Dear Mr. Faltings,

Many thanks for your quick answer and for sending me your reprints! Your comments on the so-called "Theory of Motives" are of the usual kind, and for a large part can be traced to a tradition which is deeply rooted in mathematics. Namely that research (possibly long and exacting) and attention is devoted only to mathematical situations and relations for which one entertains not merely the hope of coming to a provisional, possibly in part conjectural understanding of a hitherto mysterious region – as it has indeed been and should be the case in the natural sciences – but also at the same time the prospect of a possibility of permanently supporting the newly gained insights by means of conclusive arguments. This attitude now appears to me as an extraordinarily strong psychological obstacle to the development of the visionary power in mathematics, and therefore also to the progress of mathematical insight in the usual sense, namely *the* insight which is sufficiently penetrating or comprehending to finally lead to a "proof". What my experience of mathematical work has taught me again and again, is that the proof always springs from the insight, and not the other way round – and that the insight itself has its source, first and foremost, in a delicate and obstinate feeling of the relevant entities and concepts and their mutual relations. The guiding thread is the inner coherence of the image which gradually emerges from the mist, as well as its consonance with what is known or foreshadowed from other sources – and it guides all the more surely as the "exigence" of coherence is stronger and more delicate.

To return to Motives, there exists to my knowledge no "theory" of motives, for the simple reason that nobody has taken the trouble to work out such a theory. There is an impressive wealth of available material both of known facts and anticipated connections – incomparably more, it seems to me, than ever presented itself for working out a physical theory! There exists at this time a kind of "yoga des motifs", which is familiar to a handful of initiates, and in some situations provides a firm support for guessing precise relations, which can then sometimes be actually proved in one way or another (somewhat as, in your last work, the statement on the Galois action on the Tate module of abelian varieties). It has the status, it seems to me, of some sort of secret science – Deligne seems to me to be the person who is most fluent in it. His first [published] work, about the degeneration of the Leray spectral sequence for a smooth proper map between algebraic va-
$\frac{1}{2}$ rieties over $\mathbb{C}$, sprang from a simple reflection on "weights" of cohomology groups, which at that time was purely heuristic, but now (since the proof

of the Weil conjectures) can be realised over an arbitrary base scheme. It is also clear to me that Deligne's generalisation of Hodge theory finds for a large part its source in the unwritten "Yoga" of motives – namely in the effort of establishing, in the framework of transcendent Hodge structures, certain "facts" from this Yoga, in particular the existence of a filtration of the cohomology by "weights", and also the semisimplicity of certain actions of fundamental groups.

Now, some words about the "Yoga" of anabelian geometry. It has to do with "absolute" alg. geometry, that is over (arbitrary) ground fields which are finitely generated over the prime fields. A general fundamental idea is that for certain, so-called "anabelian", schemes X (of finite type) over K, the geometry of X is completely determined by the (profinite) fundamental group $\pi_1(X,\xi)$ (where ξ is a "geometric point" of X, with value in a prescribed algebraic closure $\overline{K}$ of K), together with the extra structure given by the homomorphism:

$$\pi_1(X,\xi) \to \pi_1(K,\xi) = \mathrm{Gal}(\overline{K}/K). \tag{1}$$

The kernel of this homomorphism is the "geometric fundamental group"

$$\pi_1(\overline{X},\xi) \qquad (\overline{X} = X \otimes_K \overline{K}), \tag{2}$$

which is also the profinite compactification of the transcendent fundamental group, when $\overline{K}$ is given as a subfield of the field $\mathbb{C}$ of the complex numbers. The image of (1) is an open subgroup of the profinite Galois group, which is of index 1 exactly when $\overline{X}$ is connected.

The first question is to determine which schemes X can be regarded as "anabelian". On this matter, I will in any case restrict myself to the case of non-singular X. And I have obtained a completely clear picture only when $\dim X = 1$. In any case, being anabelian is a purely geometric property, that is, one which depends only on $\overline{X}$, defined over the algebraic closure $\overline{K}$ (or the corresponding scheme over an arbitrary algebraically closed extension of $\overline{K}$, such as $\mathbb{C}$). Moreover, $\overline{X}$ should be anabelian if and only if its connected components are. Finally (in the one dimensional case), a (non-singular connected) curve over $\overline{K}$ is anabelian when its Euler-Poincaré characteristic is < 0, in other words, when its fundamental group is not abelian; this latter formulation is valid at least in the characteristic zero case, or in the case of a proper ("compact") curve – otherwise, one should consider the "prime-to-p" fundamental group. Other equivalent formulations: the group scheme of
$\frac{2}{3}$ the automorphisms should be of dimension zero, or still the automorphism group should be finite. For a curve of type (g,ν), where g is the genus, and ν the number of "holes" or "points at infinity", then the anabelian curves

are exactly those whose type is not one of

$$(0,0),(0,1),(0,2) \text{ and } (1,0)$$

in other words

$$2g+\nu>2 \quad (\text{i.e. } -\chi=2g-2+\nu>0).$$

When the ground field is $\mathbb{C}$, the anabelian curves are exactly those whose (transcendent) universal cover is "hyperbolic", namely isomorphic to the Poincaré upper half plane – that is, exactly those which are "hyperbolic" in the sense of Thurston.

In any case, I regard a variety as "anabelian" (I could say "elementary anabelian"), when it can be constructed by successive smooth fibrations from anabelian curves. Consequently (following a remark of M.Artin), any point of a smooth variety X/K has a fundamental system of (affine) anabelian neighbourhoods.

Finally, my attention has been lately more and more strongly attracted by the moduli varieties (or better modular <u>multiplicities</u>) $M_{g,\nu}$ of algebraic curves. I am rather convinced that these also may be approached as "anabelian", namely that their relation with the fundamental group is just as tight as in the case of anabelian curves. I would assume that the same should hold for the multiplicities of moduli of polarized abelian varieties.

A large part of my reflections of two years ago were restricted to the case of char. zero, an assumption which, as a precaution, I will now make. As I have not occupied myself with this complex of questions for more than a year, I will rely on my memory, which at least is more easily accessible than a pile of notes – I hope I will not weave too many errors into what follows! A point of departure –among others– was the known fact that for varieties X, Y over an algebraically closed field K, when Y can be embedded into a [quasi-]abelian variety A, a map $X \to Y$ is determined, up to a translation of A, by the corresponding map on H_1 (ℓ-adic). From this, it follows in many situations (as when Y is "elementary anabelian"), that for a dominant
$\frac{3}{4}$ morphism f (i.e. $f(X)$ dense in Y), f is known exactly when $H_1(f)$ is. Yet the case of a constant map cannot obviously be included. But precisely the case when X is reduced to a point is of particular interest, if one is aiming at a "characterization" of the points of Y.

Going now to the case of a field K of finite type, and replacing H_1 (namely the "abelianised" fundamental group) with the full fundamental group, one obtains, in the case of an "elementary anabelian" Y, that f is known when $\pi_1(f)$ is known "up to inner automorphism". If I understand correctly, one may work here with the quotients of the fundamental group which are obtained by replacing (2) with the corresponding abelianised group $H_1(\overline{X}, \hat{\mathbb{Z}})$, instead of with the full fundamental group. The proof follows rather easily from the Mordell-Weil theorem stating that the group $A(K)$ is a finitely generated $\mathbb{Z}$-module, where A is the "jacobienne généralisée" of Y, corresponding to the "universal" embedding of Y into a torsor under a quasi-abelian variety. Here the crux of the matter is the fact that a point of A over K, i.e. a "section" of A over K, is completely determined by the corresponding splitting of the exact sequence

$$1 \to H_1(\overline{A}) \to \pi_1(A) \to \pi_1(K) \to 1 \tag{3}$$

(up to inner automorphism); in other words by the corresponding cohomology class in

$$H^1(K, \pi_1(\overline{A})),$$

where $\pi_1(\overline{A})$ can be replaced by the ℓ-adic component, namely the Tate module $T_\ell(\overline{A})$.

From this result, the following easily follows, which rather amazed me two and a half years ago: let K and L be two fields of finite type (called "absolute fields" for short), then a homomorphism

$$K \to L$$

is completely determined when one knows the corresponding map

$$\pi_1(K) \to \pi_1(L) \tag{4}$$

of the corresponding "outer fundamental groups" (namely when this map is known up to inner automorphism). This strongly recalls the topological intuition of $K(\pi, 1)$ spaces and their fundamental groups – namely the homotopy classes of the maps between the spaces are in one-to-one correspondence with the maps between the outer groups. However, in the framework of absolute alg. geometry (namely over "absolute" fields), the homotopy
4/5 class of a map already determines it. The reason for this seems to me to lie in the extraordinary rigidity of the full fundamental group, which in turn springs from the fact that the (outer) action of the "arithmetic part" of this group, namely $\pi_1(K) = \mathrm{Gal}(\overline{K}/L)$, is extraordinarily strong (which is also reflected in particular in the Weil-Deligne statements).

The last statement ("The reason for this...") came quickly into the typewriter – I now remember that for the above statement on field homomorphisms, it is in no way necessary that they be "absolute" – it is enough that they should be of finite type over a common ground field k, as long as one restricts oneself to k-homomorphisms. Besides, it is obviously enough to restrict attention to the case when k is algebraically closed. On the other hand, the aforementioned "rigidity" plays a decisive role when we turn to the problem of characterizing those maps (4) which correspond to a homomorphism $K \to L$. In this perspective, it is easy to conjecture the following: when the ground field k is "absolute", then the "geometric" outer homomorphisms are exactly those which commute with the "augmentation homomorphism" into $\pi_1(K)$. [see the correction in the PS: the image must be of finite index] Concerning this statement, one can obviously restrict oneself to the case when k is the prime field, i.e. $\mathbb{Q}$ (in char. zero). The "Grundobjekt" of anabelian alg. geometry in char. zero, for which the prime field is $\mathbb{Q}$, is therefore the group

$$\Gamma = \pi_1(\mathbb{Q}) = \mathrm{Gal}(\overline{\mathbb{Q}}/\mathbb{Q}), \tag{5}$$

where $\overline{\mathbb{Q}}$ stands for the algebraic closure of $\mathbb{Q}$ in $\mathbb{C}$.

The above conjecture may be regarded as the main conjecture of "birational" anabelian alg. geometry – it asserts that the category of "absolute birational alg. varieties" in char. zero can be embedded into the category of Γ-augmented profinite groups. There remains the further task of obtaining a ("purely geometric") description of the group Γ, and also of understanding which Γ-augmented profinite groups are isomorphic to some $\pi_1(K)$. I will not go into these questions for now, but will rather formulate a related and considerably sharper conjecture for anabelian curves, from which the above follows. Indeed I see two apparently different but equivalent formulations:

1) Let X, Y be two (connected, assume once and for all) anabelian curves
5/6 over the absolute field of char. zero, and consider the map

$$\mathrm{Hom}_K(X, Y) \to \mathrm{Hom\ ext}_{\pi_1(K)}(\pi_1(X), \pi_1(Y)), \tag{6}$$

where Hom ext denotes the set of outer homomorphisms of the profinite groups, and the index $\pi_1(K)$ means the compatibility with augmentation into $\pi_1(K)$. From the above, one knows that this map is injective. I conjecture that it is bijective [see the correction in the P.S.]

2) This second form can be seen as a reformulation of 1) in the case of a constant map from X into Y. Let $\Gamma(X/K)$ be the set of all K-valued points (that is "sections") of X over K; one considers the map

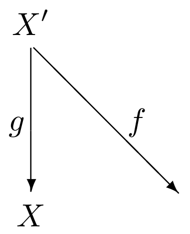
$$\Gamma(X/K) \to \mathrm{Hom\ ext}_{\pi_1(K)}(\pi_1(K), \pi_1(X)), \tag{7}$$

where the second set is thus the set of all the "splittings" of the group extension (3) (where A is replaced by X – $\pi_1(X) \to \pi_1(K)$ is actually surjective, at least if X has a K-valued point, so that X is also "geometrically connected"), or better the set of conjugacy classes of such splittings under the action of the group $\pi_1(\overline{X})$. It is known that (7) is injective, and the main conjecture asserts that it is bijective [see the correction below].

Formulation 1) follows from 2), with K replaced by the function field of X. Moreover, it is indifferent whether X is anabelian or not and, if I am not mistaken, assertion 1) follows even for arbitrary non-singular X (without the assumption $\dim X = 1$). Concerning Y, it follows from the conjecture that assertion 1) remains true, as far as Y is "elementary anabelian" [see the correction in the PS], and correspondingly of course for assertion 2). This in principle now gives the possibility, by applying Artin's remark, to obtain a complete description of the category of schemes of finite type over K "en termes de" $\Gamma(K)$ and systems of profinite groups. Here again I have typed something a little too quickly, as indeed the main conjecture should first be justified and completed with an assertion about which (up to isomorphism) complete $\Gamma(K)$-augmented profinite groups arise from anabelian curves over K. Concerning only an assertion of "pleine fidélité" as in formulations 1) and 2) above, it should be possible to deduce the following, without too much difficulty, from these assertions, or even already (if I am not mistaken) from the above considerably weaker birational variant. Namely, let X and Y be two schemes which are "essentially of finite type over $\mathbb{Q}$", e.g. each one is of finite type over an absolute field of char. zero. (remaining undetermined). 6/7 X and Y need to be neither non-singular nor connected, let alone "normal" or the like – but they must be assumed to be reduced. I consider the etale topoi X_{et} and Y_{et}, and the map

$$\text{(8)} \qquad \mathrm{Hom}(X, Y) \to \mathrm{Iskl}\ \mathrm{Hom}_{\mathrm{top}}(X_{\mathrm{et}}, Y_{\mathrm{et}}),$$

where $\mathrm{Hom}_{\mathrm{top}}$ denotes the (set of) homomorphisms of the topos X_{et} in Y_{et}, and Iskl means that one passes to the (set of) isomorphism classes. (It should be noted moreover that the category $\underline{\mathrm{Hom}}_{\mathrm{top}}(X_{\mathrm{et}}, Y_{\mathrm{et}})$ is <u>rigid</u>, namely that there can be only one isomorphism between two homomorphisms $X_{\mathrm{et}} \to Y_{\mathrm{et}}$. When X and Y are multiplicities and not schemes, the assertion below should be replaced with a correspondingly finer one, namely one should state an <u>equivalence of categories</u> of $\underline{\mathrm{Hom}}(X, Y)$ with $\underline{\mathrm{Hom}}_{\mathrm{top}}(X_{\mathrm{et}}, Y_{\mathrm{et}})$.) It is essential here that X_{et} and Y_{et} are considered simply as topological spaces, that is without their structure sheaves, whereas the left-hand side of (8) can be interpreted as Iskl $\mathrm{Hom}_{\mathrm{top.ann.}}(X_{\mathrm{et}}, Y_{\mathrm{et}})$. Let us first notice that from the already "known" facts, it should follow without difficulty that (8) is injective. In fact I now realize that in the description of the right-hand side of (8), I forgot an important element of the structure, namely that X_{et} and Y_{et} must be considered as topoi <u>over the absolute base $\mathbb{Q}_{\mathrm{et}}$</u>, which is completely described by the profinite group $\Gamma = \pi_1(K)$ (5). So $\mathrm{Hom}_{\mathrm{top}}$ should be read $\mathrm{Hom}_{\mathrm{top}/\mathbb{Q}_{\mathrm{et}}}$. With this correction, we can now state the tantalizing conjecture that (8) should be bijective. This may not be [altogether] correct for the reason that there can exist radicial morphisms $Y \to X$ (so-called "universal homomorphisms"), which produce a topological equivalence $Y_{\text{et}} \xrightarrow{\sim} X_{\text{et}}$, without being an isomorphisms, so that there does not exist an inverse map $X \to Y$, whereas it does exist for the etale topoi. If one now assumes that X is normal, then I conjecture that (8) is bijective. In the general case, it should be true that for any ϕ on the right-hand side, one can build a diagram



(where g is a "universal homomorphism"), from which ϕ arises in the obvious way. I even conjecture that the same assertion is still valid without the char. zero assertion, that is when $\mathbb{Q}$ is replaced by $\mathbb{Z}$ – which is connected with the fact that the "birational" main conjecture must be valid in arbitrary
7/8 characteristic, as long as we replace the "absolute" fields with their "perfect closures" $K^{p^{-\infty}}$, which indeed have the same π_1.

I am afraid I have been led rather far afield by this digression about arbitrary schemes of finite type and their etale topoi – you may be more interested by a third formulation of the main conjecture, which sharpens it a little and has a peculiarly "geometric" ring. It is also the formulation I told Deligne about some two years ago, and of which he told me that it would imply Mordell's conjecture. Again let X be an anabelian geometrically connected curve over the absolute field K of char. zero, $\widetilde{X}$ its universal cover, considered as a scheme (but not of finite type) over $\overline{K}$, namely as the universal cover of the "geometric" curve $\overline{X}$. It stands here as a kind of algebraic analogue for the transcendental construction, in which the universal cover is isomorphic to the Poincaré upper half-plane. I also consider the completion $X^\wedge$ of X (which is thus a projective curve, not necessarily anabelian, as X can be of genus 0 or 1), together with its normalisation $\widetilde{X}^\wedge$ with respect to $\widetilde{X}$, which represents a kind of compactification of $\widetilde{X}$. (If you prefer, you can assume from the start that X is proper, so that $X = X^\wedge$ and $\widetilde{X} = \widetilde{X}^\wedge$.) The group $\pi_1(X)$ can be regarded as the group of the X-automorphisms of $\widetilde{X}$, and it acts also on the "compactification" $\widetilde{X}^\wedge$. This action commutes with the action on $\overline{K}$ via $\pi_1(K)$. I am now interested in the corresponding action

$$\text{Action of } \pi_1(X) \text{ on } \widetilde{X}^\wedge(\overline{K}),$$

(the [set of] $\overline{K}$-valued points, or what comes to the same thing, the points of $X^\wedge$ distinct from the generic point), and in particular, for a given section of (3)

$$\pi_1(K) \to \pi_1(X),$$

I consider the corresponding action of the Galois group $\pi_1(K)$. The conjecture is now that the latter action has (exactly) one fixed point.

That it can have at most one fixed point follows from the injectivity in (7), or in any case can be proved along the same lines, using the Mordell-Weil theorem. What remains unproved is the existence of the fixed point, which is more or less equivalent to the surjectivity of (7). It now occurs to me that the formulation of the main conjecture via (7), which I gave a while ago, is correct only in the case where X is proper – and in that case, it is in effect equivalent to the third (just given) formulation. In the contrary case where
8/9 X is not proper, so has "points infinitely far away", each of these points clearly furnishes a considerable packet of classes of sections (which has the power of the continuum), which cannot be obtained via points lying at a finite distance. These correspond to the case of a fixed point in $\widetilde{X}^\wedge$ which does not lie in $\widetilde{X}$. The uniqueness of the fixed point means among other things, besides the injectivity of (7), that the "packets" which correspond to different points at infinity have empty intersection; and thus any class of sections which does not come from a finite point can be assigned to a uniquely defined point at infinity.

The third formulation of the main conjecture was stimulated by certain transcendental reflections on the action of finite groups on complex algebraic curves and their (transcendentally defined) universal covers, which have played a decisive role in my reflections (during the first half of 1981, that is some two years ago) on the action of Γ on certain profinite anabelian fundamental groups (in particular that of $\mathbb{P}^1 - (0, 1, \infty)$). (This role was mainly that of a guiding thread into a previously completely unknown region, as the corresponding assertions in char $p > 0$ remained unproved, and still do today.) To come back to the action of the Galois groups as $\pi_1(K)$, these appear in several respects as analogous to the action of finite groups, something which for instance is expressed in the above conjecture in a particularly striking and precise way.

I took up the anabelian reflections again between December 81 and April 82, that time with a different emphasis – namely in an effort toward understanding the many-faceted structure of the [Teichmüller] fundamental groups $T_{g,\nu}$ (or better, the fundamental groupoids) of the multiplicities of moduli $\overline{M}_{g,\nu}$, and the action of Γ on their profinite completions. (I would like to return to this investigation next fall, if I manage to extricate myself this summer from the writing up of quite unrelated reflections on the foundations of cohomological resp. homotopical algebra, which has occupied me for four months already.) I appeal to your indulgence for the somewhat

chaotic presentation of a circle of ideas which intensively held my attention for six months, but with which I have had for the past two years only very fleeting contacts, if any. If these ideas were to interest you, and if you happened at some point to be in the south of France, it would be a pleasure for me to meet with you and to go into more details of these or other aspects of the "anabelian Yoga". It would also surely be possible to invite you to Montpellier University for some period of time at your convenience; only I am afraid that under the present circumstances, the procedure might be a
$\frac{9}{10}$ little long, as the university itself does not at present have funds for such invitations, so that the invitation would have to be decided resp. approved in Paris – which may well mean that the corresponding proposal would have to be made roughly one year in advance.

On this cheering note, I will put an end to this letter, which has somehow grown out of all proportion, and just wish you very pleasant holidays!
Best regards

Your Alexander Grothendieck

PS Upon rereading this letter, I realize that, like the second formulation of the main conjecture, also the generalisation to "elementary anabelian" varieties must be corrected, sorry! Besides I now see that the first formulation must be corrected in the same way – namely in the case where Y is not proper, it is necessary to restrict oneself, on the left-hand side of (6), to non-constant homomorphisms, and on the right-hand side to homomorphisms $\pi_1(X) \to \pi_1(Y)$, whose images are of finite index (i.e. open). In the case where Y is replaced by an elementary anabelian variety, the bijectivity of (6) is valid, as long as one restricts oneself to dominant homomorphisms on the left-hand side, keeping the same restriction (finite index image) on the right-hand side. The "birational" formulation should be corrected analogously – namely one must restrict oneself to homomorphisms (4) with finite index image.

Returning now to the map (7) in the case of an anabelian curve, one can specify explicitly which classes of sections on the right-hand side do not correspond to a "finite" point, thus do not come from an element on the left-hand side; and if I remember correctly, such a simple characterization of the image of (7) can be extended to the more general situation of an "elementary anabelian" X. As far as I now remember, this characterization (which is of course just as conjectural, and indeed in both directions, "necessary" and "sufficient") goes as follows. Let

$$\pi_1(K)^o = \text{Kernel of } \pi_1(K) \to \widehat{\mathbb{Z}}^* \text{ (the cyclotomic character).}$$

Given a section $\pi_1(K) \to \pi_1(X)$, $\pi_1(K)$ and therefore also $\pi_1(K)^o$ operates on $\pi_1(\overline{X})$, the geometric fundamental group. The condition is now that the subgroup fixed under this action be reduced to 1!