se9], which is the revised version of [Se3]).

(5) The notions "second order long" and "kth order long" in lines -10 and -14 on page 12, are precisely the sequence of uncovered segments, that are obtained from tame sequences, as it appears in the paragraph after definition 2.4 in [Se9].

(6) Line -9 and -8 is precisely our notion of *perturbed* subsequence (2 paragraphs after definition 2.4 in [Se9]).

(7) The rest of the argument, from line -4 on page 12 until line 3 on page 13, is exactly (a sketch of) the argument that is used in proving theorems 2.3 and 2.7 in [Se3]. The current description of the authors is missing and not precise, but this will be somewhat improved in the next versions.

(8) Line -10 on page 11, "We obtain several terminal generalized equations with intervals labeled by generators w_t of H" is supposed to be a restatement of theorems 1.2 and 1.7 in [Se9] (of course, there is no hint of anything close to that in the authors' original papers). However, it is not at all clear why the argument that the authors present actually proves this statement with bounds that depend only on the system of equations, and not on the lengths of particular solutions (as the argument in their previous versions could give, for other statements).

(9) The version of February 18th was changed only in the last 3 paragraphs on page 38, and the first 3 paragraphs on page 39. Both are in the last section that analyzes general sentences. This is going to change considerably in the forthcoming versions, so we won't list the changes in detail. We note that the authors still do not realize at this point that there is a major difference between analyzing sentences with 2 quantifiers and general sentences. In particular, the procedure from section 4 of [Se4] (their construction of the tree T_{AE} in section 5 pages 19-25) is not sufficient for analyzing general sentences, and in the forthcoming versions they'll start borrowing notions, claims and arguments from [Se5] for that purpose.

(10) The version of February 20th differs from the version of February 18th in the same place where the changes in the version of February 18th were made, that is in the second paragraph of page 39 (the analysis of general sentences). The authors probably realize that their difficulties are with equations with parameters (their theorem 9 in section 3), and in analyzing general sentences.

Also, note that in this version the authors introduce (for the first time) solutions that are called *tame* (line 12 on page 39), which happens to be the exact notion (*tame*) that we used for the subsequences that are used to prove theorem 2.3 in [Se3] (their theorem 9), which they revise in two of the last versions (on January 6th and February 6th).

§9. Elementary theory of free nonabelian groups - March 8th, March 10th, and March 20th 2002

The previous version of KM 3rd paper (of February 20th 2002) has 41 pages. The new versions have 41, 41, and 42 pages. We will only indicate the changes in the new versions, in comparison with the previous version of February 20th 2002.

(1) The version of March 8th 2002 is very similar to that of February 20th 2002. The only differences are in the first 3 paragraphs on page 39, that are part

of the analysis of general sentences. At this point, the authors still don't quite understand the big difference between the analysis of sentences with 2 quantifiers, and sentences with larger number of quantifiers. This will change in the forthcoming versions.

(2) The version of March 10th 2002 was revised in the same place as the previous version (pages 38-39). The authors begin to realize that there is a difference between sentences with 2 quantifiers and general sentences. Since this understanding expands a lot in the next version, we won't get into the details of this version.

(3) The version of March 20th was changed in pages 38-39, the same pages as in the two previous versions, but here the changes are fundamental and are going to expand considerably in the next versions. In particular, this is the first version in which the authors realize that there is a major difference between sentences with 2 quantifiers and sentences with more than 2 quantifiers. In other words, for the first time they start to borrow concepts from [Se5] (that they haven't done earlier), i.e., from our sieve procedure.

(4) Page 39 lines 15-16. The notion "formula solution" that appears in this line is precisely *formal solution* that is used extensively in our papers (e.g., the title of section 1 in [Se2], and many theorems in this section (and paper) like theorems 1.1, 1.2 and 1.18). KM used a similar object in their paper [KM4] (their implicit function theorem), but never used this notion elsewhere (even in this paper it appears only once in these lines). We should note that our sieve procedure (the main part of our work, and what KM started to borrow from this version on), uses the construction of formal solutions (from section 1 of [Se2]) quite extensively. As an anecdote, we usually express formal solutions as a word in the elements z, precisely as the authors do here. Finally, note that the authors removed it in their next version...

(5) Lines 6-8 in page 39 are new (and could not been hinted at in KM original work). They are precisely (a very brief summary of) parts (1)-(3) of the general step of the iterative procedure that appears in section 4 of [Se4], and parts (1)-(3) in the general step of the sieve procedure (Se12]). These are indeed "easier" cases, as the authors describe it in line 8 of page 39, but they still require further arguments and constructions - i.e., these cases are not trivial, and there is a difference between the cases of parts (1), (2) and (3), the treatment of these cases is different and requires non-trivial arguments and constructions, none of which are mentioned or even hinted at by the authors.

(6) Definition 19 is new. The notation is typographically similar to what we used in the sieve procedure, though the definition here is different.

(7) Definition 20 on page 39 is new.

(8) Page 39 line -11: "The main difficulty is that in the process some solutions of finite type can become solutions of infinite type, and new solutions of finite type appear". This is completely new (no hints in previous versions, definitely not in the original work). This is indeed the main difficulty in the sieve procedure (that analyzes EAE sets). Compare definition 1.28 in [Se11], and the lines before it: "This next bundle we construct collects all the generic PS statements for which some of the limit groups WPHG have additional rigid or strictly solid specializations... that are not specified by

the given generic PS statements".

§10. Elementary theory of free nonabelian groups - April 15th 2003

The previous version of KM 3rd paper (of March 20th 2002) has 41 pages. The new version has 47 pages. We should note that a month after posting the previous version (from March 20th 2002), KM removed that version from the web, and their 3rd paper (Elementary theory of non-abelian free groups) was not available at all for about a year (until a new version appeared on April 15th 2003). Note that this is precisely the version in which the authors noticed that analysis of sentences with more than 2 quantifiers requires much more than what they presumed so far, and larger and larger parts from [Se5] (quantifier elimination and the sieve procedure) found their way into their papers.

The new version of April 15th is a real jump in the approach and in the sequence of revisions of KM. We will only indicate the changes in the new version, in comparison with the previous version of March 20th 2002.

(1) The notion "formula solution" that appeared in lines 15-16 on page 39 in the version of March 20th 2002 (see part (4) in the previous section), and is identical to our "formal solution", and appears only at this place in the authors' text (though they have different terms for the same notion in other parts of their text), has been removed. It doesn't appear anywhere else in the new version.
(2) Section 1 on algorithms was moved to section 4.
(3) Theorem 4 on page 9 on the boundedness of exceptional families of solutions (theorems 2.3 and 2.7 in [Se3]), that was not hinted at (and the notions to state it didn't exist) in the original papers of the authors, has now a considerably extended proof, that is very similar to the proofs of theorem 2.3 and 2.7 in [Se3].

    We have already indicated that using a metric and passing to a subsequence are not natural for the Makanin-Razborov elimination process. The division of variables into short-long pieces (e.g., lines -18, -20,-21 on page 10), is precisely our "uncovered" and "degenerate" pieces (definition 2.4 and the paragraph after it in [Se9]).
(4) Line -4 on page 10: "The maximal difference between beginnings (and ends) of y^{j_1} and y^{j_2} for $x_i \in X_1$ is short. This difference is what we call in our papers *fluctuation*, and the maximal difference is denoted in our paper $Mfluct$ (the paragraph before definition 2.4 in [Se9]). The condition or property that the maximal difference is small is precisely the definition of a tame subsequence (definition 2.4 in [Se9]).
(5) Line -2 on page 10: "Remove all such subintervals". These are precisely our "uncovered" segments (second paragraph after definition 2.4 in [Se9]).
(6) Line 4 page 11: "short variables, variables from Z and periods". This is precisely the definition of a tame subsequence in the presence of what we call pseudo periods (definition 2.6 and the second and third paragraphs after it in [Se9]).
(7) Line 11 on page 11: "variables from Z are infinitely small and grow as...". This is precisely the notion of *perturbed sequnce* in the second paragraph after definition 2.4 in [Se9].

(8) Third paragraph in page 11. This is the first time that the authors apply the JSJ decomposition and its properties to prove the theorem (theorem 4). In the last revisions the theorem showed up, but the JSJ never appeared in the argument. Indeed, the proofs of theorems 2.3 and 2.7 in [Se3] heavily rely on the JSJ, and in particular that the exceptional homomorphisms that are considered are minimal with respect to the modular group (that is encoded by the JSJ). This is now used by the authors as well: "This contradicts the property... minimal solutions" (cf. the proof of proposition 2.11 in [Se9]).

(9) In the last 3 lines (-14 to -12), the authors actually found a shortcut in the last parts of the proofs of theorems 2.3 and 2.7 in [Se3]. Their last two sentences: "This could only happen if the image of... In this case the sequence satisfies a proper equations, and, therefore, one of the equations from R" is a nice observation that we had not noticed, and can actually make our argument slightly shorter.

(10) The notion "maximal standard quotient" (line -5 on page 7) is new (never appeared in the original work). It is precisely our notion of "maximal shortening quotient" (see lemma 5.5 in [Se1] and the two paragraphs after definition 10.1 in [Se1]).

(11) Page 24 lines 5-7: "Construct fundamental sequences... with the top part being extracted from the top part of $c^{(2)}$ (above level k) and the bottom part being f_i. This is precisely the *developing resolution* in section 4 of [Se4] (see the 3 paragraphs after proposition 4.4. in [Se4]). Note that to construct the developing resolution we use the induced resolution (for its construction we devoted the whole section 3 in [Se4] - 11 pages), and the authors only refer to it as "with the top part being extracted from the top part of $c^{(2)}$.

(12) The notion "block-NTQ" group that appears in line 10 on page 23 is new (never appeared nor hinted at in the original work of the authors. It requires lemma 13 that never appeared in the original work). This is precisely the structure of the *anvil* that is central in our iterative procedure for the analysis of sentences and formulas (see the third paragraph after definition 4.4 in [Se4]). KM construction of the anvil appears in page 23 lines 14-15: "Consider a fundamental sequence c_3 obtained by pasting a sequence corresponding to b_2 to the bottom group $G_{(p)}$ of c_2".

(13) Page 27 last paragraph (lines -13 until the end). This is now identical with the proof of theorem 4.12 in [Se4] (no hint of that theorem, nor argument, nor the objects that are involved in it, in the original papers of KM).

(14) The section "projective images" on pages 30-35 has been changed considerably. There is almost no connection between the current content of this section, and the section "projective images" in the original papers of the authors [KM4]. The current version borrows notions, constructions, and arguments from the sieve procedure [Se5] (or its revised version [Se12]), but it is still rather sketchy. Note that all these were not hinted at anywhere in the original papers, but some of these notions started to appear in some of the previous versions (but not to that extent).

"block-NTQ" in line -5 on page 30 is our *anvil*, the central object that is used in our iterative procedures for validation of sentences and formulas (see paragraph 3 after proposition 4.4 in [Se4]). Note that the notion and the construction of "block NTQ" do not appear anywhere in the authors' original papers, in particular, not in connection with "projective images".

(15) The construction from line -3 on page 30 until line 2 on page 32 is supposed to be the construction of a *core* resolution as appears in section 4 of [Se11]. The construction that they present is rather sketchy, incorrect and missing, but we'll indicate similarities with the construction in section 4 of [Se11] ([Se11 is part of a revised version of [Se5]).

Part 2, line -2 on page 30. "those QH subgroups $Q_1, \dots, Q_\ell$ that don't have sufficient splittings in T_1" is the way the authors describe our *surviving surfaces* (definition 1.8 in [Se11]). The description of surviving surfaces given by the authors is wrong. What they are trying to do is to construct resolutions that will satisfy the conclusions of theorem 4.13 in [Se4].

(16) Page 32 line 4" the "enveloping" NTQ system is a new notion (no hint in the original papers, and we haven't found it even in the previous version of March 2002). It is supposed to be our *sculpted* resolution, as appears, for example, in the end of part (4) of the first step of the sieve procedure in [Se12] (a revision of the second part of [Se5]).

(17) In our sieve procedure there are two kinds of envelopes, sculpted resolutions that are embedded in penetrated sculpted resolutions. Without this nested couple of sculpted resolutions, it is impossible to apply the argument that proves the termination of the sieve procedure.

KM don't mention at all (an analogue of) our penetrated sculpted resolutions (definition 4.20 in [Se11]). Still, they sketch precisely the same argument as ours for the termination of their procedure. Needless to say, our argument can not be applied, and it doesn't make any sense, without the nested couple of "envelopes", sculpted and penetrated sculpted resolutions.

(18) Page 32 lines 10-11: "We can do it by levels from bottom to top... and repeat iteratively as many times as possible. We add also those QH subgroups...We denote ... by $N_2 = 1$". This is a very sketchy description of the procedure for the construction of a core resolution as it appears in section 4 of [Se11] (e.g., cf. the second paragraph in the proof of lemma 4.10).

In their sketch, the authors do not seem to mention (hence, to care) that the construction of the obtained resolution will be *firm* (definition 4.1 in [Se11]), and in particular that it will have properties (i) and (ii) of that definition. These are essential properties of core resolutions, that are crucial in proving the termination of the sieve procedure, a proof that the authors themselves are trying to imitate later in the paper. Note that these required properties (parts (i) and (ii) of definition 4.1) are precisely what make the construction of the core resolution (in section 4 of [Se4]) a lot more complicated.

(19) Page 32 lines -21 to page 33 line 17: this is supposed to be a sketch of part (7) in the general step of the sieve procedure [Se12].

(20) Definition 21 is new. It has no connection with the projective image that is defined by the authors in their original papers [KM4]. In particular, their original projective images were not block NTQ (but rather just NTQ), and they had no "envelopes" (our sculpted resolutions).

(21) Theorem 13 on page 33 doesn't exactly make sense as the free variables take values in NTQ groups that contain parts of the QH and abelian groups from which the projective image is composed. The authors are trying to put together their current "projective image" with the theorem that they

had for (different) "projective images" in their original papers [KM4].

(22) Page 38 lines 11-13: this is the first time that the authors claim a form of quantifier elimination (at least as their strategy to prove Tarski's problem). There was no hint to anything like that in their original papers. Also, note that as they are analyzing now sentences with more than 2 quantifiers, from what they write one can deduce that the reduction is to boolean combination of sentences with two quantifiers - precisely our main result (theorem 1 in [Se13]).

(23) Definition 22 on page 40 is new. It is very similar to our notion of valid PS statements - see definition 1.23 in [Se11]. Definition 23 is also new. Definitions 22 and 23 define our PS limit groups and resolutions - see the 4th paragraph after definition 1.23 in [Se11].

(24) The notion *width* in definition 22 is new. Note that we use the same notion, width, in the same place (the sieve procedure) but for a different purpose - the number of sculpted resolutions (see the beginning of part (7) of the general step of the sieve procedure in [Se12]).

(25) Lemma 16 on page 41 is new. It summarizes (sketches) definitions 1.25-1.28 in [Se11].

(26) The paragraph after lemma 16, line -12 to -8 on page 41, is new. The additions are precisely definitions 1.29 and 1.30 in [Se11].

(27) Lemma 17 on page 42 is new. This is precisely theorem 27 in [Se12] (see also theorem 5.17 in [Se5] which is the non-revised version, was available from 2001, and is even closer to the statement of lemma 17 in the current version). This is the crucial theorem in proving that the sieve procedure for quantifier elimination terminates after finitely many steps.

In the statement of lemma 17, KM use the notion *width* precisely as we do in theorem 27 in [Se12]. Note that in the previous section KM refer to our *width* as "type" (e.g., in definition 22), and they use the notion "width" for a completely different thing. Somehow, in the most crucial place, they kept the claim literally identical...

(28) The paragraph after lemma 17 on page 42 is new. The argument is supposed to summarize the strategy to the proof of theorem 22 in [Se12]. in line -18 on page 42, it says: "are minimal with respect to different free groups". This is true in our sieve procedure, since our core resolutions are firm (definition 4.1 in [Se11]). However, in the new definition of "projective images" (definition 21 on page 33, and the paragraphs before it), the authors don't mention that at all (note that adding the firm condition (definition 4.1 in [Se11]) makes the construction of the core resolutions much more complicated).

(29) Page 42 line -15 to -16: "we can adjust the proof of Theorem 4 for this situation". This is new. This is the key for the termination of the sieve procedure (cf. the first paragraph in the proof of theorem 27 in [Se12]).

(30) The proof of lemma 17 on page 42 is very sketchy and missing (cf. the proof of theorem 27 in [Se12]). In particular, the authors do not refer to the fact that equivalence relations on exceptional (finite type) solutions vary from one envelope (sculpted resolution) to another. They do not notice it here, but they will refer to it in forthcoming versions.

(31) The procedure that KM describe (is supposed to) prove quantifier elimination to Boolean algebra of AE formulas (see their statements in the last paragraph on page 43). However, although quantifier elimination is a major

result (this is the main theorem in our work), and KM believe that they wrote an argument that proves it, they never state quantifier elimination as a theorem, and never mentioned in their introductions that they had actually proved it, and that their strategy was (now) based on it. Of course, no form, and no hint of quantifier elimination (not even as a conjecture) appears in KM original papers.

§11. Elementary theory of free nonabelian groups - July 6th, and July 20th 2003

The previous version of KM 3rd paper (of April 15th 2003) has 47 pages. The new versions have 47, 48 and 48 pages. As in the previous sections, we will only indicate the changes in the new versions, in comparison with the previous version of April 15th 2003.

(1) There are changes in the version of July 6th 2003 but they are relatively minor.

(2) There are changes in the version of July 20th. Some of our remarks on this version have roots in the version of April 15th (but they are new in that version, and have no hint in the original papers).

Page 31 line -9 to page 32 line 5. This is part 3) in the procedure that the authors present (that has no hint in their original work), which is precisely part (2) in the first step of the sieve procedure.

Page 31 line -14: "those QH subgroups $Q_1, \dots, Q_\ell$ that don't have sufficient splitting". The authors refer to our surviving surfaces (definition 1.8 in [Se11]). However, their definition is wrong, and is not compatible with the continuation (which agrees with our notions, of course). The difference is that the surfaces Q_i can have a non-trivial splitting along the resolution (fundamental sequence). However, a surviving surface is a surface that appears in some level of the resolution without an essential splitting, i.e., it is isomorphic to a QH vertex group is some level of the resolution. Then the resolution can be modified so that these surfaces are pushed to the bottom level. Somehow, the authors missed this argument, and therefore, missed the definition of surviving surfaces (that they need to use, and without knowing, actually use).

(3) Line 21 on page 32: "Consider a fundamental sequence obtained by extracting a fundamental sequence... and pasting to it a fundamental sequence for $\bar{H}$." This is precisely the construction of the developing resolution in part (2) of the first step of the sieve procedure. Note that the authors have never defined nor explained what is "extracting a fundamental sequence" (which appears also in their construction of the tree T_{AE}, - that is in our iterative procedure for validation of an AE sentence).

We should note that in our papers there is a significant (crucial) difference between the two extractions - in sentences with 2 quantifiers, in comparison with the sieve procedure (more than 2 quantifiers). In the first case, it is the induced resolution (defined and constructed in section 3 of [Se4]). In the sieve procedure it is a framed resolution (definition 5 in [Se12]). This difference changes the meaning of generic points in the two constructed resolutions. For the authors there doesn't seem to be a difference between the two constructions...

(4) Part 4), line -17 on page 32 to line 2 on page 33: this is supposed to sketch the construction of the core resolution in section 4 of [Se11] (45 pages). Of course, what the authors describe in half a page is very sketchy. e.g., line -8 to -6: "If the dimension...into a larger system having smaller dimension" - no argument, but this is precisely what the construction of the core resolution does. lines -2 to -1: "We add also... than the subgroup in the intersection" - this is precisely our notion of *inefficient* QH groups (definition 4.4 in [Se11]).

"We add all the elements that conjugate different QH subgroups...into the same QH vertex group" (page 32 line -1 to page 33 line 1) - this is meant to be our notion of *reducing QH couple* (definition 4.6 in [Se11]). However, the authors forgot to mention that the (conjugate) QH vertex groups are assumed to be not of minimal rank... (see our definition). Of course, they meant what appears in our definition - otherwise what they wrote doesn't make sense.

Page 32 lines -5 to -4: "We first add all the QH subgroups ... and their intersection decreases the dimension". There is no explanation of what these QH subgroups are exactly. The authors probably refer to our *absorbed* QH subgroups, as they appear in definition 4.7 in [Se11]. However, the conditions in definition 4.7 for absorbed QH subgroups are far more detailed and technical. In particular, absorbed QH subgroups are always of minimal rank (no hint at that in the current version).

(5) One of the main properties of a core resolution (in comparison with induced resolution) is that it is a *firm resolution* (definition 4.1 in [Se11]). This is crucial for the termination of the sieve procedure (that the authors borrowed since the version of April 15th), and makes our construction of a core resolution (section 4 in [Se11]) much more complicated. However, the authors don't seem to mention or care about the *firm* condition (or property).

(6) Page 33 line -21 to page 34 line 5: this is a very sketchy description of the general step of the sieve procedure [Se12]. Note that our sieve procedure is not "canonical" in any way, and there are many "choices" that we made in defining it. Hence, there is no reason why another group that works on the problem will come up with exactly the same iterative procedure.

(7) Definition 19 on page 34: there is no connection between what the authors call "projective image" here and in their original paper [KM5]. Their current "projective image" is simply the *developing resolution* that is constructed in the general step of the sieve procedure in [Se12]. What they call in Definition 19 "type" is supposed to be our *width*, so their definition (of type) is sloppy and incorrect, because they haven't noticed that we use the *penetrated core resolutions* (and not only the core resolution) to construct the developing resolution, and they never mention these (penetrated) resolutions.

(8) Theorem 13 on page 34 or anything connected to it have never appeared in the authors' original work [KM5] (as the objects that are used in its statement are not even hinted at in the authors' original work). However, they state it in a form that may sound as if it is connected to theorem 3 in [KM5] (but there is no connection).

(9) Lemma 17 on page 43: in this crucial lemma, the authors still (as in previous versions) use the notion *width* as we use in a literally identical statement in theorem 27 in [Se12]. Note that the authors' analogue for our *width* is "type" (see definition 19 on page 34), whereas they use the notion *width* with a different meaning (e.g., in Definition 22, line -13 on page 41).

(10) The authors sketch a proof of lemma 17, that imitates the proof of theorem 27 in [Se12]. However, without their "projective images" (*core resolutions*) being firm subresolutions (definition 4.1 in [Se11]), and without *penetrated core subresolutions*, the argument doesn't make much sense.

Also, note that in order to prove lemma 17 the authors pass to a subsequence (line 13 on page 43). However, since they never defined *penetrated core resolutions*, and their notion of type (in lemma 17 on page 34) is not exactly our *width*, there is no reason to pass to a subsequence to get the property that the authors want ("fundamental sequences ... are embedded"). There is a need to pass to a subsequence (and we do it), exactly because one uses *carriers* and *penetrated core resolutions* (that are essential objects in our proof), but these don't exist in the current version of the authors...

(11) The proof of lemma 17 on page 43: the argument that was used to prove the theorem in the current version (theorem 2.3 and 2.7 in [Se3]) has to be modified in order to prove lemma 17. However, it seems that the authors are aware of some of the needed modifications, and not of other needed modifications. In particular, the modular groups that are associated with different "envelopes" (in the authors' borrowed terminology) are different, so it may be that solutions of infinite type (as they call it) differ just because of the difference in modular groups. This difference may arise since we deal with *framed* resolutions and not with genuine resolutions (definition 5 in [Se12) - but the authors never define a framed resolution, and because possible (infinite) nesting in abelian (edge and vertex) groups (cf. the proof of theorem 27 in [Se12]) - again, the authors seem to skip this difficulty.

§12. Elementary theory of free nonabelian groups - January 21st and April 24th 2004

The previous version of KM 3rd paper (of July 20th 2003) has 48 pages. The new versions have 50 and 73 pages. We will only indicate the changes in the new versions, in comparison with the previous version of July 20th 2003.

(1) The version of January 21st was changed only in the proof of lemma 5 on page 5. The proof is now much longer. The formulation of the lemma (which is still not precise), together with the last line on page 6, are now the analogue of lemma 1.4 in [Se10]. The authors were (essentially) using that expanded lemma in their previous versions without having its complete (necessary) statement (and it's still described along the proof and not in the statement of the lemma). Note that although the lemma is a basic (easy) tool that is used repeatedly in our work, nothing similar to that lemma neither appears nor is hinted at in the original papers of KM.

(2) The version of April 24th 2004 is considerably longer than the version of January 21st.

Lemma 7 on page 11 is a revised form of lemma 5 on page 5 in the previous version. Note that it has a new title: "Induced QH-vertex groups" (this notion has not appeared in any previous version). Recall that in section 3 in [Se4] we study a (geometric) resolution that a subgroup inherits from a given tower, and call it: *induced resolution*.

(3) Section 1.16 on page 18 is called "Minimal solutions and maximal standard quotients" - these are precisely our *shortest homomorphisms* and *maximal shortening quotients*. We note that these two notions started to appear in some of the previous versions but didn't exist in the original papers of the authors.

(4) Lemma 9 is wrong. It has exactly the same mistake as the one that I made in my original paper on the JSJ decomposition for hyperbolic groups. This mistake (in my paper) was discovered by Gilbert Levitt who also fixed it in [Le].

(5) Definition 16 on page 19 is precisely our notion of a *multi-graded solid* limit group (definition 12.4 in [Se1]). Nothing like that, not even the concept of a decomposition with respect to (one or) finitely many subgroups, is hinted at in the original papers of the authors.

(6) Section 2 "Finiteness theorems .. with no sufficient splittings" on page 19 is what was theorem 4 before (now it's theorem 6). These are the authors' restatements of theorems 2.3 and 2.7 in [Se3], and the theorem and the argument that is used for its proof are major tools in our approach to Tarski's problem. None of the content that appears here was hinted at in the original papers of the authors, not even the notions and the objects that are used for the (re)statement.

(7) In the proof of theorem 6 (starting on page 20), the authors keep referring the reader to their papers [13] and [18] (these are papers on their implicit function theorem and effective JSJ decomposition). However, these referrals are for revised versions of the paper [13] (the paper [18]) is new. There is no study (nor hints to) equations with (non-constant) parameters in the original papers of the authors [KM2]-[KM4]. Furthermore, sections 5.3 and 5.4 in [13] that the authors keep referring to (e.g., lines -19, -3 and -1) didn't exist in the original paper of the authors [KM2].

We already described in detail the similarities (or rather identities) between the current of of theorem 6 on pages 19-24, and the proof of theorem 2.7 in [Se3]. Again, the argument that we used to prove theorem 2.7 in [Se3] (their theorem 6) is not natural from the point of view of Makanin-Razborov elimination processes (as it uses lengths, passing to subsequences, and infinitesimals, and essentially Gromov-Hausdorff limits and real trees). Needless to say, no part of this argument is hinted at anywhere in the authors' original work, neither the notions nor the constructions that are involved in the argument.

We should note that the current version of the proof of theorem 6 is closer to the original (proofs of theorems 2.3 and 2.7 in [Se3]) than what existed in previous revisions, but there are still gaps and missing arguments. We summarized most of the similarities in comments on the previous revisions. We briefly mention the appearance of newly appeared similarities.

(8) The notion "period" that appears first on page 22 line -19 (and further in the argument) is precisely our notion of *pseudo-period* (definition 2.6 in

[Se9]).

(9) The "cancellation tree" on page 22 line 15 is precisely our *R-state* (definition 2.3 in [Se9]).

(10) Page 31 line 7 to line -5 together with page 34 line 1 to line -17. This is our definition of *well-separated* resolutions (definition 2.2 in [Se10]), and taut homomorphisms (definition 2.4 in [Se10]). No hint at anything like that in the original work of the authors.

(11) Section 3.6 line -3 on page 31 to line 15 on page 32. This is precisely the construction of our taut Makanin-Razborov diagram (proposition 2.5 in [Se10]). This construction has not appeared in any previous version of this paper (no hint in the original papers). Note that in order to construct it, the authors construct a locally finite diagram and use Konig's lemma (line 14 on page 32). This is our standard of arguing in such constructions (cf. e.g., proposition 2.5 in [Se10] or theorem 5.7 in [Se1]). This type of construction and reasoning is completely different from that of Makanin elimination processes that the authors use. Konig's lemma is never used in the authors' original papers, and appears here for the first time in all their sequence of revisions, to the best of our knowledge.

(12) Section 3.9 line -17 on page 34 to line 15 on page 35. This section is new. It is called "Induced NTQ systems". It is supposed to construct our *induced resolution* as it appears in section 3 of [Se10] (11 pages). The induced resolution is a central tool in our iterative procedures for analyzing sentences with 2 and more quantifiers, and as the authors borrowed these procedures in earlier versions, they used the induced resolution. However, in previous versions the induced resolution was never constructed and was referred to as a fundamental sequence "extracted" by a subgroup from an NTQ group.

The construction that the authors describe is supposed to imitate the construction that appears in section 3 of [Se10], but their description is completely wrong. In particular, our construction uses an iterative procedure that goes from top to bottom, possibly enlarges the subgroup, and continues again from top to bottom. The authors completely missed that iterative procedure and the construction they describe goes one time top to bottom. This doesn't make any sense. The output will not be an NTQ or a fundamental sequence in general. Needless to say, whenever the authors use their "induced NTQ systems" it is used as our *induced resolution*, although these are completely different objects.

(13) Line 4 to line 7 on page 35. "Increasing G_1 by a finite number of suitable elements from abelian vertex groups of $F_{R(S)}$ we join together ... from abelian vertex groups in $F_{R(S)}$". This sentence does not make any sense, and does not give any hint at what elements should be added. Could it be that the authors refer to what appears in part (ii) in the construction of the induced resolution in section 3 of [Se10]?

(14) Line 7 to line 15 on page 35. The authors say nothing about QH vertex groups. In other words, they completely "skipped" part (iii) in our construction of the induced resolution (the authors have a previous section on "induced QH vertex groups (lemma 7 on page 11), but they don't mention it in the section on induced NTQ systems, and in any way it's not a replacement for part (iii) of the construction in section 3 of [Se10]). Without this

part (or any other part), the construction that the authors describe doesn't make any sense.

(15) Page 35 lines 13 to 15. Without part (iii), and without repeating the procedure going from top to bottom iteratively, and proving that such a procedure actually terminates (as the group increases at each step of the procedure), what the authors describe does not make a sense, and can not be applied to anything in the sequel. Of course, when they'll refer to the induced resolution (their induced NTQ), they will actually use our induced resolution (as it appears in section 3 of [Se10]) with all its properties, as this is what is required for our iterative procedures for the analysis of sentences and formulas.

(16) Page 35 line 13: "Working separately with each G_i". To construct the induced resolution, one can not work separately with each factor G_i. It is true that when one goes from top to bottom in each step, the factors (that may change from one step to another) are treated separately. But to initialize the next step, before one starts going from top to bottom once again, the group must be considered as one group (and not each factor separately). Indeed, the whole factorization may change along the iterative procedure for the construction of the induced resolution, and this is analyzed in proving the termination of this procedure (see proposition 3.3 in [Se10]).

(17) Section 3.10 on page 35 is new (no hint neither in the original work of the authors, nor in the previous versions of their work). This is precisely definition 3.8 in [Se10].

(18) Definition 19 on page 36 is new (no hint in the authors' original work). It is precisely our *graded test sequences* as they are defined in definition 3.1 in [Se8].

(19) Page 39 line -16: "section 5: construction of a tree $T_{AE}(G)$". Such a section appears in previous versions, but it's not hinted at in any way in the authors' original work (its content contradicts the assumptions of theorem 5 in [KM6]). The content of this section summarizes our iterative procedure for the analysis of an AE sentence from section 4 in [Se4]. Note that the authors describe now its first, second , and general steps, precisely as in section 4 of [Se4]. We note, that in principle there is no need to describe the second step, as it is a special case of the general step (we presented it in [Se4] just for presentation purposes).

(20) Page 43 lines 15-16: "extracted fundamental sequence". This notion or object was never hinted at in the original papers of the authors. They do appear in some previous versions of them. Note that this is exactly what they define in section 3.9 on page 34, where they use the term "Induced NTQ system" (cf., our notion *induced resolution* in section 3 of [Se4]). Also, note that what the authors define in section 3.9 can not be used here, e.g., because what the authors define there is not an NTQ and hence Merzlyakov's theorem can not be applied. What the authors really (want to) use (and what we use in section 4 of [Se4]) is (the complete definition of) an *induced resolution* as it appears in section 3 of [Se4].

(21) Page 43 line -15 to -12: no hint in the authors' original papers. This is precisely part (1) of the second step of the iterative procedure that is presented in section 4 of [Se4].

(22) Page 43 line -5 to page 44 line 2: no hint in the authors' original papers.

This is precisely part (3) of the second step in section 4 of [Se4].

(23) Page 44 line 18. Note that the authors use once again the term "with the top part being extracted..." This refers to their "Induced NTQ system" in section 3.9, although the construction that appears there can not be used here. What they really refer to is our *induced resolution* as appears in section 3 of [Se4].

(24) Page 44 lines 4-20. This was never hinted at in the authors' original papers. Case 1 on line 4 is part (5) in the second step of the iterative procedure in section 4 of [Se10]. Case 2 is again part (2) and case 3 is part (4) in the second step of that procedure.

(25) Page 44 line 21 to page 45 line 2: "General step". Nothing like that is hinted at in the original paper of the authors. It is precisely the general step of the iterative procedure that is described in section 4 of [Se10]. Lines 25 to line 28 are precisely parts (2) and (3) in the general step of that iterative procedure from [Se10].

(26) Page 44 line -3. Case 3 is precisely case (4) in the general step of the iterative procedure in section 4 of [Se10]. Case 2 on line -10 is precisely part (2) in the general step of that iterative procedure. Case 1 on line -18 is supposed to be part (5) of the general step of our iterative procedure (section 4 in [Se10]). Note that in the authors' text, case 1 refers to the later cases 2 and 3, since in our procedure part (5) (Case 1) refers to the previous cases (1)-(4), i.e., cases 2 and 3.

(27) Page 44 lines -16 to line -11. What is written there is completely wrong, and seems to stem from a lack of understanding of several other constructions and procedures in our papers. If cases 2 and 3 do not apply at any level, one needs to look at the structure of the induced resolution. By proposition 4.11 in [Se10], the complexity of the induced resolution is guaranteed to drop in this case.

The authors messed up all that. First, it seems from their section 3.9, that they didn't understand the construction of the induced resolution. Hence, they don't refer to it now. Second, what they do suggest is to replace the original group (that they denote G) by some sort of an envelope, which is the image of our *developing resolution* that was constructed in the previous step. A change (an increase) of the original group by some sort of "envelope" is what we do (under different assumptions, and after checking the structure of the induced resolution!) in the procedure that analyzes formulas with more than 2 quantifiers, i.e., the *sculpted resolution* in parts (6) and (7) of the general step of the sieve procedure [Se12]. But not in this procedure that analyzes sentences with only 2 quantifiers. Here, no such "envelope" (i.e., *sculpted resolution*) is used in our procedure (there is no need to consider such), and of course, there is no referral to anything like that later in proving the termination of the procedure. So the authors are "proving" (in their theorem 19) the termination of our procedure, but the procedure that they construct is completely different (and wrong)...

(28) Theorem 19 on page 45 has no hint in the original papers of the authors (the tree $T_{AE}(G)$ has no hint either). It is precisely theorem 4.12 in [Se10].

(29) Lemma 14 on page 45 has no hint in the authors' original papers, nor the notions it uses. It is precisely proposition 4.2 in [Se10]. Note that lemma 14 (i.e., proposition 4.2 in [Se10]) applies to the resolutions that are constructed

in the procedure that is presented in section 4 of [Se10]. However, it is (completely) false if one applies it to the fundamental sequences that the authors construct using their lemma 13.

(30) The argument that the authors give to prove theorem 19 (on page 44) is essentially identical to the argument that is given in the proof of theorem 4.12 in [Se10]. However, the construction that the authors describe in case 1 of the general step of the procedure (lines -16 to -11 on page 44), is different from what appears in the (relevant parts of the) construction in [Se10], and what they describe is wrong.

It is interesting to note that beside the fact that lemma 14 does not apply to the fundamental sequences that the authors actually construct along their procedure, what they do in case 1 of the general step of their procedure essentially kills the argument that they are now using. They are trying to address this part of case 1 at the end of the proof of theorem 19 on lines 15 to 22 on page 48. However, what they write in these lines is (very unclear) reflections on the proof of proposition 4.11 in [Se10]. But proposition 4.11 refers to the general step of the iterative procedure, and to the constructions that appear in [Se10], and not to what the authors do (mistakenly, probably because of lack of understanding) in case 1 of their general step in line -16 to -11 on page 44...

(31) Section 7 "Projective images" on page 50 has no connection to a section with the same title in [KM5]. The introduction that the authors give in lines 14 to -4 is really an introduction to a quantifier elimination process (which is indeed what they are going to do - that is what we do in [Se5]). However, although quantifier elimination is a major goal, it is never mentioned as a theorem or even as a specific goal. Recall that quantifier elimination is the major goal and theorem in [Se5] and [Se6]. The content of this section is essentially the sieve procedure that appears in [Se12] - no hint at anything that is described in this section appears in the original papers of the authors.

(32) Page 51 lines 9-10: "modulo factors in the free decomposition of $F_{R(U)}$". This is wrong. The authors probably meant to define something similar to our *auxiliary resolutions* (definition 1 in [Se12]), but it seems that they didn't quite understand it. The diagram that they are considering is with respect to the freely indecomposable non-cyclic factors of $F_{R(U)}$. However, in a resolution (fundamental sequence in their terminology) of such a diagram, the values of the free factor of $F_{R(U)}$ are being changed. Hence, not every value of $F_{R(U)}$ that can be extended to a value of $F_{R(P)}$ can be extended to a value of one of the terminal groups in the diagram that the authors are constructing. e.g., it may be that N_1 is embedded in $F_{R(P)}$ but is not embedded in $F_{R(T)}$. Therefore, the group $F_{R(P)}$ can not be replaced by the terminal groups in the diagram that the authors constructed for the continuation of the procedure. This is a crucial mistake.

(33) Page 51 line -9 to -8: This is redundant, as $F_{R(T)}$ is already assumed to be a terminal limit group (rigid or solid). Page 51 lines -7 to -1: the dimension of $\bar{S}_1$ is 0, hence the remark about the dimension is empty. Also, the authors are considering the resolutions (fundamental sequences in their terminology) with respect to all the QH subgroups of T_1. This is actually meaningless. It seems that the authors misunderstood the notion of *surviving surfaces* (definition 1.8 in [Se11]), as they should have considered fundamental se-

quence only with respect to these (*surviving*) QH subgroups. In fact, these surviving surfaces were "borrowed" (at least hinted) in a previous version of the authors (page 31 line -14 in the version of July 20th 2003), but it seems that the authors didn't really understand what they were, and why they were essential, as they were taken out in the current version.

(34) Page 52 line1 to line 9: the statement of Case 1 of the authors is precisely the statement of propositions 4.2 and 4.3 in [Se10]. It is supposed to be part (2) of the first step of the sieve procedure in [Se12]. However, the conclusion of proposition 4.2 is completely false here, as the assumptions are completely different... What the authors really meant to "borrow" is theorem 4.13 in [Se11], but to state the conclusion of that theorem one needs to define the *core resolution*... (this is defined in section 4 of [Se11], and its definition takes 45 pages).

(35) Page 53 lines 14-15: this is supposed to be the construction of our *developing resolution* as it appears in part (2) of the first step of the sieve procedure in [Se12]. Note that the authors use the term "extracting a fundamental sequence" when they refer to our *induced resolution*, that they called "induced NTQ system" in section 3.9 on page 34 of this version. What they call "block-NTQ group" on line 13 is precisely the *anvil* that is constructed in part (2) of the first step of the sieve procedure.

(36) Page 53 line 19 to line 26: this is the authors' version of part (3) of the first step of the sieve procedure in [Se12]. All the mistakes that the authors made in their case 1 are dragged into this case, of course.

Page 53 line 27 to line -1: this is the authors' version of part (4) of the first step of the sieve procedure in [Se12]. Lines 29-30: "enveloping block-NTQ system" is precisely the *sculpted resolution* that is constructed in part (4) of the first step of the sieve procedure in [Se12]. Line -12 on page 53 to line 8 on page 54: this is supposed to be the authors' version of the construction of the core resolution, as it appears in section 4 of [Se11] (it's 45 pages there). What the authors describe in these few lines is only "reflections" on the construction in [Se11] - it's partial and incorrect (we have already wrote comments on these lines that appear in pages 32-33 in the version of July 20th 2003).

Note that the core resolution should have been defined earlier, as it is (supposed to be) used extensively in cases 1 and 2 (pages 52-53), and not only now. In fact, the statements that appear in these cases can be made precise only with the notion and construction of the core resolution (to compare, we use the core resolution in all cases (2)-(4) in the first step of the sieve procedure in [Se12], and not only in case (4)).

(37) Page 54 lines 9-13: the notion "projective image" that the authors use here has no connection with their original notion (with the same name) in [KM5] and [KM6]. The current notion is supposed to be the data structure that we construct along the sieve procedure (definition 9 in [Se12]). Note that what we call *width* is called here "type".

(38) Page 54 line 14 to page 55 line 27: this is supposed to be the authors' description of the general step of our sieve procedure in [Se12]. Page 54 line 15-16: the authors confuse between the index of the "type" (what we call *width*) and the index of the step (they denote both by n). In the general step of the sieve procedure, with every step there is an associated *width*

(the width is always bounded by the step number but they are usually not equal).

Page 54 line 20 to line 24: this is supposed to be the authors' version of parts (2)-(3) of our general step. However, this is wrong if part (7) of the general step of the sieve procedure (in [Se12]) applies. In particular, it will not be "projective image" of "type" n (in the terminology of the authors), and not always one can apply the construction that appears in the AE case.

(39) Page 54 line -15 to -14: the same mistake as in the first step (see comment (31) above). Page 54 lines -13 to -10: Unlike the first step, here somehow the authors do "realize" that they should use our *surviving surfaces* (definition 1.8 in [Se11]), though the way they refer to them (lines -11 to -10): "QH subgroups... that do not give sufficient splittings" is wrong (see our comment (2) in section 11, i.e., on the version of July 20th 2003).

Page 54 line -9 to page 55 line 6: this is supposed to be the authors' version of part (4) of the general step of the sieve procedure in [Se12]. However, what they write is (completely) wrong because of what they wrote on lines -15 to -14 (see comment (33)), and because they don't use our *core resolution* (in particular, theorem 4.13 in [Se11]). This is strange, because the authors already briefly "sketched" the construction of our *core resolution* on page 53 line -12 to line 8 on page 54. Somehow, the authors don't realize that our *core resolution* is relevant in all the parts of the general step of the sieve procedure and not only in the last parts in which *sculpted resolutions* are constructed and added.

(40) Page 55 line 7 to line 14: this (Case 2) is supposed to be the authors' version of part (5) of the general step of the sieve procedure in [Se12]. However, they neither use nor refer to the core resolution in this case and in that way it doesn't make any sense, and is of course completely wrong.

Page 55 line 15 to line 17: This is supposed to be the authors' very brief version of parts (6) and (7) in the general step of our sieve procedure. Note that here the authors do mention their brief sketch of our *core resolution* (as they did in the first step), though they are not aware that this construction is crucial also in their other cases (the other parts of the general step of the sieve procedure in [Se12]). Also note that the authors are not aware at all to the relevance and importance of our *penetrated core resolution* (definition 4.20 in [Se11]). Without the complete description of the general step of the sieve procedure (as it appears in [Se12]), what the authors describe doesn't make any sense.

Page 55 line -16 to line -9: see our comments (7) and (8) in section 11, on the version of July 20th 2003.

(41) Page 59 line -21: the term "formula solution" appears again. This is our *formal solution* (e.g., the title of section 1 in [Se2]). It appeared once (surely by mistake) in the version of March 20th 2002, and was taken off in the next version (see our comment (4) in section 9). The authors used a similar object in their "implicit function theorem" paper [KM4], but never used this term except once in the version of March 20th 2003.

(42) Page 60 lines -9 to -7: "We will first show that this sentence is true if and only if some boolean combination of sentences with less alterations of quantifiers is true, and the reduction does not depend on the rank of the free group F and even on the group $F_{R(S)}$". This means quantifier elimination,

and enables a classification of f.g. groups that are elementarily equivalent to a free group. Somehow, none of these two major results is stated by the authors. Could it be because these are two of our main goals (cf. [Se6])? note that the comment about independence in the group $F_{R(S)}$ was never hinted at nor mentioned in the authors' original papers [KM4]-[KM6].

(43) Page 61 line -15: "we construct the tree T_{X_k} which the analogue of $T_{AE}(G_i)$". This is precisely the authors' version of our *tree of stratified sets* (see definition 1.19 in [Se11] and the paragraph before it). No hint at anything like that in the authors' original papers.

Page 62 lines 1-3: " we will show... only a finite number of possible schemes". This is precisely the finiteness of our *proof systems* (definition 1.20 in [Se11]). No hint at any of these notions, claims or arguments in the authors original papers.

Page 62 Definition 23 and page 63 Definition 24: this is precisely definition 1.20 in [Se11] for *depths* 1 and 2.

(44) Page 64 line 17: the notation $W_{surplus}$ is precisely our $ExtraPS$ resolutions and limit groups (definition 1.28 in [Se11]). Note that the notion "surplus" that the authors use appears in quotation marks in the paragraph that explains definition 1.28 in [Se11] and comes just before it, two lines before the definition and in referral to the $ExtraPS$ limit groups.

(45) Page 65 line 1: the notion "thickness" appears (with no explanation). This is precisely our *width*, as it appears in part (4) of the first step of the sieve procedure or definition 9 in [Se12].

(46) Page 65 line 9-10: Lemma 18 is precisely theorem 27 in [Se12]. The strategy of the proof is identical, and so are the details. However, the actual description of the proof by the authors is missing and at few crucial places even wrong. It is important to note (again), that as in some of their previous versions, the authors use the notion *width* for a different purpose (see page 63 line 7), but somehow lemma 18 of the authors and our theorem 27 in [Se12] are literally identical, so both use the term "width" but with different meanings...

(47) Page 65 line -8 to page 66 line 4: this never appeared in any previous version. It is the first time that the authors realize that there is a problem with different modular groups that are associated with different envelopes. Note that like in studying equations with parameters, there is a need to pass to a particular subsequence (and essentially look at a Gromov-Hausdorff limit), which is not natural when one uses the Makanin-Razborov elimination processes.

The (new) construction that the authors describe is supposed to be the construction that appears in definition 31 in [Se12]. On page 65 lines -12 to -11: "and do not satisfy any proper equation". In general, they may satisfy another equation. That would not effect the constructions and arguments in [Se12], but that may cause a problem if one tries to prove effectiveness, as the authors claim to prove.

What the authors describe is not really coherent. On page 65 line -4, the authors add elements to every edge in the JSJ of K. However, in the previous lines in the same paragraph, the authors explain that they do not really consider K, but rather fundamental graphs of certain subgraphs of the JSJ of K. In fact, it's not clear why they needed to consider these subgraphs, after they are introduced (i.e., from line -4 on page 65 and on).

The argument that the authors use in lines 13-28 is supposed to be their translation of the proof of theorem 38 in [Se12]. In particular, the induction that they refer to in line 23 on page 66 is precisely proposition 37 in [Se12]. All that never appeared or was hinted at in any of their previous versions.

(48) Page 67 lines -13 to -11: Theorem 22 is completely new. No such classification appears or was hinted at in the original papers of the authors. This is precisely theorem 7 in [Se13].

(49) In their entire argument, the authors never mention nor apply our *penetrated core resolutions*, as they appear in definition 4.20 in [Se11]. They also never make sure that the (sub)resolutions that they work with are *firm resolutions*, as it appears in definition 4.1 in [Se11] and definition 3.9 in [Se10]. Without penetrated core resolutions, and *firm* core resolutions, the main concepts of the sieve procedure do not make sense. Recall that the sieve procedure is the procedure that we use for quantifier elimination in [Se12], and the procedure that the authors borrowed for their proofs of theorems 1 and 2 in this version.

§13. Elementary theory of free nonabelian groups - May 6th, May 9th, June 8th, October 7th, and November 11th 2004

The previous version of KM 3rd paper (of April 24th 2004) has 73 pages. The new versions have 73, 73, 75, 82 and 82 pages. We will only indicate the changes in the new versions, in comparison with the previous version of April 24th 2004.

(1) The version of May 6th 2004 is rather similar to the version of April 24th 2004. The main changes are in the effective part (pages 69 to 73). Since we didn't consider effective questions, we don't comment on them in this report.

(2) The version of May 9th 2004 is very similar to that of May 6th 2004. The main difference is a change in the order of sections 8.6 and 8.7 on pages 66 and 67 in correspondence. Somehow, the authors noticed that they would better finish analyzing fundamental sequences (*proof systems* in our terminology) of higher level, before they can prove theorem 1. This was in opposite order in the previous two versions.

(3) In the version of June 8th there are few changes. Theorem 7 and its proof on pages 26-27 is new. The diagram that the authors construct in these two pages (and the way they do it) is precisely the Makanin-Razborov diagram that is constructed in section 5 in [Se1]. Note that the authors use Konig's lemma for the construction (line -14 on page 26), a lemma that is foreign to Makanin's elimination processes, and used often in our work (e.g. in section5 of [Se1]). The authors also expanded their "proof of theorem 1" in pages 67-70, but the changes are mostly cosmetic.

Note that the notion "formula solution" (which is our *formal solution*) that the authors left (probably by mistake) at a single place on page 59 in the version of April 24th, was not removed yet (it's still on page 59).

(4) The version of October 7th 2004 has few changes and it is a bit longer. Section 1.19 on page 20 is new (the authors used some of the notions in it in previous versions, but its results do not exist in the original papers of the

authors).

Page 20 line 16: "Maximal standard fully residually free quotients" is precisely our *maximal shortening quotients* (lemma 5.5 in [Se7]). Corollary 4 on page 20 line -12 to line -8 is new. It is precisely definition 5.9 and proposition 5.10 in [Se7].

Page 29 line 5 until page 36 line 21: This is the authors' "parallel" of our construction of the *completion* of a resolution (definition 1.12 in [Se8]). The authors did have NTQ groups in their original papers, and they did prove that a limit group (fully residually free group) embeds into an NTQ group, but they never considered resolutions (fundamental sequences) with parameters, and never considered the detailed construction that they present in this version.

(5) Page 39 line -13 to -8. In these lines the authors define "Relative dimension". This is not hinted at, and of course not used, in any of the authors' original papers. This is precisely the *rank*, as defined in definition 3.8 in [Se10] (which appears just after the construction of the *induced resolution*, precisely as in the current paper of the authors...).

    It is important to note that we use this "relative rank" only when it is equal to the rank of an induced resolution (this is called *firm subresolution* in the subsequent definition 3.9 in [Se10]). Indeed, the authors define "relative rank" on page 39, but it is never used in their paper... (they never define a parallel of a *firm* subresolution)

(6) Page 51 line -20 to line -15. The authors still keep the crucial mistake that they have introduced in the version of April 24th (see our comment (28) in section 12), probably because they misunderstood part (5) of the general step of the iterative procedure for the analysis of AE sentences in [Se10]. They do revise the proof of theorem 30 in this version (pages 52-55), but the theorem is completely false if case 1 of the general step is kept as it is...

(7) Page 55 lines 13 to 20. The authors explain why their case 1 can occur only finitely many times. The statement that they are giving (without any argument) is supposed to be the statement of proposition 4.11 in [Se10]. However, proposition 4.11 claims that there is a drop in complexity in a highest level of the *induced resolution*, whereas the authors only say (lines 15-17 on page 55): "The intersection... on some level will be different" (which is clearly not enough to prove any sort of termination). Moreover, theorem 4.11 in [Se10] applies to the construction that appears in part (5) of the general step of the iterative procedure that is presented in [Se10]. It certainly does not apply to the procedure that the authors present, i.e., to what they write in case 1 of the general step (page 51 lines -20 to -15). This means that the authors give a sketch of the proof of the termination of our iterative procedure (theorem 4.12 in [Se10]), and this proof does not apply to the termination of their own procedure...

(8) The revisions in the version of November 18th 2004 are relatively minor, and we won't list them.

§14. Elementary theory of free nonabelian groups - April 11th and April 17th 2005, and May 8th 2006

The previous version of KM 3rd paper (of November 18th 2004) has 82 pages. The new versions have 82, 82 and 100 pages. We will only indicate the changes in the new versions, in comparison with the previous version of November 18th 2004.

(1) Several parts in the version of April 11th 2005 have been revised. We won't comment on most of these revisions, but we note that on page 21 lines 8-9 the authors indicate for the first time that some of the results in the section "Finiteness Theorem for Groups with no sufficient Splitting" are "formulated in different terms and in non-effective form" in [Se9]. Recall that the results in this section were never hinted at in the original papers of the authors, and the argument they give is essentially a sketch of the proofs of theorems 2.5 and 2.9 in [Se9]. The proof uses the JSJ in an essential way, and involves passing to a subsequence and analyzing a Gromov-Hausdorff limit, which is not natural if one uses the techniques that the authors usually apply (see our comments (3)-(8) in section 8 and comments (7)-(9) in section 12).
(2) The revisions in the version of April 17th 2005 are relatively minor and we won't list them.
(3) We won't comment on the version of May 8th 2006, but rather comment in the next section on the printed version from June 6th 2006.

§15. Elementary theory of free nonabelian groups - Printed version, June 6th 2006

The previous version of KM 3rd paper (of April 17th 2005) has 82 pages. The printed version has 102 pages. We indicate the changes in the printed version in comparison with the previous version, and add general comments on it, partly based on our comments about previous versions.

(1) Page 1: it says that is was received on 1 June 2001. According to the authors, it was submitted in 1999 (page 452 lines 15-16). There is no indication for any revisions after 2001.
(2) Page 452 lines 16-18: "There were some inaccuracies in [16]... This paper is a revised version of it". As we have already indicated in comments regarding previous versions, and we'll summarize and repeat these comments for the printed version - there is practically no connection between the printed version and [16].
(3) Page 452 lines -8 to -7: "We will also describe in Theorem 41 f.g. groups that are elementary equivalent to a non-abelian free group". There is no hint of anything close to that in the authors' original paper (or to any attempt or a hope to get close to it). This is precisely theorem 7 in [Se13]. Note that the authors are making the same language mistake ("elementary equivalent" and not "elementarily equivalent") that we had in the original version (see theorem 7 in [Se6]).

    In the sequel we will see that in their theorem 41, that the authors refer to, they are making the same mathematical mistake as the one in theorem 7 in [Se13], that was later found and corrected by C. Perin [Pe].
(4) Page 453 lines 1 to -7. The authors mention their main techniques, but somehow omit techniques that are used in the main body of their work (note that their title is "Our approach"). e.g., (we'll use our terminology)

equations with parameters, induced resolution, core resolution, iterative procedure for the analysis of AE sentences, and finally the sieve procedure for quantifier elimination. Needless to say, none of these objects, concepts, or techniques (that the authors do not mention) can be traced in the authors' original papers, but they are the main and the most difficult constructions and arguments that are used in the solution of Tarski's problems.

In addition, the authors do not mention the JSJ decomposition (except the fact that it can be computed). The JSJ with its canonical properties is the main concept and object that enabled the solution of Tarski's problems. Again, although the authors do mention the JSJ in [KM6], they never used any of its properties, and they were not aware of its centrality in approaching these problems. The JSJ started to appear in some of their revised versions, and in the published version it has a central role, of course, and appears in almost every section (but not in the introduction...).

(5) Page 458 lines -15 to -13: "Let $n_1 \geq n_2 \geq \cdots \geq n_k$ be a sequence... Then the tuple $size(S) = (n_1, \dots, n_k)$ is called the *regular size*. We compare regular sizes lexichographically from the left". No size, or any other form of complexity is mentioned in the original papers of the authors (and definitely not any geometric quantity). This is part of the definition of the *complexity* of a *resolution* as it appears in definition 3.2 in [Se10], and the authors are going to use it in the sequel precisely in the same places as we do.

(6) Page 462 lines -11 to -3. The authors never studied splittings, in particular, free decompositions relative to a subgroup, or relative to a finite collection of subgroups in their original papers. These basic concepts appear in theorems 9.2 and 12.2 in [Se7].

(7) Page 466 line -3 to page 467 line 7. This lemma 7, although a basic tool and used often in the published version, was not mentioned, hinted or used in any of the authors' original papers. It is precisely lemma 1.4 from [Se10].

Also, note that the authors call this section "induced QH-subgroups". Indeed, it is a special case of our *induced resolution*, as it is presented in section 3 of [Se10].

(8) Page 467 line -13 to page 469 line -15. The JSJ was not used by the authors at all in their original papers, but now it is a major tool (indeed it is the main tool in our work). In particular, Theorem 10 on page 469 (lines -17 to -15) is precisely theorem 12.2 in [Se7].

(9) Page 470 lines 14-17. Lemma 8 is wrong. As stated, it is completely false. Even modulo inner automorphisms, i.e., in $Out(G)$, the lemma is wrong as was observed by G. Levitt, who found this mistake in an analogous theorem of mine for torsion-free hyperbolic groups (from 1991), and stated and proved a correct statement [Le].

(10) Page 477 line 20: "Minimal solutions and maximal standard quotients". These are precisely our *shortest homomorphisms* and *maximal shortening quotients* (claim 5.3 and and lemma 5.5 in [Se7]).

(11) Page 480 lines 13 to 20: the notion of "sufficient splitting" and a sufficient splitting relative to a finite collection of subgroups was never hinted or used in any of the authors' original papers (it is used a lot in the printed version). Definition 18 that appears in these lines is precisely (the negation of) definition 12.4 in [Se7].

(12) Page 481 lines 9 to line 16. Lemma 16 didn't exist in the authors original

papers. It is precisely definition 5.9 and proposition 5.10 in [Se7]. In fact, the conditions of lemma 16 of the authors are identical to what appears in definition 5.10 in [Se1] (our original version) and not to what appears in the printed version [Se7]. We corrected the statement of the definition in the printed version after a mistake that was pointed out to us by M. Bestvina. The authors interpreted our old definition, hence, there is a problem with their case (1) (a rigid subgroup should be replaced by its "socket" - see part (i) in definition 5.9 in [Se7]), precisely the problem that was pointed out to us by Bestvina.

(13) Page 482 lines -17 to -13: Definition 20 is new. The authors never looked on splittings modulo a subgroup in their original papers. This is the authors' interpretation of *flexible quotient*, as it appears in definition 10.3 in [Se7].

(14) Page 482 line -1 to page 483 line 2: Lemma 18 is false (lines 8-10 on page 483 give a wrong argument).

(15) Page 484 lines 8 to 10. Lemma 20 is new - no hint in the original papers of the authors. It is precisely the paragraph before definition 10.5 in [Se7], that argues that there exists finitely many *flexible quotients*. The authors notion "reducing" is precisely our *flexible* (see our comment (13)).

(16) Page 484 line -6. The notions "reducing" and "algebraic" are new - never hinted at or used in any of the authors' original papers. "algebraic" solutions are precisely our *strictly solid* homomorphisms (definition 1.5 in [Se7]).

(17) Page 485 lines 12-18. Theorem 11 is new. It was never hinted at or used in any of the authors' original papers. This is a central theorem in our work (the analogue of theorems 2.5 and 2.9 in [Se9]). The proof that the authors give is essentially the same one that is used in [Se9], and we commented on that in previous versions (see comments (3)-(9) in section 10 and comments (6)-(9) in section 12). Note that in previous versions (e.g., the version of April 17th 2005) the authors mentioned that similar theorems were proved in our work, but this was taken out in the published version.

(18) Page 485 lines -5 to -3. "since the group K is ... one may assume that ... is a monomorphism". This is wrong. It was true for the definitions and the statement of their theorem 11 in some of their previous versions. However, in the current statement of theorem 11, the authors "adapted" our notion of *strictly solid* homomorphisms (their "algebraic solutions"), and for this current statement one can not make this reduction.

Page 486 lines 4-5: "Generalized equations for which K is not embedded into ... result some of the equations from R". The authors are doing the same mistake as in page 485 lines -5 to -3. They (the authors) continue to refer to the statement of the theorem that appears in their previous versions, and not to the current statement that uses our *strictly solid* homomorphisms ("algebraic solutions") and *maximal flexible quotients* ("complete reducing system").

Page 486 line -7: "In case (2) we obtain an equation from the family R". The same mistake as in page 485 lines -5 to -3.

(19) Page 487 line 16 to page 490 line 14. The argument that the authors describe is precisely (a sketch of) the argument that appears in the proof of theorem 2.9 in [Se9] (see our comments (3)-(9) in section 10 and (6)-(9) in section 12). This argument requires passing repeatedly to subsequences, and essentially studying Gromov-Hausdorff limits of these sequences. It is a geometric

argument that is very different from Makanin's elimination schemes that the authors used in their original papers.

(20) Page 496 line 3: "Standard maximal residually free quotients" are our *maximal shortening quotients*. page 486 line 11: "principal" standard maximal fully residually free quotient is our *strict* quotient (see comments (10) and (12) above).

(21) Page 496 line -12 to page 498 line -1. This is precisely the construction of our *completion* as it appears in definition 1.12 in [Se8].

(22) Page 504 line 1 to page 505 line 19. The first and second restrictions on fundamental sequences had not appeared, or hinted at, or used in the authors' original papers. Together they are precisely our notion of *well-separated resolution* as defined in definition 2.2 in [Se10]. These resolutions and homomorphisms that are *taut* with respect to them, are basic, central and repeatedly used in our iterative procedures for quantifier elimination.

(23) Page 506 line 10 to page 507 line 14: In this section the authors describe a construction that they call "Induced NTQ system". No object like that appears or hinted at in the authors' original papers, not to mention the construction that they described. What the authors aim to describe is the construction of our *induced resolution* (definition 3.5 in [Se10]). Note that the authors use the term "induced" only here and in section 3.10 on page 466 ("induced QH subgroups"). Whenever they apply the induced NTQ system (*induced resolution* in our terminology) they use the term "extracted" or "extracting".

Note that the induced resolution plays a major role in our papers, and (now) in the rest of the authors' paper. In the sequel, wherever the authors use the *induced resolution* ("Induced NTQ system"), they are actually using the object that is constructed in section 3 of [Se9], and not the object that they are describing in their section 7.12.

We have already commented on the construction that appeared in some earlier versions, e.g. our comments (12)-(16) in section 12. The description in the published version is somewhat closer to our construction than what is described in some of the earlier versions, but still suffers from the same major gaps and mistakes. We indicate a few:

(24) Page 506 lines -11 to -10: "Increasing G_1 by a finite number of suitable elements from abelian vertex groups of $F_{R(S)}$ we join together ... from abelian vertex groups in $F_{R(S)}$". The sentence does not give any hint at what elements should be added. Could it be that the authors refer to what appears in part (ii) in the construction of the induced resolution in section 3 of [Se10] (starting at the 3rd paragraph after definition 3.2 in [Se10])?

(25) In all the construction of their "induced NTQ systems" the authors say nothing about QH vertex groups. In other words, they completely "skipped" part (iii) in our construction of the induced resolution (they have a previous section on "induced QH vertex groups" (lemma 7 on page 466) that is not mentioned here. In any case, this notion and lemma are not a replacement of part (iii) in section 3 of [Se10]). Without this part (or any other part), the construction that the authors describe doesn't make any sense (once again, in all their referrals to "induced NTQ systems", the authors have to use an object that is constructed according to the complete construction that appears in section 3 of [Se4], and not the vague, partial and mistaken

object that they are describing in this section).

(26) Unlike previous versions (see our comment (16) in section 12) the authors seem to realize that one can not work with the factors separately, but each time the iterative procedure is applied from top to bottom, the whole group is considered and not each of its factors separately.

On page 507, lines 4 to 8, the authors aim to explain why the iterative procedure terminates after finitely many steps (what is proved in proposition 3.3 in [Se10]). The 3 possible decreases they count are actually true. However, according to the proof of proposition 3.3 in [Se10], these decreases happen in some highest level, and in all the levels above that level the associated decompositions have the same structure as in the step before, and below that level there is no control on what happens with the associated decompositions (their complexity may increase).

The authors just indicate that in "some level" one of the possible decreases might occur. This does not imply termination of the procedure.

(27) Page 507 lines 9-10: "the image of the to i levels of F_{R_Q} on the level $j+1$ is the same as the image of G on this level". This is completely false. It is true that the image is contained in the image of G in this level (this is precisely the statement of lemma 3.6 in [Se10]). From our point of view, what the authors wrote in lines 9-10 (page 507) is a crucial misunderstanding of the construction of the induced resolution that is presented in section 3 of [Se4].

(28) Page 507 lines 10-12: "Each QH...the tuple of these larger QH-subgroups". This is supposed to be the complexity of a geometric subresolution as it appears in definition 3.2 in [Se10]. Note that along the construction the authors didn't say a single word on QH subgroups, and they appear suddenly when they aim to associate a complexity with their "induced NTQ systems".

It is interesting to note that although we define the *complexity* of an *induced resolution* (definition 3.2 in [Se10]), we never use it, as we don't assign a complexity to an induced resolution in our iterative procedures (it only appears as a complexity of a *core resolution* that we define later). Needless to say, the authors don't use it (the complexity of their "induced NTQ systems") in the sequel either...

(29) The construction of the induced resolution in section 3 of [Se10] takes as an input a *well-structured* resolution and a subgroup of its completion. The *well-structured* structure is fundamental in the construction (see part (iii) of the construction that the authors completely skipped).

A *well-structured* structure is a slightly weaker assumption than a *well-separated* structure (all these are our notions), that the authors "borrowed" in their sections 7.8 and 7.9 (pages 504-505) "first and second restrictions on fundamental sequences". In section 7.10 "Induced NTQ systems" the "first and second requirements on fundamental sequences" are never assumed or mentioned. Without this (geometric) structure, the whole construction of "induced NTQ systems" can not make any sense (and once again, when it is used in the sequel it's always according to the construction that appears in section 3 of [Se10], and (implicitly) under its assumptions and input).

(30) Page 507 lines 15-20: section 7.13 "Relative dimension". Nothing of that sort appears or is hinted at in the authors' original papers. See our comment (5) in section 13.

(31) Page 508 lines 15 to 25: definition 23 is new. The notions that are used in the definition (in particular "algebraic solutions" in part (2) line 21) were never hinted at in the original papers of the authors. What the authors define is precisely our *graded test sequences*, as they are defined in definition 3.1 in [Se8].

(32) Page 514 line 8. The whole section "decision algorithm for $\exists\forall$ sentences" was never hinted at, has no traces, and the tools that are used in it never existed in the authors' original papers. In fact, in their original papers, the authors were not aware that there is any significant difference between sentences or formulas with 2 quantifiers and sentences or formulas with more than 2 quantifiers. The procedure that the authors describe in this section (the construction of their $\exists\forall$-tree $T_{AE}(G)$) is the iterative procedure that we use for the validation of an AE sentence, as it appears in section 4 of [Se10].

The existence of a procedure of the type presented in section 4 of [Se4] is one of the main difficulties in approaching Tarski's problems (to us it took several years to come up with it). Note that the procedure described in section 4 of [Se4] is not canonical in any way, and it is very likely that another attempt to solve Tarski's problems would have come up with a different approach/procedure. The content of this section (as well as other sections in the sequel) contradicts the statement of theorem 5 in [KM6], in which the authors claim that every procedure of some general type terminates, and that such general procedure can be (easily) applied to sentences with an arbitrary number of quantifiers. If this theorem 5 in [KM6] was true, there is no need of a procedure of the type that the authors describe in this section.

(33) Page 518 lines 10-12: "Suppose $F_{R(U_0)}$ is not rationally equivalent to a regular NTQ system... This is the hardest case". This case is precisely the counter-example that I've presented in Edinburgh 1998 after O. Kharlampovich's talk (i.e., after the authors' announcement in June 1998). Theorem 5 from [KM6] was supposed to cover this case as well, but is false. In other words, in their original papers from December 2000, the authors had no strategy, nor any tools, to deal with the case that they now refer to (in justice) as the hardest.

(34) Page 518 lines 20-22: "We may assume that $F_{R(U_0)}$ is freely indecomposable, otherwise we can effectively split into a free product of finitely many freely irreducible factors...and continue with each of the factors in the place of $F_{R(U_0)}$". This is a crucial mistake - a factorization followed by a reduction that the authors suggest is absolutely forbidden. It demonstrates a basic misunderstanding of the procedure that is presented in section 4 of [Se4], and leads to technical mistakes in the procedure itself (e.g., see our comment (33) in section 12).

(35) Page 520 line 14: "a carefully chosen chain of block-NTQ groups". This never appears or hinted at in the original papers of the authors, or anything similar or close to it. This is the main object that is used in our procedures for analyzing sentences and formulas with 2 and more quantifiers - this is the *anvil* as it is presented in part (2) of the first step of the iterative procedure in section 4 of [Se10], and in definition 4.5 in [Se10].

(36) Page 520 line -16 to page 522 line 18: This first step of the procedure

for the construction of the tree $T_{AE}(G)$ is supposed to be identical to the first step of the iterative procedure for validation of an AE sentence that appears in section 4 of [Se10]. However, the authors probably misunderstood certain parts and some assumptions and technicalities in the first step of the procedure in section 4 of [Se10] and that led to several crucial mistakes.

Page 520 line -2 to page 521 line 1: ”Let ... be the subset of homomorphisms... and satisfying the additional equation $U_1(X_1,\dots,X_k) = 1$”. This is a mistake that leads to further mistakes in the sequel. What one should consider are only homomorphisms in $shortest\ form$ as they are defined in definition 4.1 in [Se10]. Note that indeed definition 4.1 in [Se10] was (mistakenly) omitted in the original version [Se4], and this is probably the version that the authors used (although the revised version [Se10] was published in 2004).

Page 521 lines 5-6: ”modulo the images ... of the factors in the free decomposition of $H_1 =< X_2,\dots,X_m >$”. This is what is done in the first step in section 4 of [Se10], but it makes sense only if the authors would have considered only $shortest\ form$ homomorphisms (definition 4.1 in [Se10]). page 521 lines 21-22: ”canonical sequences for H_1 modulo the factors in the free decomposition of the subgroup $< X_3,\dots,X_m >$” - again the same mistake (makes sense only for $shortest\ form$ homomorphisms).

(37) Page 521 lines -11 to -9: Lemma 23 is supposed to be identical to proposition 4.3 in [Se10], which is a key observation for the construction and the termination of the iterative procedure for the analysis of AE sentences. No trace, or hint of anything related to this proposition can be found in the authors' original papers (again, according to theorem 5 in [KM6] there is no need of this lemma or anything around it, including the notions it uses). However, because the authors haven't used $shortest\ form$ homomorphisms in the construction of their fundamental sequences, both the formulation of lemma 23 and the argument that is used for its proof (that imitates the proof of proposition 4.3 in [Se10], but the assumptions there are slightly different) are wrong.

In the formulation of the lemma, it is not true that ”it is possible to replace $H_{(p)}$ by a finite number of proper quotients of it without losing values of initial variables of $U = 1$” (page 521 lines -10 to -9).

The argument that is used to prove the lemma is very confused. The authors are trying to change homomorphisms to be in $shortest\ form$ (definition 4.1 in [Se10]), but it is somewhat late at this stage (they should have considered only such homomorphisms to start with). In particular, at this stage, taking ”minimal solutions with respect to A_{D_t}” (line -3 page 521) changes the values of ”variables of $U = 1$” and this is not allowed (i.e., by doing that one loses ”values of initial variables of $U = 1$” (line -2) that the procedure must handle).

(38) Page 522 lines 5-7” ”block-NTQ group ... generated by the top p levels of the NTQ group corresponding to the fundamental group c and the group ... corresponding to some branch of the tree $T_{CE}(G_{(p)})$”. This is supposed to be the construction of the *anvil*, as it is presented in part (2) of the first step of the procedure in section 4 of [Se10], and in definition 4.5 in [Se10].

Note that what the authors write is mistaken, as in general one can not take an amalgamated product as they suggest, because the group $G_{(p)}$

that is associated with the terminal level of their NTQ group may not be embedded in the group that is associated with the top level of the branch of the tree $T_{CE}(G_{(p)})$. This technical difficulty is treated in part (2) of [Se10] but somehow the authors missed it...

(39) Page 522 lines 8-10: "One can extract from c ... induced by the fundamental sequence c. Denote this *extracted fundamental sequence* by c_2". What the authors are really refering to here is their "Induced NTQ systems" (page 506 section 7.12) (strangely, it is called here "extracted fundamental sequence", and the verb "induced" is mentioned earlier). However, whatever is constructed in section 7.12 can not work here (see comments (23)-(28) above), and what is really needed (and what the authors are actually implicitly refering to) is our *induced resolution*, as defined in definition 3.5 in [Se10].

(40) Page 522 line 19: "9.3 Second step" this is a section that has no trace in the authors' original papers. It is supposed to summarize the second step of the iterative procedure presented in section 4 of [Se4]. Note that the authors describe the first, second , and general steps of their iterative procedure, precisely as in section 4 of [Se4] (although the second step is actually a special case of the general step, and we've included it only for presentation purposes).

Page 522 lines 23 to 25: "Consider the set of those homomorphisms... and satisfy some additional equation $U_2 = 1$". Once again, the same mistake as in the first step - only *shortest form* homomorphisms (definition 4.1 in [Se4]) should be considered. Omitting the shortest form requirement will cause difficulties in the sequel (as in the first step). We note again, that the shortest form requirement didn't exist in [Se4] (the version that was probably available to the authors), and was added in [Se10].

Page 522 lines 26-27: no hint in the authors' original papers. This is precisely part (1) of the second step of the iterative procedure that is presented in section 4 of [Se4].

Page 522 lines -9 to -4: no trace or hint in the authors' original papers. This is precisely part (3) in the second step of the iterative procedure that is presented in [Se10].

(41) Page 522 lines -2 to -1: "case 1" is precisely part (5) of the second step of the procedure in [Se10] (no hint in the authors' original papers).

Page 523 lines 1 to 5: "Case 2" is precisely part (2) of the procedure in section 4 of [Se10] (the same mistakes that the authors made in their "first step" recurs here, see comments (35)-(38) above).

Page 523 lines 6 to 11: "Case 3" is supposed to be part (4) of the procedure in section 4 of [Se10]. However, it seems that the authors didn't quite understood part (4). According to the authors, the case in which G is mapped to a proper quotient, and the case in which N_0^1 is mapped to a proper quotient are treated in the same way. This is a crucial mistake. In part (4) of the second step in [Se10] these two cases are treated differently. In fact, the argument that the authors describe for the termination of their iterative procedure (i.e., the proof of Theorem 36 on page 524 line 3) can not apply if these two cases are treated in the same way (as they do). Of course, the argument that the authors try to describe in proving theorem 36 is (supposed to be) precisely the argument that is used for the termination of the procedure that is described in section 4 of [Se10] (i.e., theorem 4.12 in [Se10]), and does not apply to the construction they describe.

(42) Page 523 lines 12 to -1 : "General step". Nothing like that is hinted at in the original paper of the authors (according to theorem 5 in [KM6] there is no need in it). It is precisely the general step of the iterative procedure that is described in section 4 of [Se10].

Page 523 lines 16-19. What the authors are describing is supposed to be parts (2) and (3) in the general step of that iterative procedure from [Se10]. Note that what the authors describe here is a slight variation of what appears in parts (2) and (3) of the general step in [Se10], but this variation can still work (with essentially the same arguments).

Page 523 line -11 to -6: "Case 2" is supposed to be part (2) of the general step that is presented in [Se10]. However, the crucial mistake that the authors made in their construction of the "block NTQ group" (i.e., the *anvil*) in Case 3 of the second step, recurs here (see comment (40)). In lines -7 to -6 the authors construct an NTQ group "with the top part being $c^{(n)}$ above level p and the bottom part f_i. This is wrong, and the argument that the authors use for the termination of their procedure (the proof of theorem 36 on page 524), which is the argument that is used in proving theorem 4.12 in [Se10], can not work for the construction that the authors describe (it does work for the construction that appears in [Se10]).

Page 523 lines -5 to -2: "Case 3" is precisely part (4) of the general step in [Se10].

(43) Page 523 lines -18 to -12: "Case 1" is supposed to be part (5) of the general step in [Se10]. Note that in the authors' text, case 1 refers to the later cases 2 and 3, since in our procedure part (5) (Case 1) refers to the previous cases (1)-(4), i.e., cases 2 and 3.

Page 523 lines -16 to line -12. What is written there is completely wrong, and seems to follow from a lack of understanding of several other constructions and procedures in our papers. If cases 2 and 3 do not apply at any level, one needs to look at the structure of the induced resolution. By proposition 4.11 in [Se10], the complexity of the induced resolution is guaranteed to drop in this case.

The authors messed up all that. First, from their section 7.12 on page 506, they didn't seem to understand the construction of the *induced resolution*. Hence, they don't refer to it now. Second, what they do suggest is to replace the original group (that they denote as G) by some sort of an envelope, which is the image of our *developing resolution* that was constructed in the previous step. A change (an increase) of the original group by some sort of "envelope" is what we do (under different assumptions, and after checking the structure of the induced resolution!) in the procedure that analyzes formulas with more than 2 quantifiers (i.e., the *sculpted resolution*), but not in this procedure that analyzes sentences with only 2 quantifiers. Here, no such "envelope" is used in our procedure (there is no need to consider such), and of course, there is no referral to anything like that later in proving the termination of the procedure (the whole proof of theorem 36 on page 524 breaks down because of what the authors do in their "Case 1"). So the authors are "proving" (in their theorem 36) the termination of our procedure in section 4 of [Se10], but the procedure that they construct is completely different (and wrong)...

(44) Page 524 line 3. Theorem 36 has no hint in the original papers of the authors (the tree $T_{AE}(G)$ has no hint either). According to (the false) theorem 5 in [KM6] there is no need in such theorem (as it is claimed there that a general procedure that does what the authors describe terminates). It is precisely theorem 4.12 in [Se10]. As we have already mentioned, its proof is a reflection of the argument that is used in proving theorem 4.12 in [Se10], and it applies to the procedure in section 4 in [Se10], not to the (actual details of the) procedure that the authors describe in their general step on page 523.

(45) Page 524 lines -2 to -1: " Those QH-subgroups ... that are mapped into QH-subgroups of the same size by some η_i are called stable". This is a new definition with no hint in the original papers. It is precisely our *survived surfaces* as they appear in definition 1.8 in [Se11].

Page 525 lines 1 to 5: Lemma 24 has no hint in the authors' original papers, nor the notions it uses. It is precisely proposition 4.2 in [Se10]. Note that lemma 24 (i.e., proposition 4.2 in [Se10]), and the argument that the authors present to prove it, apply to the resolutions that are constructed in the procedure that is presented in section 4 of [Se10]. However, the authors didn't use *shortest form* homomorphisms (definition 4.1 in [Se10]) for the construction of their fundamental sequences; hence, even if one corrects the statement of their lemma 23 (which is supposed to be proposition 4.3 in [Se4]), lemma 24 will still be false for the obtained fundamental sequences.

Furthermore, Lemma 24 (or rather proposition 4.2 in [Se4]) can not be applied if one uses the fundamental sequences that the authors use in their first, second and general steps of the procedure (because of the critical mistakes in cases 1 and 2 of the general step of their procedure).

(46) The argument that is supposed to prove theorem 36 and appears on page 525 line -12 to page 526 line 3, that actually briefly sketches the proof of theorem 4.12 in [Se10], applies to the procedure that is presented in section 4 of [Se10], but not to the procedure for the construction of the tree $T_{AE}(G)$ that the authors presented in pages 520-523.

It is interesting to note that beside the fact that lemma 24 does not apply to the fundamental sequences that are constructed in the general step of the procedure that the authors describe, the construction that appears in case 1 of the general step of "their" iterative procedure, kills the argument. The authors are trying to address this part (i.e., the application of their case 1) at the end of the proof of theorem 36 on page 525 line -4 to page 526 line 3. However, what they write in these lines is (very unclear) reflections on the proof of proposition 4.11 in [Se10]. But proposition 4.11 refers to the general step of the iterative procedure, and to the constructions that appear in [Se10], and not to what the authors do (mistakenly, probably because of lack of understanding) in case 1 of their general step on page 523 lines -16 to -12...

(47) Page 527 line -12 to page 533 line -1: section 11 "projective images" has no connection to a section with the same title in [KM5]. The content of this section is essentially the sieve procedure that appears in [Se12]. This procedure, the objects and notions that are involved in it, or even the need for it, have no hint in the original papers of the authors. In particular, the

main object that is constructed along the procedure, "block-NTQ" (page 527 line -2), has no trace in the authors' original papers. "block-NTQ" is precisely the *anvil* that is used extensively in the sieve procedure in [Se12] (e.g., see its construction in part (2) of the first step of the procedure in [Se12]).

Note that our sieve procedure is not "canonical" in any way, and there are many "choices" that we made in defining it. Hence, there is no reason that another group that works on the problem will come up with exactly the same iterative procedure.

Also, note that the sieve procedure is used in our work for quantifier elimination [Se12], which is the main goal of our entire work. In some of the previous revisions (e.g., of April 24th 2004) the authors have added an introduction to this section ("projective images"), in which they basically explain that it is essentially used for quantifier elimination, although they never stated q.e. as a theorem (in spite of its obvious importance). Somehow, in the published version, all this introduction for "projective images" disappeared.

(48) Page 528 lines 19-21: "Consider a finite family of terminal groups of fundamental sequences of P modulo factors in the free decomposition of $F_{R(U)}$". This is a (consistent) fatal mistake that appears ever since the authors started to "borrow" (elements from) the sieve procedure, i.e., since the version of April 24th 2004. The authors probably tried to define something similar to our *auxiliary resolutions* (definition 1 in [Se12]), but they definitely didn't quite understand it. The diagram they are considering is with respect to the freely indecomposable non-cyclic factors of $F_{R(U)}$. However, in a resolution (fundamental sequence in their terminology) of such a diagram, the values of the free factor of $F_{R(U)}$ are being changed, and the values of the other factors are modified by conjugation. Hence, not every value of $F_{R(U)}$ that can be extended to a value of $F_{R(P)}$ can be extended to a value of one of the terminal groups in the diagram that the authors are constructing. For example, it may be that N_1 is embedded in $F_{R(P)}$ but is not embedded in $F_{R(T)}$. Therefore, the group $F_{R(P)}$ can not be replaced by the terminal groups in the diagram that the authors constructed for the continuation of the procedure. This is a crucial mistake.

Page 528 line 25: "Therefore, we can further assume that (it) is freely indecomposable modulo these factors". A false (critical) reduction. Probably caused by the same reasons that led to the mistake in lines 19-21.

(49) Page 529 line 11 to page 530 line 5: this part is completely new, and has not appeared even in the previous revisions of the authors. page 529 line 15: "tight enveloping NTQ group" is supposed to be the *core resolution* as it is constructed in section 4 of [Se11] (definition 4.1 in [Se11]). The *core resolution* ("tight enveloping NTQ group") has no trace or hint in the authors' original papers (there was no need for it there, because of (the false) theorem 5 in [KM6]), but it was "borrowed" in some of the previous revisions. However, in the previous revisions it is mentioned only in the general step of the procedure. It is the first time that the authors realize that the *core resolution* is needed even in the first step of the procedure.

It is interesting to note that what the authors call here "enveloping fundamental sequence" is called in [Se12] *algebraic envelope* (see the last line of the second paragraph of the general step of the sieve procedure in [Se12]).

(50) Page 529 line 16: "induced by the image". This is precisely our terminology (e.g., see the paragraph before proposition 4.5 in [Se11]), for the *induced resolution* associated with a subgroup of a tower. Although the authors "borrowed" the notion (as well as the construction) of an "induced NTQ system" in section 7.12 on page 506, they always referred to it before (earlier in this version and in the previous revisions) as "extracted fundamental sequence" (e.g., line 10 on page 522).

Page 529 lines 17-18: "because we add only elements from abelian subgroups". This is completely false, and proves again that the authors didn't understand the construction of the *induced resolution* as it appears in section 3 of [Se10] (definition 3.5 in [Se10]). Note that the authors "borrowed" the construction of the *induced resolution* in section 7.12 on page 506 and we have already commented on their lack of understanding of the construction (see comments (23)-(29) above). We should note that in spite of that lack of understanding whenever the authors use the induced resolution, it is always used as in section 3 of [Se4] (the description of the authors doesn't make any sense, and doesn't produce an NTQ group).

Page 529 line 18: the notation $Ind(F_{R(U)})$ is new - never appeared in the previous revisions, or earlier in this version. This is precisely our notation for the *induced resolution* - (see definition 3.5 in [Se10]).

(51) Page 529 lines 19-22: "We first add ... and their addition decreases the dimension". There are no explanations of the nature of these QH subgroups (how does one define the dimension? of the group? of the new induced resolution?). The authors probably refer to our *absorbed* QH subgroups as they appear in definition 4.7 in [Se11]. However, the conditions in definition 4.7 for absorbed QH subgroups are far more detailed and technical. Note that in previous versions (e.g., the one from July 20th 2003) the authors didn't notice that *absorbed* surfaces need to be of minimal rank, but this was fixed here ("do not have free variables").

Page 529 lines 23-24: "we add also those QH-subgroups Q ... and have less free variables than the subgroup of the intersection". This is meant to be our notion of $inefficient$ QH groups (definition 4.4 in [Se11]).

Page 529 lines 25-26: "We add all the elements that conjugate different QH subgroups...into the same QH vertex group" - this is meant to be our notion of *reducing QH couple* (definition 4.6 in [Se11]). Again there is no explanation what is the dimension, or how to check if it is reduced or not.

Page 529 lines 27-28: "We add edge groups of abelian groups ... that have non-trivial intersection with $Ind(F_{R(U)})$". This is wrong, and doesn't make any sense. This is probably the interpretation of the authors of our procedure of adding *pegs* of abelian vertex groups (the paragraphs after proposition 4.5 in [Se11]). However, it is completely false - we add *pegs*, not edge groups...

(52) Page 529 line 29: "by levels from top to the bottom". This doesn't make sense. The construction of the *induced resolution* is done iteratively from top to bottom. However, to calculate dimensions, when adding QH vertex groups and pegs of abelian vertex groups, and mostly to finally obtain a $firm$ subresolution (definition 4.1 in [Se11]), one has to go from bottom to top...

Page 529 lines -10 to -8: "As a size of a QH-subgroup ... we consider the size of the QH-subgroup in the enveloping group containing Q as a subgroup of finite index". This is precisely definition 3.2 in [Se4]. Note that the notion of "dimension" of a "fundamental sequence", which is central in the construction of the "tight NTQ envelope", makes sense only for fundamental sequences that satisfy the restrictions that are listed in sections 7.8 and 7.9 on pages 504-505 (our *well-separated* resolutions). However, these restrictions are not assumed or used in the construction of "induced NTQ systems" in section 7.12 on page 506, and this construction is essential in the construction of "tight NTQ envelope" (i.e., the core resolution).

(53) Page 529 lines 11 to -8. In these lines, the authors construct their "tight enveloping system" (line -10), which is the authors' analogue for our *core resolutions*. As we have indicated in comments (49)-(52), the construction that the authors use is supposed to be similar to the construction of the *core resolution* in section 4 of [Se11]. However, the authors slightly modified the construction in section 4 of [Se11], and probably because of lack of understanding, what they are actually describing is completely wrong, and can not serve for the purposes that will be needed in the sequel, i.e., for the purposes that the *core resolutions* in section 4 of [Se11] was constructed for.

Needless to say, this mistaken construction immediately leads to false statements in the sequel, that can be fixed only if the construction that is described by the authors is replaced by our actual *core resolutions*.

(54) Page 529 lines -3 to page 530 line 1: "If the dimensions are the same, we can always reorganize the levels so that... If all the parameters .. are the same, then $TEnv(S_1)$ has one level the same as S_1". This is new - no hint or trace to it in the authors' original papers, and it doesn't appear even in the previous revisions in the first step of the procedure (in the section "projective images"). This is precisely theorem 4.13 in [Se11]. Finally, this statement of the authors is valid for our *core resolution* (theorem 4.13 in [Se11]), and is indeed a fundamental property of the *core resolution* and one of its basic properties, but it is completely false for the construction of the "tight enveloping system" that the authors presented in this page (lines 11 to -8).

Page 530 lines 1 to 4: "Notice that the dimension of the tight enveloping NTQ fundamental sequence... is the same as the maximal dimension of the corresponding subgroup in the terminal group in the enveloping fundamental sequence modulo..." This is completely false (the mistake probably follows from what is explained in comment (48) above).

Page 530 line 5: "Then we consider an induced NTQ system S_1... with respect to $TEnv(S_1)$". This is completely new - no hint to it in the original papers, not even in the previous revisions. This is precisely what is done in part (2) of the first step of the sieve procedure in [Se12].

(55) One of the main properties of our *core resolution* (in comparison with induced resolution) is that it is a $firm$ resolution (definition 4.1 in [Se11]). This is crucial for the termination of the sieve procedure (that the authors borrowed since the version of April 15th 2003), and makes our construction of a core resolution (section 4 in [Se11]) much more complicated... However, the authors don't seem to mention or care about the $firm$ condition (or

property).

(56) Page 530 lines 6-7: Lemma 25 is crucial. Nothing similar appears or is hinted at in the original papers of the authors (there is no need for anything like the lemma because of (the false) theorem 5 in [KM6]). The lemma is supposed to be the authors' interpretation of proposition 3 in [Se12]. However, along the construction in [Se12] we used our *auxiliary resolutions* (definition 1 in [Se12]). In the construction that the authors present they take "solutions of $F_{R(M_i)}$ minimal with respect to the group of canonical automorphisms corresponding to this splitting" (page 529 lines 7-8), and they don't discuss any analogue of our *auxiliary resolutions*. This is what we do in case there are no parameters, and it would suffice for a similar lemma with no parameters (see proposition 4.3 in [Se10]). However, taking these minimal solutions does not suffice in the presence of parameters. Hence, the lemma as stated is false.

Surprisingly, if the authors would have done what they are doing here in the construction of their tree $T_{AE}(G)$, i.e., consider only minimal solutions to start with, then lemma 23 on page 521 (the "parallel" of proposition 4.3 in [Se10]) would have been true. Somehow, they missed that in analyzing sentences with 2 quantifiers in section 9...

Page 530 lines 16-20. The authors are explaining how to find the fundamental sequences that are discussed in lemma 25 effectively. However, the "algorithm" they give is completely false. As we have already explained in comment (37), one can not analyze all the homomorphisms of a tower (instead of only minimal ones), and then continue the fundamental sequence after a Diophantine condition is added. This is misunderstanding of some basic concepts and a crucial mistake.

(57) Page 530 line 25: "Amalgamate ... along $\bar{H}$". In general, $\bar{H}$ is not embedded in a fundamental sequence that is associated with it. Hence, in such a case, one needs to replace the fundamental sequence that appears in top, or take appropriate limit quotients (see part (2) of the first step of the sieve procedure in [Se12]).

Page 530 line 26: "This gives a block-NTQ system". This is precisely the construction of the *anvil* in part (2) of the first step of the sieve procedure in [Se12]. Note that the *anvil* or rather "block-NTQ" is the main construction that the authors are using both in this section ("projective images"), and in their analysis of $\forall\exists$ sentences. This is also the main tool in [Se10] and [Se12]. However, this "block NTQ" was never hinted at in the authors' original papers (nor the need for its existence).

Note that because the construction of the "tight enveloping system" on page 529 is wrong, and "slightly" different from the construction of our *core resolutions* (see comment (53)), the construction of the "block-NTQ" that the authors describe in line 25, that imitates the construction of our *anvil* in part (2) of the first step in [Se12], can not serve the purposes it is supposed to in the sequel, unless the "tight enveloping system" is replaced by our *core resolution*.

(58) Page 530 lines 27-28: "Consider a fundamental sequence obtained by taking a fundamental sequnce for ... and pasting to it a fundamental sequence corresponding to $\bar{H}$. This is precisely the *developing resolution* as constructed in part (2) of the first step of the sieve procedure in [Se12]. There is no hint

of anything similar to that, or of the need of anything like it in the authors' original papers. The mistaken construction of the "tight enveloping system" that caused severe problems in the construction of the "block NTQ" (i.e., the *anvil*) (see comment (57)), causes similar difficulties here. Hence, the construction that the authors describe in lines 27-28 will work in the sequel, only if their "tight enveloping system" will be fixed to coincide with (or to have the same fundamental properties as) our *core resolution*.

(59) Page 530 lines -10 to -9. Case 2 is precisely case (3) of the first step of the sieve procedure in [Se12]. No hint in the authors' original papers. Comments (57) and (58) effect this case as well.

Page 530 lines -4 to -3. "If we have not applied...". This is precisely part (4) of the first step of the sieve procedure in [Se12]. No hint in the authors' original papers. Here the authors add our *sculpted resolution* (see part (4) of the first step in [Se12]).

Page 530 line -2 to page 531 line 3. This is a very brief (and missing) summary of definitions 8 and 9 in [Se12] (the authors omit our *auxiliary resolutions* that appear in these definitions, and this will cause mistakes in the sequel). Also note that the authors rename our *width* (as it appears in definitions 8 and 9 in [Se12]) and call it "type".

(60) Page 531 line 4 to page 532 line -13: "step n of the construction" has no hint or trace in the authors' original papers (according to (the false) theorem 5 in [KM6] there is no need for it). Its content is very similar to the general step of the sieve procedure in [Se12]; however, it describes it very briefly and it contains quite a few fatal mistakes.

Page 531 lines 9-10: "which do not have sufficient splittings modulo $F_{R(U)}$ and which are terminal groups for fundamental sequences modulo $F_{R(U)}$". This is a critical mistake. A fundamental sequence, as the authors suggest, may change the values of the original group, hence, it is absolutely forbidden to use such sequences as a preliminary step. The authors made a similar mistake in their first step (see comment (48)).

Page 531 lines 11-16: "If the image ... is the proper quotient...". This is a very brief summary of parts (1)-(3) of the general step of the sieve procedure in [Se12].

(61) Page 531 line 22 to page 532 line 6: "Case 1" is supposed to be the analogue of part (4) of the general step of the sieve procedure in [Se12]. However, it contains quite a few fatal mistakes.

(62) Page 531 lines 20-21: "We construct .. modulo the rigid subgroups in the decomposition of the top level..". This is supposed to be what we do in the general step of the sieve procedure in [Se12]. However, without our *auxiliary resolutions* (as they appear in definitions 8 and 9 in [Se12]), what the authors do does not make any sense and immediately leads to critical mistakes in the sequel.

Page 531 lines -16 to -15: "then using Lemma 11 we can make its size to be smaller than $size(TEnv(W_{n-1}))$". This is supposed to be the analogue of theorem 4.13 in [Se11]. However, because their construction of the "tight enveloping system" on page 529 is wrong (i.e., doesn't have the fundamental properties of our *core resolutions*), theorem 4.13 in [Se11] is not valid here, and the statement of the authors is completely false. This is indeed a fatal mistake.

(63) Page 531 lines -15 to -13: "Similarly to Lemma 25... that the image of H in E_{n-1} ... is a proper quotient of E_{n-1} or ... does not have a sufficient splitting modulo J". This is supposed to be the authors' interpretation of proposition 3 in [Se12]. However, since they don't use our *auxiliary resolutions* and they consider (multi-graded) resolutions (fundamental sequences) "modulo rigid subgroups in the top level of $F_{R(L_1^{(n-1)})}$" their conclusion (which is similar to the conclusion of our proposition 3 in [Se12]) is false. Again, a critical mistake.

(64) Page 531 line -9 "block-NTQ induced.." an induced NTQ system (our *induced resolution*) is an NTQ group, not a block-NTQ. page 531 lines -6 to -5: "pasting to it a fundamental sequnce corresponding to $\bar{H}^{(n)}$" here it should be "block NTQ" and not a fundamental sequence.

Page 531 lines -4 to -3: "We can consider in this case only fundamental sequences that either do not have a maximal dimension or.. do not have maximal size". The authors do not provide any argument for that. Indeed, because of the wrong construction of their "tight enveloping system" the statement is false. The statement is correct if the "tight enveloping system" is fixed to be our *core resolutions* (or a replacement with the same fundamental properties), and then the argument can be found in theorem 4.18 in [Se11].

(65) Page 532 lines 7 to 15: "Case 2" is supposed to be the analogue of part (5) of the general step of the sieve procedure in [Se12]. However, it contains some crucial mistakes.

Page 532 line 10: "modulo the variables of the next level of $L_1^{(n-1)}$". This is supposed to be the authors' interpretation of what is done in part (5) of the general step of the sieve procedure in [Se12], but what they wrote is completely false. In particular, without any analogue of our *auxiliary resolution* (definitions 8 and 9 in [Se12]) what they wrote doesn't make any sense.

(66) Page 532 lines 18 to 22: Definition 25 defines "projective images". Although the notion "projective image" appears in a section with this name in [KM5], there is no connection between what the authors define now, and what existed in the section with this name in [KM5]. All the objects and notions that are associated now with a "projective image" are objects and notions that are analogous to objects and notions that are used or constructed along the sieve procedure in [Se12].

(67) Page 532 lines -10 to -1: Theorem 38 or anything connected to it never appeared in the authors' original work [KM4]-[KM6] (as the objects that are used in its statement are not even hinted in the authors' original work). However, they state it in a form that may sound as if it is connected to theorem 3 in [KM5] (but there is no real connection).

Because of the wrong construction of "tight enveloping systems" on page 529 (see comments (49)-(53)), theorem 38 as stated is wrong. It can be made correct only if the "tight enveloping system" is replaced by our *core resolutions* or an object with the same fundamental properties. Even after such a replacement, the arguments and the constructions that the authors use for the proof of theorem 38 need to be (significantly) modified to agree with the constructions that are presented in the general step of the sieve procedure in [Se12].

(68) Page 532 line -2 to -1 "there is a generic family of solutions that induces a family of solutions of $TEnv(W_n)$ such that each solution has rank equal to $dim(TEnv(W_n)$. This is completely new, and never appeared even in the previous revisions of the authors. This is precisely our notion of $firm$ *subresolution* as it appears in definition 3.8 in [Se10] (note that what the authors call "generic family" is their replacement for our *test sequence* or *graded test sequence*), and the property that the authors associate with their "tight enveloping system" (their interpretation of our *core resolution*) is precisely what appears in the definition of our *core resolution* in definition 4.1 in [Se11]. What the authors claim in these two lines is precisely theorem 4.8 in [Se11].

(69) Page 533 lines 9 to 16. The argument regarding the size of the "tight enveloping system" that the authors present is completely false (because of the wrong construction of the authors "tight enveloping system" on page 529). It would work for our *core resolution*, according to theorem 4.13 in [Se11].

(70) Page 533 lines -14 to -10: Lemma 26 is completely new. No hint for it in the original papers of the authors, or even in any of the previous revisions. The authors don't really give an argument in the "proof" of the lemma (lines -9 to -2), but they seem to describe their reflections from the proof of proposition 4.11 in [Se11] (which is more general than the authors' lemma 26).

Note that lines -2 to -1 on page 532 are supposed to be the analogue of theorem 4.8 in [Se11]. Proposition 4.11 in [Se11], which is considerably more general than lemma 26 of the authors, is still far from the proof of theorem 4.8 in [Se11]. It is true that theorem 4.8 in [Se11] is proved by induction, as the authors indicate in line -1 on page 533, but the actual proof is non-trivial and requires quite a long argument.

(71) Page 534 line 1 to page 551 line 2. In the original paper [KM6] there is also a section "The proof of Theorems 1 and 2". However, there is no connection between the content of the current section under this name and what appeared in the original paper. Furthermore, the main part of the argument in the current section has no hint or trace in the original papers, and it can be easily traced in the sieve procedure for quantifier elimination in [Se12].

Note that in the original paper of the authors [KM6], there was no real difference between sentences with 2 quantifiers, and sentences with more quantifiers. In both cases, the main tool was the (false) theorem 5 in [KM6]. Furthermore, in the original papers there was no hint of any form of quantifier elimination. Now the authors study first formulas with 2 quantifiers, using a procedure that is used in [Se10] and in section 2 of [Se11], and then they study general formulas, using a procedure that is used in [Se12] (that is much more complicated than the one that is used in the case of 2 quantifiers). What they prove now is a quantifier elimination theorem (theorem 39 on page 546), precisely as the main theorem in [Se12]. As in the previous sections in this paper, the procedures that the authors use are borrowed from [Se10] and [Se12], but the authors inserted quite a few fatal mistakes into the arguments and the constructions that they borrowed.

(72) Page 535 line 3: the notion "level" of the authors that is used often in this section is precisely the notion *depth* from definition 1.20 in [Se11].

Page 535 lines -3 to -2: the term "formula solution" appears here twice, and only here. This is precisely our *formal solution* (e.g., the title of section 1 in [Se2]). It appeared once (surely by mistake) in the version of March 20th 2002, and was taken out in the next version (see our comment (4) in section 9), but then returned in the version of April 24th 2004 and wasn't removed (see comment (41) in section 12). The authors used a similar object in their "implicit function theorem" paper [KM4], but never used this term except once in the version of their last paper of March 20th 2002.

(73) Page 537 lines 9 to 12: "We will first show that this sentence is true if and only if some boolean combination of sentences with less alterations of quantifiers is true, and the reduction does not depend on the rank of the free group F and even on the group $F_{R(S)}$". No hint or trace of anything close to that in the authors' original papers. This is precisely the approach of the sieve procedure in [Se12].

(74) Page 538 line 5: "we construct the tree T_{X_k} which the analogue of $T_{AE}(G_i)$". This is precisely the authors' version of our *tree of stratified sets* (see definition 1.19 in [Se11] and the paragraph before it). No hint for anything like that in the authors' original papers.

(75) Page 538 lines 18 and 19-20: "12.3 Schemes for Y_{k-1}" and "We will show... only a finite number of possible schemes". This is precisely the notion of a *proof system* and the finiteness of our *proof systems* (definition 1.20 in [Se11]). No hint of any of these notions, claims or arguments or approach in the authors' original papers.

(76) Page 539 line 6 to page 9: Definitions 26 and 27 are precisely definition 1.23 in [Se11]. What the authors call "initial sequence" (page 539 line -11) is precisely our *valid PS statement* (definition 1.23 in [Se11]). Needless to say, no hint of anything similar in the authors' original papers.

(77) Page 539 lines -11 to -10: "we have to add variables for primitive roots of specializations of edge groups and abelian vertex groups in the decomposition of...". This is precisely (a modification of) the sentence: "by adding specializations of primitive roots of edge groups and pegged abelian vertex groups in the graded abelian decomposition of..." that appears in the 3rd paragraph after definition 1,23 in [Se11]. No hint of anything similar in the authors' original papers.

Page 540 lines 10-12: "Let $H = F_{R(W)}$ be the group discriminated by fundamental sequences..". This is precisely our *PS limit groups* as they appear in the 4th paragraph after definition 1.23 in [Se11]. No hint in the authors' original papers.

(78) Page 539 line -11. Note that the authors introduce the notion "width" in this line. In [Se12] *width* has a different meaning (see part (4) of the first step, or definition 9 in [Se12]).

Page 540 line -11: the notation $W_{surplus}$ is precisely our *ExtraPS* resolutions and limit groups (definition 1.28 in [Se11]). Note that the notion "surplus" that the authors use appears in quotation marks in the paragraph that explains definition 1.28 in [Se11] and comes just before it, two lines before the definition and in referral to the *ExtraPS* limit groups.

(79) Page 540 line -9 to page 541 line 5: Lemma 27 has no hint (including the

notion it uses) in the authors' original papers. It is precisely theorem 3.6 in [Se11] (or theorem 1.33 in [Se11] in the minimal rank case).

(80) Page 541 lines 3-4: "one of the finite number of fundamental sequences that describe the variables for which solution... is reducing or equivalent to...". This is new and never hinted in the authors' original papers. It is precisely our *Collapsed Extra PS resolutions*, as they appear in definitions 1.29 and 1.30 in [Se11].

(81) Page 541 line 7. the notion "depth" appears here for the first time, and just means step number. Note that the same notion *depth* indicates in our papers the maximal length of a path in a proof system (our *proof system* is what the authors call "scheme").

page 541 line 18: "generic family of solutions". The authors have defined "generic families" in definition 23, page 508 lines 15-25. Their definition is supposed to be the analogue of our *graded test sequence* (see comment (31)). However, this definition of "generic family" can be used only in the procedure for the analysis of sentences with 2 quantifiers (the authors' construction of the tree $T_{AE}(G)$ in section 9 that starts on page 514 line 8). In the sieve procedure, i.e., the procedure that the authors borrowed for the analysis of formulas with more than 2 quantifiers (the one that is used in their "projective images" section), the notion of a "generic family" as defined in definition 23, is false. For that procedure, one needs to consider reduced modular groups, and technically to use any form of Merzlyakov theorem in the current context the authors should have borrowed our notion of *framed resolutions* (definition 5 in [Se12]). Without framed resolutions, the use of "generic families" and general forms of Merzlyakov's theorem does not make any sense in the current context.

(82) Page 542 line 16: "the sequence of proper projective images stabilizes". This is precisely the notion that is used in the sieve procedure (definition 24 in [Se12]). The authors apply their theorem 38 on page 532 to get these stable "projective images". However, there is a major problem with this application; Although the authors borrowed the sieve procedure to prove theorem 38, The proof of theorem 38 as it appears in this paper is false (see comment (67)). It suffers from several fatal mistakes, some of which were caused because of the construction of the "tight enveloping system" on page 529 (our *core resolution*) - see comments (49)-(53).

Because of the authors' lack of understanding of the construction of our *core resolution* (their "tight enveloping system"), and their lack of understanding of "generic families" in the context of our sieve procedure, they don't have any way (any notion or construction) to distinguish between our *core resolution* and our *penetrated core resolution* (definition 4.20 in [Se4]). Both of these resolutions, the *core* and the *penetrated core* are essential in obtaining any form of stability of what the authors call "block NTQ" (our *anvils*) and their associated fundamental sequences (our *developing resolutions*). Without these notions (or rather constructions), and without examining their appropriate *induced resolutions* (see definition 9 in [Se12]), there is no real meaning to the sentence "projective images stabilize". To see how the sentence of the authors could be made precise (with all the notions and procedures at hand) see the statement and the proof of proposition 23 in [Se12].

(83) Page 541 lines 20-24: "fundamental sequences induced by the subgroup of the enveloping group generated by... We call this group a second principal group and consider projective images for these fundamental sequences". This is completely new. No hint or trace in the authors' original papers. What the authors are trying to define here is our *sculpted resolution* as it appears in part (4) of the first step of the sieve procedure, and parts (6) and (7) of the general step of the sieve procedure in [Se12]. What they write is completely wrong (i.e., one can not work or continue the procedure with the construction that the authors suggest, and hope for termination).

The authors perform things in reverse order - once one fixes the (second) *algebraic envelope*, one should first look at its sequence of *core resolutions* ("tight enveloping system") and then construct a resolution that is composed of a sequence of induced resolutions.

Page 541 lines 23-24: "second principal group" is supposed to be the authors' analogue of our *second sculpted resolution*, as it appears in part (4) of the first step of the sieve procedure. Probably because of the mistaken construction of their "tight enveloping system" on page 529 (comments (49)-(53)), the authors do not realize that beside the *sculpted resolution*, one needs to construct, keep, and examine the structure of a related resolution, the *penetrated sculpted resolution* (see parts (6)-(7) of the general step of the sieve procedure in [Se12]). Without *penetrated core resolutions* and *penetrated sculpted resolutions*, the procedure won't terminate...

(84) Page 541 line 26: "thickness". This notion appears here for the first time and nothing similar to the notions and constructions that are needed to define it appears in the original papers of the authors. It is supposed to be our *width*, as defined in part (4) of the first step and parts (6) and (7) of the general step of the sieve procedure in [Se12].

(85) Page 541 lines -11 to -9: "When the chain of projective images of these sequences of increasing depth (and fixed thickness) stabilizes, we add new variables, increase thickness, and so on". The authors are trying to describe what we do in the general step of the sieve procedure (part (6)-(7)) in [Se12], but it seems that they completely misunderstood it, as what they write doesn't make any sense.

In the general step of the sieve procedure in [Se12], it is impossible (conceptually) to "know" when the constructions that are associated with a certain algebraic envelope stabilize. What we do in the general step (parts (6)-(7)) is check the objects that are associated with every algebraic envelope (sculpted, penetrated sculpted, and developing resolutions, core and penetrated core resolutions, Carriers) at every step, going from the first algebraic envelope to the last (the one with maximal *width*), and for the first one in which there is a change we modify the constructions that are associated with it, and basically forget or remove the higher width algebraic envelopes. If there is no change in any of them we add an algebraic envelope of a bigger width.

A major principle of the sieve procedure is that not only the constructions that are associated with algebraic envelopes are changed along the procedure, but also the algebraic envelopes themselves are changed! only when we examine an infinite path of the procedure we can talk about stable algebraic envelope with its stable associated objects (see proposition 23 and definition 24 in [Se12]).

Therefore, what the authors write in lines -14 to -9 on page 541 does not agree with the basic concepts of the sieve procedure in [Se12]. From our point of view, it's not only a mistake, but a basic misunderstanding that doesn't allow one to construct our sieve procedure, or in fact any procedure that is supposed to analyze Diophantine or more generally definable sets.

(86) Page 541 lines -5 to -4: Lemma 28 is precisely theorem 27 in [Se12]. Needless to say that such a lemma couldn't have been stated in the authors' original papers, as the notions and objects it deals with didn't exist, and (the false) theorem 5 in [KM6] didn't require anything similar. The strategy of the proof is identical, and so are the details. The current version is definitely better than previous versions that started to appear in some of the authors' revisions.

It is important to note (again), that even though the authors use the *width* with a different meaning than ours (our *width* is their "thickness") the formulations of lemma 28 of the authors, and theorem 27 in [Se12], are literally similar (in particular, both formulations use the notion "width").

(87) Page 542 line 6: "minimal values from some NTQ system". What does minimal have to do in this context?

Page 542 lines 10-11: "it is enough to consider only solutions for which the images of edge group generators become only bounded powers". No hint for any construction like that, or any argument of this sort in the authors' original papers. It is precisely what is done in the proof of theorem 27 in [Se12] (see definition 31 in [Se12]).

Page 542 lines -20 to -10: This is an entirely new construction - no hint or trace to it in the authors' original papers. It is precisely the construction (and the pass to subsequences) that is performed and presented in definition 31 of [Se12].

(88) Page 543 lines 1 to 3: "As we did in the proof of theorem 11, we construct for K_E... such that K_E is not conjugated into a fundamental group of a proper subgraph of the JSJ decomposition D ...". This statement is wrong, but it is supposed to be the authors' translation of theorem 32 in [Se12]. The argument that proves it has not much to do with the proof of their theorem 11 (i.e., our theorems 2.5 and 2.9 in [Se9]) - see the proof of theorem 32 in [Se12].

Page 543 lines 8-9: "Therefore the quadratic system ... has smaller size than the quadratic system of..". This is precisely proposition 37 in [Se12]. The proof of this proposition is not as simple as the authors' claim in lines 5-8, and requires a significant modifications to the argument that was used to prove theorems 2.5 and 2.9 in [Se9] (i.e., what the authors call theorem 11).

Page 543 lines 11-15. This is a very brief summary of the rest of the argument that proves lemma 27 in [Se12], after proposition 37 has been proved (though the authors omitted many of the details).

(89) Page 544 lines 1-14. Lemma 29 is supposed to be lemma 6 in [Se12]. This is a basic lemma in the whole strategy for quantifier elimination that is presented in [Se11] and [Se12]. This particular lemma, as well as the whole strategy towards quantifier elimination, as well the quantifier elimination itself, have no hint in the authors' original papers.

The exact statement of lemma 29 is still wrong (it is identical to the statement of theorem 1.33 in [Se11] that is false in this context). The authors use the term "generic family" in part (1) of the lemma (line 6). They define a "generic family" in definition 23 on page 508 lines 15-25. However, this definition of "generic family" (i.e., our *graded test sequence*) is invalid here. One has to define *framed resolutions* (as they appear in definition 5 in [Se12]), and prove a form of a generalized Merzlyakov theorem, in order to make the notion "generic family" precise in the context of the lemma (see the difference between the formulations of lemma 6 in [Se12] and theorem 1.33 in [Se11]).

(90) Page 544 line -9 to page 546 line 18. The strategy of the proof of theorem 1 (Tarski's problem), and the objects and constructions it uses, is now identical to the strategy and the objects and constructions that are presented in [Se11] and [Se12] (although what the authors describe is still sketchy and far from being precise). The original papers of the authors contain no hint of this approach, nor of all the constructions and the objects that are used in it and are presented in [Se8]-[Se12]. The original papers of the authors do have some of the objects that are presented in [Se7]-[Se8], but these are very far from the approach that they describe in their published version.

(91) Page 546 lines -17 to -16. Theorem 39 is identical to theorem 41 in [Se12]. This quantifier elimination is the main goal of our work on Tarski's problem, and our whole approach, objects, constructions, and procedures are directed towards it. The authors do mark a reference on this theorem ([33]), but as this is the main goal, the whole approach is directed towards it, and all the other results essentially follow from it, how come it has no hint in their original papers?

(92) Page 546 line -10 to -9. Theorem 41 is identical to theorem 7 in [Se13]. This is the classification of f.g. groups that are elementarily equivalent to a non-abelian free group. Nothing close to it is hinted in the original papers of the authors, and there are no tools in their writings as how to get anywhere close to such a theorem.

The formulation of theorem 7 in [Se13] has a mistake, that was found and fixed by C. Perin [Pe]. Exactly the same mistake exists in the formulation of theorem 41 of the authors. Also, the statement of theorem 7 in our original version [Se6] has a mistake in English (elementary equivalent instead of elementarily equivalent). This same mistake in English found its way to the description of theorem 41 as it appears in the authors' introduction (page 452 lines -8 to -7).

Appendix: Some bold similarities for non-experts

In this appendix we list some of the main types of similarities, both mathematical and literal, between the many versions of the paper "Elementary theory of free nonabelian groups" and the papers [Se1]-[Se6]. In order to make it understandable by non-experts, the main types of similarities are presented in brief, with one or two examples for each. The more detailed review can be found in the attached 15 sections.

(1) It seems that the authors have been working hard in order to erase any

"visible" or "bold" similarities. However, in a few places they were not careful enough.

For example, *resolutions* are very basic in our work. The authors use the term "fundamental sequence" instead of our term *resolution*. In their paper of April 2001 the authors use in general the term "fundamental sequence", but at a single place our term *resolution* sneaks in (see comment (22) in section 2). It should be noted that after we pointed this fact in a note to Mark Sapir, the authors claimed that it was a "joke", but to be on the safe side they quickly removed it.

Similarly, *formal solution* is a very basic concept and used a lot in our work. The construction of these formal solutions appears in the authors' paper "implicit function theorem", but they never use this term. However, the term "formula solution" sneaks into the authors' papers in a few places. It first appeared once in the version of March 20th 2002 (comment (4) in section 9), and was removed in the next version. Surprisingly, it appeared again in the version of April 24th 2004 (comment (42) in section 12), and even twice in the same line in the published version (comment (72) in section 15).

(2) Our notation sneaks into the authors' versions as well. For example, the *induced resolution* is a central object in all the terminating procedures that are used in [Se4]-[Se5]. It was "borrowed" by the authors (see comment (23) in section 15). In the published version they use once the notation $Ind(F_{R(U)})$ to denote what they call "induced NTQ system", i.e., our *induced resolution* (comment (50) in section 15). This is precisely our notation (see definition 3.5 in [Se10]).

(3) Measures of complexity are central in guaranteeing the termination of the main procedures in our work. The authors were not aware of any termination problems in their original papers (see sections 1 and 2), but after seeing our counter-example, they decided to borrow some of our measures of complexity. Although there is nothing canonical in our choice of complexity measures, the typographical similarity between ours and the authors' measures cannot be missed (comment (1) at the end of section 3).

Compare the authors': "A characteristic of R will be the sequence:

$$ch(R) = (k, (g(R_{j1}), r(R_{j1})), \ldots , (g(R_{jn}), r(R_{jn})))$$

where $(g(R_{ji}), r(R_{ji})) \geq (g(R_{j,i+1}), r(R_{j,i+1}))$... ... Denote by $ab(R)$ the sum of ranks of non-cyclic centralizes in R minus the number of such centralizers".

with our notation:

$$Cmplx(Res(t, a)) = (rk(Res(t, a)),$$

$$(genus(S_1), |\chi(S_1)|), \ldots , (genus(S_m), |\chi(S_m)|), \ Abrk(Res(t, a))).$$

(it should be noted that rk and $Abrk$ in our notation is the same as k and $ab(R)$ in the authors' notation).

(4) Our original papers [Se1]-[Se6] and even the final versions [Se7]-[Se13] contain a few mathematical mistakes, as well as mistakes in English. These found their way into the authors' papers. An example of the combination

of both mathematical and language mistakes is described in comment (92) in section 15 (our mathematical mistake was found and fixed by C. Perin) or in Comment (12) in the same section (found and fixed by M. Bestvina).

(5) Many of the constructions and the definitions in [Se1]-[Se6] are rather long and quite complicated. Kharlampovich and Myasnikov realized that they have to use some (or rather many) of these objects, but often they probably haven't quite understood the constructions or the definitions in detail. Somehow, they chose to skip some parts or steps in these complicated constructions, and as one may expect, the result usually doesn't make any sense (for example, see comments (23)-(27) and (49)-(53) in section 15). However, whenever the authors need to use these objects and constructions later in their papers, they use it exactly as they are used in our papers, i.e., according to the precise constructions, and with the exact properties, that appear in [Se1]-[Se6] (and not as described in their own papers)...

(6) Even though the published version of the paper under review has gone through approximately 30 revisions, in a period of more than five years, it contains dozens of mistakes, many of which are fatal (see section 15). Many of the mistakes reflect basic misunderstandings of the constructions and the arguments in the paper.

For example, the authors have a section titled "induced NTQ system". How could the authors ever expect that what they describe gives an NTQ group at all (see comments (23)-(27) in section 15)? it would have, if they would have carefully (and not carelessly) copied the content of section 3 in [Se10]. Another example for a very basic misunderstanding and a fatal mistake caused by it is described in comment (43) in section 15, and there are dozens more (especially in the published version, see section 15).

(7) The main technical difficulty in tackling Tarski's problems (using any form of *formal solutions*) is the ability or the possibility to find terminating iterative procedures for the analysis of sentences and formulas. Even today there is no conceptual explanation why such procedures exist, and there is a big difference, conceptual and technical, between the existence of such a procedure for the analysis of formulas with two quantifiers, and formulas with more than two quantifiers. Most of our work was dedicated to finding such procedures, and it took us quite a few years.

Because of a fatal mistake in [KM6], Kharlampovich and Myasnikov were not aware of the need to find such procedures at all (see sections 1-2 or [Se14]). Furthermore, because of that same crucial mistake they haven't realized that there is a significant difference between formulas with 2 quantifiers and formulas with more than 2 quantifiers (these are treated by a simple induction in [KM6]).

The long and complicated terminating procedures that we have managed to find (in [Se4] and [Se5]) are by no means canonical. They involve a long list of choices along their constructions. How come the authors chose precisely the same procedures with identical choices as ours among myriad possible alternatives?

(8) Both conceptually and technically, the JSJ decomposition, together with its canonical properties, is the most important and powerful object that is used in [Se1]-[Se6]. It is used in dozens of places in our work, and it enabled the application of geometric perception and techniques to study algebraic

and model theoretic objects and problems. From our perspective, it is the most significant advantage that we had in comparison with Makanin and Razborov who worked on Tarski's problems in the 1980's.

Although the JSJ is mentioned in Kharlampovich and Myasnikov's original paper [KM6], it was never used in any essential way (it is only mentioned in [KM6] as a substitution for Makanin and Razborov construction of their diagram). They were surely not aware that the JSJ, and especially its canonical properties, are the key in approaching the main difficulties that arise in tackling Tarski's problems, partly because they didn't know what these main difficulties are. Along the various versions of their papers, the JSJ and its canonical properties were transformed to be the main tool as well...

(9) The general strategy or the approach that we used in tackling Tarski's problems is by no means canonical (indeed, in a more recent work on free products [Se15], we used an alternative approach to answer and generalize Tarski's problems). There are many choices that we have made in [Se1]-Se6], in many of the steps of our program and in the constructions that we have used. It is very unlikely that a different group working on these problems will come up with exactly the same strategy, the same objects, and the same choices as we made.

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