Simple Groups of Finite Morley rank, 2002

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1. Introduction

Although stability theory was developed as a tool for proving results in pure model theory, it was seen early on, notably in [16], that the theory could provide nontrivial information about specific algebraic theories, and it was natural to try to get some concrete sense of the range of applicability of stability theoretic notions with regard to familiar algebraic theories. The first to take up this challenge (publicly, at least) was Macintyre, who in two papers published in 1971 [36, 37] classified the $\aleph_0$-stable abelian groups and fields. The abelian groups turned out to be direct sums of groups of bounded exponent and divisible groups, a result which has become particularly useful in a generalization due to Nesin which is applicable to $\aleph_0$-stable nilpotent groups, along with a variant by Borovik and Poizat applying to 2-Sylow subgroups of $\aleph_0$-stable groups, which will shortly provide the framework for my discussion here. The infinite $\aleph_0$-stable fields are all algebraically closed, a result which provided Macintyre with a devious route toward something a bit closer to his heart, namely the classification of fields allowing quantifier elimination.

Of course model theorists' groups and fields tend to be enriched by additional structure, and one also wants to know what the range of such enrichments can be, in the sense of geometrical stability theory (as in [38]), but this is a very different problem, not only at the level of structures, but at the level of morphisms.

As far as classical algebraic structures are concerned, the outstanding open question with regard to fields is the following:

The Stable Fields problem

Is every stable field separably closed?

As far as groups are concerned, one would like a good theory of solvable groups, and of simple groups. For solvable groups there is an extensive and useful theory, or set of related theories, a good deal of which can be found in [22], and which turns out to be essential for the analysis of nonsolvable groups as well; this subject continues to develop, with Frécon the leading practitioner at present. The obvious conjecture as far as simple groups are concerned is entirely definitive:

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The Algebraicity Conjecture

A simple group of finite Morley rank is isomorphic to an algebraic group—more precisely, to the group of rational points over some algebraically closed field, of an algebraic group defined over that field.

Zilber first made this conjecture, along with a much broader conjecture, or family of conjectures, of considerable notoriety and influence. Since then a long series of developments well known to model theorists have considerably cleared the air, including, most relevantly to our present purpose, Hrushovski's universal counterexample construction, a stability theoretic modification of Fraïssé amalgamation. However the Algebraicity Conjecture remains untouched among the debris, as does the following more plausible candidate for the Hrushovski technology:

The Affine Pseudoplane problem

Is every $\aleph_0$-categorical stable group abelian by finite?

As it happens, while working on the $\aleph_0$-categorical $\aleph_0$-stable version of this with Baur and Macintyre, with all of us very much under the influence of the then recent book of Kegel and Wehrfritz [34], I came across the idea of introducing a purely algebraic version of connectivity using a minimum condition (this is found on page 97). I don't think the idea of trying to fake geometry on such a basis had ever crossed my mind prior to that.

Well, enough chatter. What I actually want to discuss is the status of the *Borovik program* relating to the Algebraicity Conjecture, whose characteristic features are (a) a heavy use of the methods of finite group theory, as applied in the classification of the finite simple groups, and (b) a focus on the 2-local structure, and specifically on the structure of a 2-Sylow subgroup itself, within a putative (minimal) counterexample to the Algebraicity Conjecture. To this list one might reasonably add (c) the exclusion, or at least the separation, whenever warranted, of the pathologies associated with "bad fields." At this point we have managed to emancipate ourselves to a considerable degree from that last provision, while not entirely renouncing it. Readers unfamiliar with the jargon of the subject will need to look into the next section to completely decode what follows, though I will define some of the more obscure terms as I go along. I should also discharge Borovik from any direct responsibility for the specific formulations that follow. His own account can be found in the survey [18].

In its simplest formulation, the Borovik program takes as its objective the following special case of the Algebraicity Conjecture:

(*) A tame simple K^*-group of finite Morley rank which is not a “bad group” is algebraic.

I use the term “tame” here to mean “involving no bad field”; the definition commonly in use in the literature is more restrictive, including a restriction on bad groups as sections.

Tameness was invoked critically, and uncritically, in the first papers written in direct pursuit of this program [17, 1, 5, 6]. One use in [5] was slightly frivolous: tameness was invoked in connection with the theory of central extensions of Chevalley groups. We knew that this was unnecessary, but its removal takes a bit of model theory (a genericity result of Newelski and Wagner) and a bit of K-theory, and when one is working in the tame context this is all irrelevant, so we dealt with that point in a separate article on the side [13]. Other applications of tameness were more substantial, but fortunately Jaligot found ways around the tameness arguments of [1] and [6] early on, and in later work building on those two papers we conscientiously avoided any further use of the tameness hypothesis, so it dropped out of a large part of the program at that point.

As far as Borovik's initial paper [17] is concerned, he made it plain that the use of tameness was restricted to a single argument, and in fact to a single line of that argument: he needed to know that a connected solvable group without involutions is nilpotent, a strong but easily derived consequence of tameness. This has finally been eliminated in work I will describe in section 6, so at this point I would like to declare the matter put to rest, but in fact Jaligot and I have managed to find another use for the tameness hypothesis in our paper [26], and in fact we make extravagant use of tameness in that paper. The upshot of this is that it remains useful to pay some attention to the tame case as such, within a broader framework.

I have still not formulated the non-tame version of the program, and I do not simply want to replace $(*)$ by

(**) A simple K^*-group of finite Morley rank which is not a "bad group" is algebraic.

The fact is, a tame simple K^*-group without involutions is a bad group, for unsatisfactory reasons that have been alluded to above, but we have very little understanding of simple K^*-groups without involutions, and I would not wish to import the problem of their classification into the Borovik program. A more reasonable formulation of the objective would be the following:

(†) A nonalgebraic simple K^*-group of finite Morley rank has *normal 2-rank* at most 2.

For me personally, the value of the bound is less significant than the existence of a bound. The normal 2-rank is the maximal rank—dimension over F_2—of an elementary abelian 2-group whose normalizer contains a Sylow 2-subgroup. Given what one knows a priori about Sylow 2-subgroups of groups of finite Morley rank, a bound on the normal 2-rank gives firm control of the structure of the Sylow 2-subgroup. As will be seen in section 4, when we go over the results in detail, we are not too far from (†), and so far we have been able to proceed without getting much involved in individual configurations. That kind of work is hidden, however, in some of the geometric classifications we call on via [35], and to some degree plays a role in the rank 2 amalgam method as well (see §4 below).

Now the K^* assumption which has been incorporated into everything above amounts to the assumption that one is considering a minimal counterexample. Altınel has made the audacious suggestion that we should try to prove some form of these results outright, e.g.:

(††) A simple group of finite Morley rank which is not algebraic has finite 2-rank.

(It is far too early to give any thought to particular bounds.)

With this, we leave the orbit of the Borovik program; our last version I call "Altınel's Jugendtraum." We two have been pursuing this together, and it is instructive. What we have found to date is that one can redo some parts of the theory, working around possible "degenerate type" sections, just as we have learned to work around bad fields, but that to do this we actually need some of the work of Wagner on bad fields and its consequences, namely [44] and [39]. Unfortunately, at this stage we are also being forced to learn to live largely without the elegant theory of solvable groups of finite Morley rank, which has been brought to its highest pitch of refinement in the work of Frécon, with consequences for the Borovik program in its K^*-form which may not be evident, as they are hidden in rather technical lemmas “proved on demand,” but which are certainly substantial.

I will say something about Altınel’s ideas in section 7, though not enough.

I lectured on this material on two occasions in the summer of 2002, once at Ravello, in celebration of young Angus Macintyre’s 60th birthday, and once at Hattingen, in commemoration of what would have been Reinhold Baer’s 100th, the conference subject being “Algebra, Geometry, and their interactions,” than which I would be hard pressed to find a more congenial one. I would like to dedicate this essay to both figures, and I would have liked to address myself equally to algebraists and model theorists here, but I have settled on the Ravello audience as my target—no doubt some algebraists will find the technical details, if anything, less off-putting than my intended audience may, but they will not find anything about the context in which a problem of this type arises. For that, one might look into the first pages of the text [22], or for another route into the subject, and a good deal of information about it, the survey [18] mentioned above.

Finally, I would like to mention three recent articles which together cover the same ground as the present one, in considerably more detail: [3, 4, 23]. The last of these contains a result on groups of odd type proved shortly before the Ravello meeting, which can be used to pull together the threads of three recent papers to give a significantly clearer picture of the odd type case; the version of the odd type results that I give here is taken from a draft of that preprint dated July, 2002. While that preprint is not intended as a survey, at least in the form I have it, it gives a very timely and succinct account of where we stand at present, along with the missing ingredient needed to get us there.

2. Background

I have split my discussion of background material into two sections, as some of the background material provides an essential framework for the whole discussion. That material is found here; I hope it has a familiar look. In the next section (which perhaps should really be an appendix) I will run over some far more technical matters which become relevant at specific points in the discussion.

I would recommend reading this section if the terminology is not completely familiar, and skipping the next (which should not in any case be familiar) till it is needed.

2.1. The four types

A *Sylow p-subgroup* of a group of finite Morley rank is a maximal p-subgroup. In algebraic groups the structure of such groups depends primarily on the characteristic of the base field. In characteristic p these groups are unipotent, and are in particular Zariski closed (hence definable) and of bounded exponent; in other characteristics they are finite extensions of the p-parts of maximal tori (which are, in particular, divisible abelian). Something similar ought to happen in groups of finite Morley rank, if one believes the Algebraicity Conjecture, and does happen for $p = 2$, regardless of one's theology. However bearing in mind that there are groups of finite Morley rank of "mixed characteristic"—any direct product of finitely many algebraic groups over algebraically closed fields of variable characteristic will do—the following is the most one could expect.

Theorem 2.1. **[24]** *Sylow* 2*-subgroups of a group of finite Morley rank are conjugate, and if S is the connected component of one such, then it has the following structure:*

$$S = U * T;$$

namely, a central product with finite intersection of factors U, T which are, respectively, (1) a 2-unipotent *group, that is a definable connected nilpotent* 2*-group of bounded exponent, and (2) a* 2-torus, *that is a divisible abelian* 2*-group.*

We then have, correspondingly, the following four types of groups of finite Morley rank, depending on whether the factor U or T is actually present (nontrivial):

2-Sylow° structure in groups of FMR:
Types

T \ U	$\neq 1$	$= 1$
$\neq 1$	Mixed	Odd
$= 1$	Even	Degenerate

$S = U * T$:
2-Unipotent · 2-Torus
(finite intersection)

What is wrong with this picture?

It would be tempting to leave to the end the natural query, "What ever happened to the alternating groups?" The fact is, they have just quit the scene—there is no place for any sort of analog of the alternating groups in the table above. This is just as well.

In the simple case, one would like to prove that mixed and degenerate types do not occur, and that even and odd types are algebraic, with algebraically closed base field of characteristic 2 in the former case, and not 2 in the latter. There are substantial results going in this direction, if one restricts attention to K^*-groups.

2.2. K-groups and K^*-groups

Definition 2.1. *Let G be a group of finite Morley rank.*

1. *A* section *of G is a quotient H/K with $K \lhd H \leq G$; it is* definable *if H and K are.*
2. *G is a K-*group *if every connected simple definable section of G is algebraic.*
3. *G is a K^*-*group *if every proper connected simple definable section of G is algebraic.*

So a K^*-group either is a K-group, or else is simple and constitutes a minimal counterexample to the Algebraicity Conjecture.

Definition 2.2. *A connected simple group G of finite Morley rank is called "minimal connected simple" if it has no proper definable connected simple section.*

One may wonder why the foregoing "definition" is given at all; the point is only that "minimal connected simple" and "connected minimal simple" turn out to be distinct concepts, and the former is the important one. For minimal connected simple groups, the expected result according to the Algebraicity Conjecture is that we have a group of type PSL_2. The most extreme counterexample to this would be furnished by the well known, but entirely hypothetical, "bad groups."

Observe that all connected simple K^*-groups of finite Morley rank of degenerate type are minimal connected simple.

2.3. Bad groups and bad fields

A *bad group* is a connected simple group of finite Morley rank such that every proper definable connected subgroup is nilpotent. Another way to phrase this, and one which probably is to be preferred, is as follows: a bad group is a connected simple K^*-group whose Borel subgroups are nilpotent. Such groups are minimal connected simple, and a beautiful application of the geometry of involutions shows that they contain no involutions. In particular they are of degenerate type. It may well be that the emphasis on the structure of the Borel subgroups is not quite what one wants; the essential issue seems to have more to do with the pattern of intersections among various Borels, and the number of distinct conjugacy classes of Borels. This matter has been taken up by Jaligot, but the subject remains unclear. What is at stake is some rational way of dealing with degenerate type groups as a class. One wants not only to control the Sylow 2-subgroup in such groups (which one can very likely do) but also to get some sort of limitation on the complexity of the geometry of the collection of Borel subgroups.

A *bad field* is a structure of the form $(K; T)$ where K is a field, T is an infinite proper subgroup of its multiplicative group, and the structure has finite

Morley rank. Bad fields are prominently visible, though unnamed, in early work of Zilber on connected solvable groups of finite Morley rank.

A bad field $(K;T)$ is said to be *involved* in a group G if G has definable sections $(K_0;T_0)$ such that

1. Conjugation in G induces an action of T_0 on K_0;

2. There is an isomorphism $(K;T) \simeq (K_0;T_0)$.

This isomorphism requires some elucidation. The meaning is that the group structures on K and T correspond to the group structures on K_0 and T_0, and at the same time the action of T on K by multiplication corresponds to the action of T_0 on K_0.

In some ways, bad fields are not that different from the finite fields that finite group theorists are forced to work with, such as $K = F_2$, with T equal to the multiplicative group, but unfortunately also trivial. In our case, though, the difficulties lie at the opposite extreme, and particularly in the case in which T is torsion free.

We have learned a great deal about bad fields in the last ten years or so. On the one hand, work of Wagner and Poizat suggests that they probably do exist in characteristic 0, and probably do not exist in positive characteristic. The latter, if true, would simplify a number of issues; as of the present writing, though, this question seems to be tied up with deep number theoretic problems. On the other hand, we have also learned to work around bad fields quite efficiently, notably in Jaligot's thesis [33], and the effects of this will be clear in the next section, when we present concrete results.

On the third hand, in the juggler's sense, one of Wagner's results on bad fields [44], and a consequence derived by Poizat [39], gives us new techniques for neutralizing the effects of bad fields without simply wishing them away, and these appear to be essential for the pursuit of Altınel's Jugendtraum (section 7).

Notwithstanding the parallelism in the terminology, which is perhaps regrettable, it was clear enough at the start that bad groups and bad fields are horses of distinct colors, and this has become clearer as time goes on. Bad groups are representative of the type of situation in which the methods of finite group theory offer no purchase (no involutions, and TI-sets but no character theory), while bad fields should be considered a complication whose significance needs to be evaluated, and in some specific cases there remains much to be said, in practice, for working out proofs first under the assumption that no bad fields are involved, and later revisiting the general case. This will be amply illustrated in later sections.

Definition 2.3. *A group of finite Morley rank is* tame *if it involves no bad field.*

This is not quite the way the term is used in the literature, but this usage has much to recommend it, and will be adopted here.

At this point, please go on to section 4 for a statement of the main results to date.

3. Technicalities

Ahem.

At this point, please go to section 4 for a statement of the main results to date.

This section is a misplaced appendix. One will need to be aware of various 2-ranks, and the 2-generated core, to completely decode the details of odd and degenerate type groups in the next section, and we will eventually launch into a discussion of Burdges' signalizer functor theorem with noticeable relish, but this section can be used for reference as needed. I have tossed in here everything that I could not stomach seeing in its proper place, and it should not make a great deal of sense on its own.

3.1. Notions of 2-rank

The term "rank" becomes overloaded. We have Morley rank, Lie rank, Tits rank, and various p-ranks (mainly for $p = 2$), all of which are relevant at one point or another. Normal 2-rank is relevant in both odd and degenerate type groups, and Prüfer 2-rank is relevant to odd type.

Definition 3.1.

1. *The* p-rank *of an elementary abelian p-group is its dimension as a vector space over F_p. The p-rank of a general group is the supremum of the p-ranks of its elementary abelian p-subgroups. Notation: $m_p(G)$.*

2. *The* normal p-rank *of G is the supremum of the p-ranks of those elementary abelian p-subgroups which are normal in a Sylow p-subgroup. Notation: $n_p(G)$.*

3. *The Prüfer p-rank of G is the supremum of the p-ranks of divisible abelian p-subgroups of G (this, by the way, will be finite if G has finite Morley rank). Notation: $\mathrm{P}r_p(G)$.*

In a group G of finite Morley rank we will have

$$\mathrm{P}r_2(G) \le n_2(G) \le m_2(G).$$

In an algebraic group with algebraically closed base field, if p is different from the characteristic of that field, then $\mathrm{P}r_p(G)$ is the Lie rank, the dimension of a maximal torus. The parameter n_2 is finite if and only if the group is of odd or degenerate type, in which case m_2 is also finite.

3.2. The 2-generated core

This subsection, and more especially the next on signalizer functors, are needed to follow Borovik's strategy, both as in [17] and in more recent variations, for handling the odd type case; and they are equally relevant to the degenerate case. One can just about keep this material out of the statement of the results themselves, apart from unfinished business in degenerate type, but among other things it is deeply implicated in the most recent work on avoiding the tameness assumption.

It's quite possible to skip this subsection; the next one is more essential, but the notions treated here are relevant to understanding why that is.

Let G be a group of finite Morley rank, S a Sylow 2-subgroup, and $1 \le k \le \infty$, with k to be set equal to 2 eventually. Then the subgroup $\Gamma_{k,S}(G)$ is by definition the definable closure of the following subgroup of G:

$$\langle N_G(A) : A \le S, m_2(A) \ge k \rangle$$

We can drop the subscript S with a fairly clean conscience, as the various possibilities are conjugate: $\Gamma_k(G)$ then stands for a subgroup of G depending in fact on S, but well determined up to conjugacy in any case. The condition that interests us, for suitable k, is

$$(!_k) \qquad \Gamma_k(G) < G$$

Since Γ_k shrinks as k increases, the most stringent form of this condition corresponds to $k = 1$; in this case, it turns out, we have a condition equivalent to the existence of a "strongly embedded" subgroup (which we will not pause to define here). For $k = \infty$ we would have "weak embedding" if we were in the even type case, but we will be using this notation exclusively in connection with odd and degenerate type groups, and in those contexts the 2-rank $m_2(G)$ is finite, so for large k we will have $k > m_2(G)$, and therefore $\Gamma_k(G) = 1$, depriving $(!_k)$ of any potential interest.

The subgroup $\Gamma_k(G)$ is called the *k-generated core* of G, and the value that interests us happens to be $k = 2$, for technical reasons laid out in [17], and implicit (though hidden) in the discussion of signalizer functors below.

For any given value of k, condition $(!_k)$ is felt to be a stringent smallness condition on G, though to justify this feeling one has to do some very serious work, or else assume the Algebraicity Conjecture. As it happens, the condition $(!_2)$ is not actually met by any algebraic group of odd type, though one has to take a good look at PSL_2 to be sure of this [23].

From a practical point of view, $\Gamma_2(G)$ only enters the scene in the course of exploiting Gorenstein's signalizer functor method in the manner of [17]. So we will discuss that point next.

3.3. Signalizer functors

This seems to be one of the more obscure chapters in the theory of the finite simple groups, though one wonders how much of that is due to the obscurity of the name. Aschbacher managed to give a coherent account of the subject in the finite case in the last, and trickiest, chapter of his efficient and lucid text on finite groups [15, chap. 16]

I think it is helpful to bear in mind that what we will be dealing with is in no sense a functor, and as far as I see does not signal very much (it may signal the presence of some large normal subgroup in an ostensibly simple group, I suppose, or it may simply signal the need to pay careful attention at this point). For our purposes, as I shall explain, more often than not a signalizer functor will correspond to some notion of *unipotence*.

A *p-signalizer functor* θ associated to a group G (and, in practice, possibly some other data, such as a particular elementary abelian p-subgroup A of G—but let us leave this aside) will be a function which assigns to each element a of order p, a definable (and usually connected) subgroup $\theta(a)$ of $C_G(a)$, and which satisfies the following conditions, the last one being critical:

1. Invariance: $\theta(a^g) = \theta(a)^g$;
2. $p^\perp$: $\theta(a)$ is a $p^\perp$-group; that is, it contains no p-element.
3. Balance: $\theta(a) \cap C_G(b) = \theta(b) \cap C_G(a)$ when a, b are commuting elements of order p.

I don't find the symmetry of the balance condition particularly helpful, and I would prefer an equivalent form, either the weakest:

$$\theta(a) \cap C_G(b) \leq \theta(b)$$

or the strongest:

$$\theta(a) \cap C_G(b) = \theta(b) \cap C_G(a) = \theta(a) \cap \theta(b)$$

again, with a, b commuting elements of order p. In particular, the weak form will correspond in our context to a notion of "robustness". If one takes $\theta(a)$ to be, in some sense, the "unipotent radical" of $C_G(a)$, then the balance condition suggests that this notion of unipotent radical is in some way absolute—independent of the ambient group, under certain conditions. The second condition could also be interpreted as meaning that the characteristic is not p. We will give examples in subsection 6.

A signalizer functor θ is called nilpotent, solvable, trivial, connected, or finite, respectively, if all of the groups θ have the corresponding property; on the other hand it is *nontrivial* (disconnected, etc.) if *one* of them is. In finite group theory one can make good use of general solvable signalizer functors, and even of K-group signalizer functors. We appear to be less fortunate; in our context, what is wanted are *connected nilpotent* signalizer functors. This critical fact, which has been known to Borovik for a very long time, is exploited in a very clear way in [17], and has been taken up again in [23]. It is really the only point that needs to be retained here.

We have not actually stated as yet what it is that one wants to do with signalizer functors, or why $\Gamma_2(G)$ comes into the picture. We will go into this now.

Let θ be a p-signalizer functor, and A an elementary abelian p-group. Then one defines $\theta(A)$ as

$$\langle \theta(a) : a \in A \rangle$$

It is convenient to have θ connected at this point, thereby guaranteeing that $\theta(A)$ is also definable. The signalizer functor θ is said to be *complete* over A if the group $\theta(A)$ is again a $p^\perp$ group, and generates θ over A in the sense that

$$\theta(a) = \theta(A) \cap C_G(a)$$

for $a \in A$. (In this sense, $\theta(A)$ is a group whose existence is signalled by, and explains, the existence of θ.)

One has the following general result, which though fundamental is not well documented in the literature:

Theorem 3.1. *Let θ be a connected nilpotent p-signalizer functor over a group G of finite Morley rank. Then for any elementary abelian p-subgroup A of rank at least 3, θ is complete over A.*

A special case of this was given in [22], but the proof given took advantage of two special features of the situation to condense matters substantially: $p = 2$ there, which is not a problem from the point of view of the present survey, but in addition the group involved was assumed tame, which eliminates an issue which becomes relevant at a more general level. A full treatment of Borovik's nilpotent signalizer functor theorem is included in an article by Burdges [25].

For brevity, let us say that θ is *complete* if it satisfies the conclusion of the previous theorem: θ is complete over any elementary abelian p-subgroup of rank at least 3. The final point in this line of argument is the following.

Theorem 3.2. *Let G be a simple group of finite Morley rank with $n_2(G) \geq 3$, and let θ be a nontrivial complete 2-signalizer functor on G. Then $\Gamma_2(G) < G$.*

Details can be found in [17], and a detailed review of the theory is given in [23], showing how it can be combined with more recent work to give the results detailed in the following section.

3.4. The Thompson rank formula

This charming and quite elementary device for getting strong information about simple K^*-groups of finite Morley rank with at least two conjugacy classes of involutions applies only when configurations which should lead to contradictions have been reduced to their most extreme forms, and where one can therefore compute quite a lot of information, but when the method works, it delivers a good deal at an affordable price: either the exact rank of the group in question, or a sharp bound on it.

This can be worked out most simply in a group of even type which is known to have a *finite* number (at least two) of conjugacy classes of involutions. One selects two of these classes C_1, C_2 and one then somehow defines a map

$$\phi : C_1 \times C_2 \to I(G)$$

using basic facts about dihedral groups and their definable closures. The definition of ϕ is not much to the point at this particular moment. The main point is that since $I(G)$, the set of involutions in G, is the union of finitely many conjugacy classes, generically (i.e., on a generic subset of the domain of ϕ) the values of ϕ lies in one specific conjugacy class, which we will call C_3 (this may be one of the two we started with). We now have the following numerical data to deal with:

$$g = \mathrm{rk}(G);\ c_i = \mathrm{rk}(C_G(t)) \text{ for } t \in C_i;\ f = \mathrm{rk}(\phi^{-1}(t)) \text{ for } t \in C_3$$

Here "f" stands for "fiber rank".

Now bearing in mind that $\mathrm{rk}(C_i) = g - c_i$, and that $\mathrm{rk}(C_1 \times C_2)$ can be evaluated in two ways, either as $\mathrm{rk}(C_1)+\mathrm{rk}(C_2)$ or via the map ϕ as $\mathrm{rk}(C_3)+f$, we get

$$g = c_1 + c_2 - c_3 + f$$

In favorable cases we should know what c_1 and c_2 are, but we are less likely to be able to determine the class C_3 a priori, so we have to consider (and compute) various possibilities for the pair (c_3, f). If one has a very firm hand on the conjugacy classes of involutions one may arrive at a precise result, and in any case one can aim at an estimate.

This works well in some small groups of even type, and can also be used in odd type. At one stage it looked like a "silver bullet" for sufficiently well determined configurations, but about the time of Jaligot's thesis the view shifted. Jaligot set out to extend the tame case of mixed type groups to the general mixed type case, with the Thompson rank formula in his back pocket, but reached a contradiction while doing the fusion analysis that constitutes the natural run-up toward the Thompson rank formula. Since then other cases have emerged in which just messing around with involutions in a similar vein produces information that the Thompson rank formula would miss—even in the case in which all involutions are conjugate. The Thompson rank formula is not obsolete (it remains essential, as far as we can see) but it plays a less dominant role than we expected.

If at this point you still haven't read the next section, oh perverse reader (mon semblable, mon frère), please do.

4. Results

Theorem 4.1. *There is no simple K^*-group of finite Morley rank of mixed type.*

Fortunately. This was proved in [5] in the tame case, then in [31] (also in [33]) in general, at which point we became more circumspect in our use of tameness, and more or less committed to dropping its use in the even type context completely. It proved considerably harder to eliminate such uses as we had already made of tameness in that case, but Jaligot did that as well in [32] (also in [33]). Jaligot began with the idea of applying the Thompson rank formula to reach the final contradiction, but matters took a somewhat different course. (See subsection 3.4 for elaboration of this point.)

One may regret the hypothesis "K^*" in the above. Altınel has something interesting to say about that; see section 7.

The mixed type case succumbed relatively rapidly. The next was far more troublesome.

Theorem 4.2. *A simple K^*-group of even type is algebraic.*

I'll go over the general proof strategy in the next section. This proof may, among other things, serve as a metaphor for how one proves such things in the finite case—or how one might prove them if there were no sporadic groups or any other messiness involved (quadratic extensions of the base field create as much mischief as sporadic groups do—it is not simply a question of pathology).

The proof is short by the standards of finite group theory, but long by conventional standards, and long enough to raise some of the same issues that the unreasonably long proofs in finite group theory pose—namely, at what point exactly is it reasonable to consider that a theorem has been proved? One doesn't want to belabor this point, but it merits belaboring.

In this particular case, not everything submitted is in print at this time, and two papers have not been submitted (one is being polished, and the second one is waiting on the first). We also rely indirectly on still unpublished work of Tits and Weiss, hopefully to appear in the near future; we also feel we can work around this to bring the proof within the orbit of the fully published literature, but have not as yet taken the trouble to do so. *Added in proof:* this work appeared in late 2002 [43].

At the same time, the route to the end of the proof became quite clear some time ago, once [12] reached its final form; I think as far as our initial strategy was concerned, that might have corresponded to the half-way point, but we came under the influence of the amalgam method at a propitious moment and made a quick dash to the end. In retrospect the key result is a version of Aschbacher's global $C(G,T)$ theorem, the last in a long series of characterizations of SL_2. This is achieved in the series of papers [1, 6, 13, 32, 7, 8, 12, 9, 10, 11], making use of [35] toward the end. Details in the next section.

The two remaining types, degenerate and odd type, have a good deal in common. They are characterized by the condition $m_2(G) < \infty$, and degenerate type is picked out by the additional condition $\mathrm{P}r_2(G) = 0$.

The situation for these types has been clarified recently, and is summed up in [23].

Theorem 4.3. *Let G be a simple K^*-group of finite Morley rank and of odd type. Then one of the following occurs.*

1. *G is algebraic, over an algebraically closed field of characteristic not 2;*
2. *$\mathrm{P}r_2(G) \leq 2$;*
3. *G is a minimal connected simple group.*

One would like to sharpen both the second and third condition to

$$n_2(G) \leq 2,$$

and perhaps also to $\mathrm{P}r_2(G) = 1$. This may involve the treatment of some individual cases; so far, one has been able to argue on rather general lines. The third alternative in particular is unsatisfactory, but goes away in the tame case:

Theorem 4.4. *Let G be a minimal connected simple group of finite Morley rank and of odd type. If G is tame, then $\mathrm{P}r_2(G) \leq 2$.*

This is proved in [26], making the most extravagant use of tameness to date. Without tameness, at present there is no known bound on $\mathrm{P}r_2(G)$. This is a major gap—the major gap—and stands out more plainly now that the situation in odd type has been largely clarified.

We come at last to degenerate type, that is the case in which the Sylow 2-subgroup is finite. The methods used for odd type show the following in this case [25].

Theorem 4.5. *Let G be a simple K^*-group of degenerate type. Then one of the following occurs:*

1. *$n_2(G) \leq 2$;*
2. *$\Gamma_2(G) < G$.*

Again, one would like to fold the second possibility back into the first. That may well be possible.

In the tame case, the only possibility is that G is a bad group, since in the absence of bad fields it follows from Zilber's Field Theorem that the Borel subgroups are nilpotent. In particular such a group has no involutions. All of this is a bit too glib, and in the degenerate case tameness unfortunately cuts out the real issues. However, the net result of all the above is the following.

Theorem 4.6. *Let G be a tame simple K^*-group of finite Morley rank. Then one of the following occurs.*

1. *G is algebraic (a Chevalley group, and identified as such);*
2. *G is a bad group, and in particular has no involutions;*
3. *G is of odd type, and has Prüfer 2-rank 1 or 2.*

In the last case mentioned, one would probably expect the group in question to be minimal connected simple. In that case, the possibilities have been delineated in [26]. Several fairly explicit configurations arise even in this tame case, which may prove quite resistant to our current methods, including the intriguing possibility of a "generically desarguesian" generically projective plane.

5. Even type

Accounts of this case can be found in [3] and [10]. In my discussion below I use a large number of technical notions not defined here, as I am aiming mainly at a sense of the shape of the proof and the relationship of certain points with the finite case. In later sections I will be discussing matters of considerably less breadth in considerably more detail.

The idea of the proof is as follows. One first defines notions of *Borel subgroup* and *minimal parabolic subgroup*—normally Borel subgroups have been defined in the usual way as maximal connected solvable subgroups, but a different and more restrictive definition is used in this case. Then one fixes a Borel subgroup B, and one considers the subgroup G^* generated by the proper minimal parabolic subgroups which contain B. One then considers the following three possibilities:

Thin $G^* < G$;

Quasithin G is generated by two minimal parabolic subgroups containing B;

Generic Neither of the above.

The target in each case is an identification of G with a Chevalley group over an algebraically closed field of characteristic 2; the three cases correspond, respectively, to Lie ranks 1, 2, or ≥ 3. In the thin case one wants $G \simeq \mathrm{SL}_2$, and ultimately this goes back to explicit computations [27, 21]. In the other two cases one is able to invoke general recognition theorems. At the moment we favor a geometrical approach, though the possibility also exists of arguing directly in terms of a pattern of "root SL_2" subgroups, which would bring out more strongly the role of the identification of SL_2.

For the thin case one needs the papers [1, 6, 32, 7, 8]; more precisely, one needs the last of these papers, but they form a linear sequence of gradually broader results (with [13] needed in the background). The quasithin case is treated in [12, 9, 10] using [35], and the generic case comes from [19], again combined with [35], with the whole thing put together in [11].

A central role is played by an analog of Aschbacher's global $C(G,T)$ theorem, which in our context is essentially the same thing as the statement for the thin case, but plays a broader role in the proof. Identification theorems for SL_2 are used in two ways. One of these, naturally enough, is as identification theorems for SL_2. The other and broader role is as a versatile source of contradictions, when configurations which are obviously much larger than SL_2 are forced to a contradiction via an argument showing that they must, nonetheless, reduce to SL_2. As far as I can see, the fact that our main technical device is also a distinct subcase is a coincidence.

The sequence of steps leading up to the global $C(G,T)$ theorem go under the names of strong embedding, weak embedding, strongly closed abelian subgroups, and pushing up, all well-established notions in the theory in the finite case, which turn out to be very judiciously calibrated also in our context. In the finite case, there are other equally important notions that would come into play along the way, which vanish from the scene in our context. One could choose to view the notions we use as the "skeleton" of the finite proof, which after all also aims, typically, at an identification of the given group with a group of Lie type. This makes for an interesting "reading" of the existing arguments.

As far as the proofs are concerned, it rapidly becomes clear that we are living in a different category, sandwiched somewhere between the finite and algebraic in spirit, though of course not literally. Connectedness plays a hugely prominent role throughout, which is a bit strange when one is tracking ideas coming from the finite case. Toward the end, as one reaches pushing up, one rejoins the standard lines of argument.

As I have said, I am not going to define the various notions involved here, as my intent is just to give a sense of the shape of this part of the argument, and its relation to the finite case; and, above all, the fact that the "thin" case requires the most lengthy analysis.

The quasithin case is treated as a "rank 2 amalgam" problem in the sense of Delgado and Stellmacher [29], but more in the version of [42], which fits our context more closely. The adaptation of that method to our case is discussed in some detail in [10]. There is a technical point that must be verified before one uses such methods, which is dealt with in [12], arguing directly from the global $C(G,T)$ theorem. That brief article somehow sidesteps a large chapter of group theory that comes into play in the finite case.

In [12] we sharply emancipate ourselves from the standard approach, and work from that point on with so-called "third generation" techniques, which are under active development in the finite case—but we make do quite nicely with their most classical form. These techniques rely on a bit of representation theory, which is not at all in good shape in our category—but it turns out that we need very little of it in our particular case.

As for the generic case, this can be handled by combining an analog of a theorem of Niles due to Berkman and Borovik [19] with a classification theorem of Kramer, Tent, and van Maldeghem [35].

6. Burdges' Signalizer Functor Theorem

Now for something completely different: odd and degenerate type.

Borovik provided a surprisingly powerful “trichotomy theorem” for tame simple K^*-groups of odd type in [17], suggesting that the non-algebraic ones are all small in one sense or another; later work with Berkman confirmed this suggestion. However the worm of tameness got into the apple rather early and apparently quite deeply. In fact as Borovik emphasized, it was needed at only one specific point, in the following form:

Connected solvable groups without involutions are nilpotent.

This is, however, a strong form.

The situation has been clarified in a number of respects recently, dealt with in [20, 26, 25, 23], and summarized in [23]. The first of these offers a more efficient notion of the “generic” algebraic case; the second deals with the minimal connected simple case, but only in the tame setting; the third supplies the technical point needed to eliminate tameness from [17]. This motley crew is brought into some degree of order by [23], leading to the picture as given in section 4.

In thinking about the odd case, one is led to focus on the following condition, where $O(H)$ stands for the maximal connected normal definable subgroup without involutions (the “core”) in a given group H.

(*no-core*) $\qquad O(C(i)) = 1$ for i any involution.

One point is that this happens “typically.” There are two issues: (1) the classification of the non-typical cases; (2) a proof that this, again “typically,” implies algebraicity. Two approaches to the latter are found in [17, 20]. For the former, the canonical approach remains the one given initially in [17] via signalizer functors (and inherited from similar but far messier considerations in the finite case). The signalizer functor in question is defined by

$$\theta(i) = O(C(i))$$

Under a tameness hypothesis, as indicated earlier, this is a nilpotent signalizer functor, and the technical machinery sketched in section 3.3 kicks in. (This discussion is illuminated by the details in [23] and the early sections of [17].) To summarize, if $O(C(i))$ is ever nontrivial, one has a nontrivial signalizer functor, and for $n_2(G)$ large enough one can use the "completeness" of the functor to force the 2-generated core $\Gamma_2(G)$ to normalize a proper subgroup, and hence itself be proper. So the "atypical" cases turn out to be $n_2(G) \leq 2$ and $\Gamma_2(G) < G$, and the latter can be pushed further, as shown in [23].

All of this falls apart in the non-tame case as $O(C(i))$ need not be nilpotent; the rest of the argument has no relationship to tameness.

To bridge the gap, Burdges shows [25] that nontrivial solvable signalizer functors produce nilpotent ones. The idea is to replace $O(C(i))$ by $U(C(i))$ where U stands for the "unipotent radical" of $O(C(i))$, in a suitable sense. This is an old idea, but one that did not seem to work. The obvious notion of "unipotence" is p-unipotence for p an odd prime: let $U_p(H)$ be the largest connected definable nilpotent p-subgroup of bounded exponent (ideally, one should prove nilpotence where possible rather than building it into the definition, but I say it this way for the sake of simplicity). This will give a signalizer functor as well, but it may be trivial, and one obvious problem is that there is a torsion free case corresponding to $p = 0$.

It turns out that one can define a suitable though not particularly natural characteristic 0 notion of unipotence that applies here, and thus derive another nilpotent signalizer functor:

$$\theta_0(i) = U_0(C(i))$$

In the presence of bad fields (K, T) of characteristic 0 this is an implausible idea, but one exploits the following properties of such fields:

1. K_+ is indecomposable
2. $\mathrm{rk}(T) < \mathrm{rk}(K)$

There still remains the real possibility that $U_p(C(i)) = 1$ for all p, including $p = 0$, while $O(C(i)) \neq 1$. In this case one shows that $O(C(i))$ is an abelian, and hence nilpotent, signalizer functor. So, one way or another, one can extract a nilpotent signalizer functor from the original solvable one.

Just before Borovik's early use of tameness was eliminated, Jaligot and I again found ourselves relying on tameness, so as explained in section 4 our understanding of odd type simple K^*-groups in general still lags behind the tame case.

Note added in proof: Chapter 10 of Burdges' thesis (Rutgers, 2004) corrects an inaccuracy in the above.

7. Altınel's Jugendtraum

Would it be possible to prove the finiteness of the set of sporadic simple groups without proving the Odd Order Theorem? Presumably not— a more interesting question is whether one could bound the 2-rank of sporadic groups without classifying them, and without the Odd Order Theorem. In any case, that is the project Altınel has proposed in our context (or in any case, that's what it sounds like to me). I would formulate the challenge as follows:

(J) Prove that a nonalgebraic simple group of finite Morley rank has finite 2-rank.

In other words, eliminate even and mixed types. Note that the K^*-case of this is known. As we have been relying almost entirely on the K^* hypothesis for some time, it comes as something of a surprise to me that one can redo at least some portions of the analysis in a different way, relying on Wagner's results on fields of finite Morley ranks, and the consequences for linear groups found by Poizat. Altınel has shown that the following strong form of the elimination of mixed type groups goes through.

Theorem 7.1. *Suppose that every simple group of finite Morley rank of even type is algebraic. Then there is no simple group of finite Morley rank of mixed type.*

So we need think no more about the mixed type case; the challenge is to deal with even type.

We still want to work in an inductive framework, so we introduce the following notion.

Definition 7.1. *Let G be a group of even type. Then G is an L-group if every definable simple section of G of even type is algebraic; L^*-groups are defined similarly, taking proper sections.*

Evidently, it suffices to deal with even type L^*-groups. Here is a striking and encouraging result by Altınel that can be proved at the outset.

Theorem 7.2. *Let G be an L-group of finite Morley rank and even type. Let $B(G)$ be the subgroup of G generated by unipotent 2-subgroups. Then $B(G)$ is a K-group.*

This looks almost like a "magic bullet" for the problem. It falls short of that, but on the other hand it definitely gets the ball rolling.

So far, proofs in this area seem rather laborious compared to their K^*-antecedents, and one might expect severe problems early on in the analysis. At this point, we have looked into the strong and weak embedding problems. We can for example state the following [14].

Theorem 7.3. *Let G be a simple L^*-group of finite Morley rank with a strongly embedded subgroup M. Let S be a Sylow 2-subgroup of G, and $A = \Omega_1(S)$ (the subgroup of S generated by its involutions). Then there is a subgroup $L \leq G$ of the form SL_2 with $A \leq L$. If $L < G$ then $C_G(L)$ is infinite.*

This corresponds closely with the first major step in the analysis of the K^*-case, which after some preliminaries divides into two major cases, the first distinctly easier than the second. At the moment I don't feel up to the task of writing something sensible about what we have learned about the proper use of Wagner's theorem on fields of finite Morley rank in this context. After the strong and weak embedding cases, one would next encounter "strongly closed abelian subgroups," where Borovik found a beautiful shortcut (the theory of pseudo-reflection groups). If this idea does not go over to the L^* case, then we may have to resurrect some parts of the finite analysis which we have been spared to date.

8. By way of a conclusion

The main goals of the Borovik program, at least as I have understood them, have now been met in the tame case; in the general case we still need at least a bound on Prüfer rank for minimal connected simple groups. While on the whole the Algebraicity Conjecture still seems reasonable to me, I am certainly prepared to see sporadics, both of Hrushovski's type (in which case one would expect them to come in the form of bad groups) and even, conceivably, in nature, though the latter possibility seems to be receding, since the Borovik program would be one of the most plausible ways of rounding such things up, or at least detecting their silhouettes. One certainly does not see any particularly plausible configurations lurking in the shadows from the point of view of this analysis, though some of the unruly geometries described in [26] need to be looked at. I never thought it at all likely one would turn up, but Pascal's (or Nasreddin Hoca's) wager gives the thought some weight. At this point we have not encountered many configurations that seem worth thinking about in their own right, certainly none as compelling as bad groups and fields.

What one would really like to know along the present lines is one of the following: (a) any counterexample to the Algebraicity Conjecture involves a bad group (we may not have the right notion of "bad", but one wants a comparably compelling notion); (b) there is an absolute limit to the height of counterexamples, the height being the height of the partial order of definable connected simple sections, under involvement. Even controlling the 2-rank (absolutely) seems sufficiently ambitious.

Even in their most extreme forms, bad groups of rank 3—most plausibly torsion free, but conceivably even of bounded exponent—remain a real possibility. We know through work of Sela (in course of publication, and currently archived at `http://www.ma.huji.ac.il/~zlil`) that free groups have reasonable definability properties, and at present one expects that their theories are stable. I read this as an indication that there may be some well-behaved simple $\aleph_0$-stable groups constructible by a variant of Hrushovski's methods, but that the combinatorial group theory involved would be deep. Some time ago, one of the leading practitioners of that fine art cautioned me that "most groups exist"; a day later, after examining the axioms, he continued, "but perhaps not these." And so there we are.

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Note: Papers not in print may be available at `http://math.rutgers.edu/~cherlin/FMR`.