CHAPTER 1

Algebra at the Beginning of the Twentieth Century

Since the theory of semigroups is clearly a branch of abstract algebra, it is appropriate to set the scene for later chapters by beginning this book with a very short account of the development of abstract algebra up to the start of the twentieth century. In particular, I discuss algebra's transition, over the course of the nineteenth century, from a subject concerned entirely with the solution of polynomial equations to a discipline that deals with general 'structures' within mathematics, where 'structure' may be understood loosely to mean a set together with one or more operations which are subject to certain conditions. This transition is the subject of Section 1.1. My account of algebra's progression is not of course a comprehensive one — I refer the interested reader instead to Corry (1996).

The consolidation of group theory at the beginning of the twentieth century, together with some postulational considerations, led, in 1904, to the coining of the French term 'semigroupe', which was taken over into English naturally, and almost immediately, as 'semigroup'. The original notion of a 'semigroup', however, did not correspond to the modern definition. Moreover, the term was coined in 1904 merely to provide some terminology for a problem in group theory: the study of semigroups for their own sake did not begin until around 1918, as we will discover in Chapter 3. The circumstances surrounding the introduction of the term 'semigroup' are discussed in Section 1.2. Finally, Section 1.3 features an overview of the development of semigroup theory, which will serve as a short guide, more detailed than that given in the preface, to the material of later chapters.

1.1. A changing discipline

Like many of the words used in mathematics, the term 'algebra' has been employed in different ways down through the centuries. As almost any book on the general history of mathematics will reveal (see, for example, Katz 2009, §9.3), the word 'algebra' is a Latin corruption of the Arabic term 'al-jabr' which appears in the title of al-Khwārizmī's ninth-century text *Al-kitāb almukhtasar fi hisāb al-jabr w'al-muqābala*, which may be rendered into English as *The book on restoration and balancing*. The term 'balancing' ('al-muqābala') refers to the need, when solving equations, to add the same quantity to both sides, while 'restoration' ('al-jabr') indicates the addition of a positive term in order to cancel out a negative (Stedall, 2008, §1.4.2). In the centuries after its adoption into European languages, the term 'algebra' came to be used to refer to the process of solving polynomial equations.[1] This is of course akin to the way in which the term 'algebra' is used in schools to this day, although it was a long time before such 'algebra' came to be written symbolically (see, for example, Katz 2009, §12.4).

A major change in the use of the word 'algebra' was set in motion, however, in the early nineteenth century. By this stage, the centuries-old general solution to a quadratic equation had been complemented by further general solutions for cubic and quartic equations (by Ferreo, Tartaglia, and Cardano in the sixteenth century — see Katz 2009, §12.3 or Stedall 2011), and abortive efforts were under way to find a general solution to the quintic. The inability of mathematicians to find such a solution was explained in the 1820s, when Niels Henrik Abel (1802–1829) published a proof of the insolubility of the general quintic in radicals (Katz, 2009, §21.2). Abel next set out to find conditions for the solubility of a given equation but died before he could make much headway. As is well known, this problem was picked up by Évariste Galois (1811–1832), who, in his short life (shorter even than Abel's), established the beginnings of a general theory concerning polynomial equations and their solutions in radicals. He determined that a useful tool in the study of the interrelations between the roots of a given polynomial was a particular collection of permutations, which he termed a 'groupe'. In Galois's work, we see the foundations of what we now term Galois theory in his honour, and of the later theory of groups. This is not the place to recount the full history of either of these disciplines,[2] but we note that Galois's work marks an epoch in the development of modern algebra. As Peter M. Neumann (2011, p. 1) puts it,

> [Galois's] mathematical intuition, once it was understood, changed the theory of equations from its classical form into what is now universally known as Galois Theory, together with its associated 'abstract algebra', including the theory of groups and fields.

As the nineteenth century progressed, it came to be recognised that Galois's 'group' was just one particular instance of a more general structure that also appeared in the work of other mathematicians in different contexts (for example, in Gauss's work on quadratic forms — see Section 6.2). By the end of the century, these disparate notions had been united under the heading of 'group theory' — see Wussing (1969). I make some very brief comments on the early abstract definitions of a group in Section 1.2.

Another concept that is implicit in Galois's work is that of a *field*. As with the notion of a group, however, Galois did not deal with fields in the abstract, but merely, for the most part,[3] with the rational numbers $\mathbb{Q}$ and extensions thereof. The broader, explicit study of fields (though still not in an abstract setting) was due first to Leopold Kronecker (1823–1891) and then to Richard Dedekind (1831–1916). The abstract definition of a field came later, in an 1893 paper by Heinrich Weber (1842–1913). Like that of a group, the notion of a field came to be recognised in many different contexts. Thus, by the end of the nineteenth century, mathematicians were beginning to realise that not just groups, but other general structures, were common to superficially unrelated areas of mathematics (Katz, 2009, §21.5). Because of its use in connection with groups and their role in the solution of polynomial equations, the term 'algebra' gradually came to apply to this new 'structural' approach to mathematics more generally.

During the final years of the nineteenth century and the early years of the twentieth, other abstract structures began to emerge, and the first tentative steps were taken towards the study of these objects in an entirely abstract setting. Thus, for example, we have the 1910 paper 'Algebraische Theorie der Körper' of Ernst Steinitz (1871–1928), in which the abstract theories of fields and integral domains were developed almost from scratch (see Corry 1996, 2nd ed., §4.2). I will have more to say about Steinitz's paper in Chapter 5.

As is well documented (see, for example, Corry 1996, 2nd ed., Chapter 5), one of the major proponents of the new 'abstract algebra' was Emmy Noether (1882–1935). In papers of the 1920s, for example, she explored the still-young notion of an abstract ring. Some of her results on factorisation in commutative rings will be stated in Chapter 4. Her work embraced not only the study of abstract structures, but also that of the connections between them — she made extensive use of homomorphisms, for example. Noether's approach to algebra was taken up by the research group that formed around her and was also broadcast to a wider audience by B. L. van der Waerden's 1930 *Moderne Algebra*. This extremely influential text, based upon the lectures of Noether in Göttingen and Emil Artin (1898–1962) in Hamburg, was largely responsible for the spread of the new view of algebra around the world (see Corry 1996, 2nd ed., §1.3 or Schlote 2005). It will be cited many times throughout the present book.

The transition to the new 'structural' algebra had largely been completed by the time that a true theory of semigroups began to emerge. Nevertheless, it is a process that we should be aware of, particularly in connection with the earlier semigroup-related studies that we will see in Chapters 4 and 5, for example. One aspect of modern algebra that I have not mentioned, but which is particularly pertinent to upcoming material, including Section 1.2, is the postulational/axiomatic approach. The use of axiomatic definitions is now so widespread in abstract algebra, particularly in its teaching, that the discipline is often mistakenly thought of simply as the study of axiomatically defined objects. In fact, such definitions are just a way of expressing the ideas that are integral to abstract algebra. Nevertheless, the postulational method has grown up alongside such algebra, and we will see its influence on the development of semigroup theory in later chapters. Indeed, a detailed discussion of this trend is given in Section 4.1.

1.2. The term 'semigroup'

As we saw in Section 1.1, one of the mainstays of the nineteenth century's burgeoning abstract algebra was the theory of groups. The first attempt at a definition of an *abstract* group was that given by Arthur Cayley (1821–1895) in a paper of 1854, but the notion does not seem to have received serious interest until the 1880s (see Katz 2009, §21.5). Further suggestions followed as to what the abstract definition of a group should be, and it is against this backdrop that the term 'semigroup' was coined. I deal here only with those aspects of this development that lead us to the term 'semigroup' — for a fuller account of the maturation of group axiomatics, see Neumann (1999).

There is much to criticise in Cayley's phrasing of his initial definition of an abstract group (see Neumann 1999, pp. 289–290 and Stedall 2008, §13.1.4), but what emerges is a finite system with a closed, associative multiplication and with an identity element such that "if the entire group is multiplied by any one of the symbols, ... the effect is simply to reproduce the group" (Cayley, 1854, p. 124). Thus, Cayley's 'group' was a monoid G for which $gG = G = Gg$, for any $g \in G$. We see that this is indeed the modern notion of a (finite) group.

As noted above, Cayley's suggested notion of abstract group was largely disregarded by other mathematicians at the time, who saw no great benefit in a move to an abstract setting. However, by the 1880s, the abstract notion of a group was beginning to take hold and was even being actively promoted by other mathematicians. Among these was Heinrich Weber, who, in a paper of 1882, defined a (finite) group to be a system G with a closed, associative multiplication, for which the following condition holds:

$$\text{for } a, b, c \in G, \text{ if } ac = bc \text{ or } ca = cb, \text{ then } a = b. \tag{1.1}$$

Thus, Weber defined a group to be what we would now term a *cancellative* semigroup (condition (1.1) is known as the *cancellation law*), although the fact that a finite cancellative semigroup is necessarily a group means that Weber's objects were in fact groups. Indeed, Weber's work displays two of the major features of early group definitions: the restriction to the finite case and the lack of any explicit mention of inverse elements. The incorporation of infinite groups into group theory, which had hitherto been a theory of strictly finite objects, was a major theme of abstract algebra in the 1890s.

One of the first texts to attempt to treat both finite and infinite groups was also one of the first to deal with groups purely in the abstract: J.-A. de Séguier's *Élements de la théorie des groupes abstraits*, published in Paris in 1904 (see Wussing 1969, English trans., p. 252). With de Séguier's book it became clear that certain results which hold for finite groups do not, in general, hold for infinite groups. In particular, certain general systems which form groups when finite do not form groups when infinite. The desire to give these 'non-groups' a name led de Séguier to the definition of a new concept: that of a *semigroup*, or *semigroupe*, as it was in the original French (de Séguier, 1904, p. 8). This was not, however, our modern notion of 'semigroup'.

'Semigroups' were introduced to the English-reading mathematical world later that same year in a review of de Séguier's book by L. E. Dickson (1874–1954), who followed this a few months later with the first original paper to feature the word 'semigroup' in its title: 'On semi-groups and the general isomorphism between infinite groups' (Dickson, 1905b). Nevertheless, this cannot be said to be a paper on semigroup theory. In this brief article, Dickson explored some of the motivation for the definition of a 'semigroup'; an account of the results of this paper may also be found in Schmidt (1916). We explore de Séguier's definitions through their treatment at Dickson's hands.

From Dickson's review, we have (the English translation of) de Séguier's original definition:

Definition 1.1. A set G, which has generating set $S \subseteq G$ with respect to a given binary operation, forms a *semigroup* if the following postulates hold:

(1) $(ab)c = a(bc)$, for all $a, b, c \in G$;
(2) for any $a \in S$ and any $b \in G$, there is at most one solution, $x \in G$, of $ax = b$;
(3) similarly for $xa = b$.

This is clearly not the modern notion of a semigroup, for which only postulate (1) is required: de Séguier had used the term 'corps' for such a system. This word is now used in French to mean 'field'.

Following his introduction of this notion of 'semigroup', de Séguier went on to state (without proof) that it follows that $ax = ax'$ implies $x = x'$, for any $a \in G$. In his review, Dickson suggested that de Séguier's argument must have run as follows. Express a in terms of the generators: $a = a_1 a_2 a_3$, say, for $a_1, a_2, a_3 \in S$, so that we have $a_1(a_2a_3x) = a_1(a_2a_3x')$. We apply postulate (2) to arrive at $a_2(a_3x) = a_2(a_3x')$, and after a further two applications of the same postulate we obtain $x = x'$, as required. However, to Dickson's mind, there was a small problem with this argument: at the second stage, we have made the implicit assumption that a_2a_3x belongs to G. Unfortunately, there is no guarantee of this, since, as Dickson pointed out, the postulates given above do not ensure closure of G. This was a rather curious criticism for Dickson to make: unlike de Séguier, who evidently included closure as part of the definition of his binary operation, Dickson must have been regarding such an operation simply as a function on $G \times G$, but with no guarantee that the image of the function was contained in G. However, recognising de Séguier's assumption of closure, Dickson decided to make it explicit, commenting that "[m]ost readers, I think, would find it more natural to have this property as a postulate" (Dickson, 1904, p. 160). Dickson thus modified the definition of a 'semi-group' accordingly, also removing all references to the generating set S and, indeed, inserting the hyphen. With the modified definition, it is certainly the case that $ax = ax'$ gives $x = x'$ since the above reasoning now holds. In fact, in his subsequent paper, Dickson modified his definition of 'semi-group' once more, to include this and its dual as postulates:

Definition 1.2. A set G forms a *semi-group* under a given binary operation if the following postulates hold:

(0′) if $a, b \in G$, then $ab \in G$;
(1′) $(ab)c = a(bc)$, for all $a, b, c \in G$;
(2′) for any $a, x, x' \in G$, if $ax = ax'$, then $x = x'$;
(3′) for any $a, x, x' \in G$, if $xa = x'a$, then $x = x'$.

It is now clear that what Dickson defined as his 'semi-group' is what we would call a cancellative semigroup; he suggested the name 'algebra' for a system satisfying just postulates (0′) and (1′). As we have already noted in connection with Weber's definition of a group, every finite cancellative semigroup is necessarily a group. Indeed, Definition 1.2 is precisely that given by Weber for a finite group. De Séguier's and Dickson's 'semi-group' was thus only of special interest when it was infinite and did not form a group.

Dickson's main concern was the question: when is a 'semi-group' a group? The most obvious example of a cancellative semigroup that is not a group is the semigroup of positive integers under addition. However, Dickson did not give this as an example. He instead provided a general construction for a particular type of 'semi-group' which forms a group when finite but not when infinite. Before we examine this in detail, we first need some preliminary definitions from Dickson's paper.

Definition 1.3. Two groups G and H are said to be *generally isomorphic* if there exists a binary relation $R \subseteq G \times H$ between their elements such that:

(1) for each $g \in G$, there is at least one $h \in H$ with $g\,R\,h$;
(2) for each $h \in H$, there is at least one $g \in G$ with $g\,R\,h$;
(3) if $g\,R\,h$ and $g'\,R\,h'$, then $gg'\,R\,hh'$.

The relation R is called a *general isomorphism.*

Another term that was used for 'general isomorphism' in the early decades of the twentieth century was 'homomorphism' (see Section 7.2), although this was of course a more general use of the word than that in current circulation.

We now define two subsets of generally isomorphic groups G and H:

$$G' = \{g \in G : g\,R\,1\}, \qquad H' = \{h \in H : 1\,R\,h\}.$$

It is easy to show that if G and H are both finite, then G' and H' are subgroups. De Séguier had attempted, without success, to show that this is also the case for infinite G and H. In fact, this is not so, as demonstrated by the following counterexample constructed by Dickson. We take G and H to be infinite cyclic groups on different generators: $G = \langle a \rangle$ and $H = \langle b \rangle$. The relation R is given by the rule

$$a^i\,R\,b^j \iff i + j \geq 0;$$

R is easily shown to be a general isomorphism. Setting $b^0 = 1$, we see also that $a^i\,R\,1$ whenever $i \geq 0$. Similarly, for $a^0 = 1$, $1\,R\,b^j$ whenever $j \geq 0$. Thus

$$G' = \{1, a, a^2, \ldots\} \qquad \text{and} \qquad H' = \{1, b, b^2, \ldots\},$$

neither of which is a group, although each is of course a 'semi-group' in de Séguier's sense. It was a simple desire to give a name to such sets that provided the initial impetus for the definition of a 'semi-group'.

De Séguier's concept of 'semigroup' remained in common use until around 1940, after which the modern definition was adopted in the wake of David Rees's paper of that year (see Chapter 6). In fact, I have found the word 'semigroup' being used in four slightly different senses between 1904 and 1940:[4]

- a set with an associative binary operation (Hilton, 1908);
- a set with an associative, cancellative binary operation (Schmidt, 1916);
- a set with an associative, left cancellative binary operation (Bell, 1930);
- a set with an associative, commutative, cancellative binary operation, with respect to which there exists an identity (Clifford, 1938).

As far as I can determine, the earliest source to give the modern definition of a semigroup is in fact very close, chronologically speaking, to the initial definition of 1904. This source is Harold Hilton's 1908 book *An introduction to the theory of groups of finite order*, in which we find:

> A set of elements is said to form a *group*, if (1) the product of any two (or the square of any one) of the elements is an element of the set; (2) the set contains the inverse of each element of the set. If the set satisfies condition (1) but not (2), it is called a *semi-group*. (Hilton, 1908, p. 51)

On the first page of the book, Hilton had defined 'elements' to be 'things' whose composition is always associative. The existence of an identity element was similarly assumed implicitly. We know from his preface that Hilton consulted de Séguier's book but there is no explicit indication of why he changed the definition of a semigroup. One possible solution is found in the preface:

> The nomenclature of the subject is by no means settled. I have tried to select definitions which have the advantage of being self-explanatory ... (Hilton, 1908, p. v).

The way in which the definitions of a group and a semigroup are given in the above quotation does seem to emphasise the fact that a semigroup requires half as many postulates as a group.

As we have noted, the hyphen in 'semi-group' originated with Dickson (1904), and continued to be used in most of the early sources on semigroups, perhaps to emphasise the connection with groups; after all, many of these sources concerned group theory, not semigroup theory. The American semigroup pioneer A. H. Clifford did not use the hyphen (see, for example, Clifford 1938, 1941), but it was employed by Rees (1940) and persisted as far as the semigroup-theoretic papers of another British author, Gordon Preston (in, for example, Preston 1954c). Indeed, among those writing in English, its seems to have been British authors who, for a time, retained the hyphen, whereas it was dropped by Americans. For example, in a letter to Preston, dated 30 January 1955, his fellow semigroup theorist Douglas Munn, a Scot, apologised:

> I'm afraid I have been consistently spelling this in the American fashion — without the hyphen!

As semigroup theory took on a life of its own, however, the hyphen was gradually dropped.

Alternative names for semigroups abound:

> Many authors, including most of those writing in French, use the term 'demigroup[e]' for [a set with an associative binary operation]; these authors reserve 'semigroup' for what we shall call a cancellation semigroup. Other terms are 'monoid' (Bourbaki) and 'associative system' (Russian authors). (Bruck, 1958, p. 23–24)

The term 'monoid', which seems to have originated with Bourbaki (1943), is now applied only to a semigroup with an identity element.[5]

Casting the net a little wider and considering other languages, we find that the term *Semigruppe* was used in German to correspond to de Séguier's 'semigroup' (Suschkewitsch, 1935). However, this usage does not seem to have been very widespread; indeed, A. K. Sushkevich is one of only two authors I can find who used this term, the other being Fritz Klein-Barmen.[6] I mention it here only in the interest of completeness. I. V. Arnold (1929) used the term *Halbgruppe* (with reference to Schmidt 1916) to mean a set with an associative, commutative, cancellative binary operation; nowadays, the German *Halbgruppe* corresponds to the modern English sense of *semigroup*. Rather than the term *associative system* (*ассоциативная система*), Russian authors now use *polugruppa* (*полугруппа*) for a set with an associative binary operation: the Russian prefix *polu-* corresponds to the Latinate *semi-*. Indeed, most modern terms for *semigroup* follow the 'half a group' pattern: for example, the Portuguese *semigrupo*, the Hungarian *félcsoport*, and the Japanese *hangun*. Other terms for semigroups, in a range of languages, will be indicated in later chapters.

1.3. An overview of the development of semigroup theory

When we look at the group theory of the late nineteenth and early twentieth centuries, we sometimes see the notion of a semigroup appearing implicitly, but this was not because the authors in question were studying semigroups as such[7] — it was merely that the context within which these mathematicians were working enabled them to postulate, for instance, only closure in order to ensure that their object of interest was indeed a group. This condition suffices, for example, in the case of a finite group of permutations: associativity and the existence of an identity and inverses follow automatically.

During the early decades of the twentieth century, a number of generalisations of the group concept began to emerge. These arose not as generalisations for generalisation's sake, but rather to fulfil certain mathematical needs that the traditional notion of a group could not meet. Thus, for example, Oswald Veblen and J. H. C. Whitehead introduced their notion of a 'pseudogroup' as a tool for describing structures in differential geometry (Section 10.2), while Heinrich Brandt devised his so-called 'Gruppoid' as the abstract structure formed by certain systems of quaternary quadratic forms (Section 6.2). Other similar concepts also arose around this time, and it was against this backdrop that the notion of a semigroup began to find its way into the general mathematical consciousness.

Like those of a 'pseudogroup' and a 'Gruppoid', the idea of a semigroup did not emerge simply as an arbitrary generalisation of a group: it appeared in response to some very natural observations concerning the transformations of a set. The theory of groups is, in essence, the abstract theory of permutations, and, since non-invertible transformations are no less ubiquitous in mathematics than permutations, some mathematicians began to feel that a broader study of such mappings was called for, together with the development of the corresponding abstract theory. A number of researchers gave their attention to this issue, but the person who carried out the most comprehensive early study was a Russian-born mathematician, A. K. Sushkevich (1889–1961).

Sushkevich, whose life and work are the subject of Chapter 3, spent most of his working life at Kharkov State University in Ukraine. It was here that he carried out the majority of his semigroup-theoretic investigations, beginning in around 1918. During the 1920s, 1930s, and, to a lesser extent, the 1940s, he published a large number of papers, and also a monograph, concerning the theory of what he termed 'generalised groups'. He proved, for example, (the finite version of) a theorem that is fundamental to semigroup theory: the generalised Cayley Theorem, which states that any semigroup may be embedded in a semigroup of transformations of some set. However, apart perhaps from one or two papers, Sushkevich's work passed into a certain obscurity, for reasons that I endeavour to explain in Section 3.4, and does not seem to have been particularly well known even to later Soviet authors.

In the 1930s, different types of semigroup-theoretic problems began to emerge, seemingly influenced by the rise of abstract algebra. The nineteenth-century concepts of groups, integral domains, and fields, for example, had been joined by other now-familiar notions during the early decades of the twentieth century: rings, for instance, in the 1920s and lattices and universal algebras in the 1930s. The abstract notion of a semigroup also began to receive more attention around this time. In contrast to the studies by Sushkevich, in which the transformations of a set were never very far away, the new investigations involving semigroups were unashamedly abstract, with little, if any, mention made of semigroups of transformations.

Another feature of the growing study of semigroups in the 1930s was a heavy ring-theoretic influence. Although ring- and associative algebra-related considerations had occasionally affected Sushkevich, much of his work had been developed by analogy with issues pertaining to groups. In the 1930s, however, researchers began to consider the problem of unique factorisation in semigroups and in their systems of ideals (following Noether's study of the same for commutative rings) and that of embedding cancellative semigroups in groups (by analogy with the embedding of integral domains in fields). These problems are the subjects of Chapters 4 and 5, respectively. It is in connection with the former that the name of A. H. Clifford (1908–1992) first enters our story: Clifford went on to develop a great deal of influential early semigroup theory and will therefore enjoy a prominent position within this book. Indeed, it is difficult to overstate Clifford's importance for the development of semigroup theory.

The birth of a true *theory* of semigroups, independent of those of groups and rings, came in around 1940. This was the year that David Rees (1918–2013) published his seminal paper 'On semi-groups', in which he developed a sensible notion of 'simple' semigroup and went on to derive a semigroup analogue of the Artin–Wedderburn Theorem for semisimple rings and algebras. His result is now known, appropriately enough, as the Rees Theorem, or, occasionally, as the Rees–Sushkevich Theorem, since it subsumes an earlier result of Sushkevich in the finite case.

The Rees Theorem was semigroup theory's first major structure theorem, and it was followed very quickly, in 1941, by the second: a result of Clifford, characterising certain semigroups that are unions of groups. This latter theorem marked the beginning of a truly independent theory of semigroups, for, unlike the Rees Theorem, it has no analogue in either group or ring theory. Together, these results of Rees and Clifford provided models for future semigroup structure theorems, while the wider material of their papers suggested avenues for further research that were soon seized upon by other mathematicians (see Chapter 6).

The study of semigroups expanded rapidly around the world during the 1940s, with many papers appearing on the subject, their authors influenced to varying extents by the work of Rees and Clifford. Thus, for example, the 1940s saw the beginning of Paul Dubreil's Paris-based school of 'demi-groupes', with its focus on congruences on semigroups (Chapter 7). They also saw the rebirth of the Soviet school of semigroup theory (Chapter 9). Sushkevich had, by this time, abandoned semigroup-theoretic research, but the subject was picked up by E. S. Lyapin (1914–2005) in Leningrad. However, Lyapin's work owed little to that of Sushkevich: his approach to the theory of semigroups was fresh and considerably more abstract and was heavily influenced, at least initially, by his background in group theory. Lyapin's Leningrad-based semigroup school was the first of several strong pockets of semigroup theory within the USSR, other early examples being those centred around L. M. Gluskin (1922–1985) in Kharkov and V. V. Wagner (1908–1981) in Saratov. The Soviet study of semigroup theory, and that conducted in Central and Eastern Europe more generally, grew in parallel with that of Western Europe and North America. Different approaches were developed to similar problems, while communications difficulties led to the duplication of a great deal of work. The comparison of the work of mathematicians on opposite sides of the Iron Curtain is a central theme within this book: the general situation, with regard to East-West communication, is dealt with in Chapter 2, but specific instances are considered in several later chapters.

Semigroup theory continued to grow during the 1950s and became an even more international endeavour: besides the American, French, Soviet, and Slovak schools that had emerged in the 1940s, there now appeared Hungarian and Japanese schools, for instance (see Chapter 8). During this decade, certain major subdivisions of the modern theory put in their first appearances. Thus, for example, the notion of an inverse semigroup was introduced independently by Wagner in the

USSR in 1952 and by G. B. Preston (born 1925) in the UK in 1954; the development of this subsequently much-studied concept is the subject of Chapter 10. Matrix representations of semigroups, which had already seen some study by both Sushkevich and Clifford, were treated in greater depth by W. D. Munn (1929–2008) and J. S. Ponizovskii (1928–2012): see Chapter 11.

By the mid-1950s, the published semigroup literature had expanded to the extent that it was becoming difficult for researchers to be familiar with it all.[8] Moreover, some confusion over differing definitions and notation began to emerge. It therefore came to be felt that some standardisation of the theory was required. The most natural way for this to be effected was through the publication of a monograph that collected together the important aspects of the theory thus far. The first such monograph was Lyapin's *Semigroups* (*Полугруппы*) of 1960, with a second, *The algebraic theory of semigroups* by Clifford and Preston, appearing the following year. Both of these books presented a unified view of semigroup theory and did indeed help to standardise the notation and terminology that is still in use today.

From its earliest days, semigroup theory was visible at conferences: Sushkevich spoke about semigroups of transformations at the 1928 International Congress of Mathematicians (ICM) in Bologna, for example (see Section 3.3.1). Indeed, the theory was communicated both at large general conferences and also at smaller algebraic meetings. The first international conference devoted exclusively to semigroups took place in Czechoslovakia in 1968, by which time the theory was very firmly established on the mathematical landscape. One of the outcomes of this conference was the foundation of a journal devoted to semigroup-related matters, *Semigroup Forum*, in 1970. Not only has this journal provided a focus for the international semigroup community, but it has also improved the visibility of the theory within the wider mathematical world. The communication of semigroup theory through monographs, seminars, and conferences is the subject of the final chapter of the present book.

It should be emphasised that all of the above applies to the *algebraic* side of the theory of semigroups, upon which the present book focuses.[9] The development of the *topological* side of the theory has been dealt with elsewhere: see Hofmann (1976, 1985, 1992, 1994, 1995, 2000) and Lawson (1992, 1996, 2002). Nevertheless, the two parts of the theory are by no means mutually exclusive; comments on topological semigroups will be made from time to time in what follows.

There is a small number of earlier sources on the history of the algebraic theory of semigroups, which proved to be a useful starting point for my own investigations. These are Dubreil (1981), Howie (2002), Knauer (1980), Preston (1991), and Schein (1986b, 2002). A few historical notes may also be found in such books as Bruck (1958), Clifford and Preston (1961, 1967), Howie (1995b), and Lawson (1998). In the study of the progress of semigroup theory in the USSR, several useful survey articles are available: these are cited at the beginning of Chapter 9.

CHAPTER 2

Communication between East and West

The problems that hindered contacts across the Iron Curtain[1] are well recognised and, indeed, exist within living memory. Research has been carried out on cultural and academic exchanges between East and West during the Cold War and also in earlier decades (see, for example, Byrnes 1976 and David-Fox 2012). 'Cold War science' has also been the subject of several studies.[2] However, to the best of my knowledge, the communications issues surrounding 'Cold War mathematics' have not been treated systematically. Nevertheless, they have been dealt with in passing in a number of sources, usually on the subject of Soviet mathematics: see, for example, Gerovitch (2002), Zdravkovska and Duren (2007), Graham and Kantor (2009), and Gessen (2011). The present chapter represents a small contribution to the study of East-West communications; a great deal more work might yet be done. Since many of the issues faced by mathematicians in this regard were not discipline-specific, much of this chapter will deal with the communications difficulties of scientists in general. I should note at this point that 'West' is used here, and throughout the book, to refer to Western Europe and North America (and sometimes, playing loose with world geography, to Australasia), while 'East' usually denotes the USSR, though it also, on occasion, refers to the former communist countries of Central and Eastern Europe more generally.

Levels of contact varied over the decades, as different disruptive influences came and went. Following the communications problems that were caused by the First World War, the October Revolution, and the subsequent Russian Civil War, contacts between the newly formed Soviet Union and the West appear to have resumed at levels comparable to those between Russia and the West in previous decades: scientists travelled freely in and out of the Soviet Union, and scientific publications similarly went back and forth. However, with Stalin's rise to power in the late 1920s, the situation began to change: Soviet scientists came under criticism for publishing their work abroad, and anyone with foreign contacts was subject to suspicion. Indeed, in his book on Soviet physics, the historian Alexei B. Kojevnikov (2004, p. 85) referred to

> the twenty years of Stalin's dictatorship [early 1930s–1953], when Soviet science worked in virtual international isolation, with practically no foreign travel, visits, personal communications, conferences, or correspondence, and when most contacts with the rest of world science would be reduced to exchanges of printed works.

By the end of the 1930s, communication between Western scientists and their Soviet counterparts was, at best, extremely difficult, a situation that continued until

Stalin's death in 1953. During Khrushchev's 'thaw', however, international contacts began to grow: it increasingly became possible for Western scientists to travel into the USSR and, at least in principle, for Soviet scientists to go in the opposite direction. Fewer bars existed to personal correspondence and exchange of publications. However, the Soviet Union continued to be suspicious of Western visitors and of its own citizens who had any contact with foreigners. Moreover, the United States, for example, was, at times, equally suspicious of any of its scientists who strove to communicate with their Soviet counterparts. Thus, although by the 1960s levels of communication across the Iron Curtain were generally good, they were by no means always smooth. On the Soviet side, for example, correspondence was still often subject to postal censorship.

Although the ability to travel to conferences on the opposite side of the Iron Curtain and the levels of personal correspondence that were possible varied over time, the availability of publications from 'the other side' appears to have remained high throughout the relevant decades. A 1962 Western appraisal of Soviet mathematics indicated that access to publications, rather than travel or personal correspondence, was the main point of contact between mathematicians in East and West:

> Some of us have personal mathematical friends in the USSR, and some of us have visited there, but for the most part we know Soviet scientists by their mathematics, by what they publish. (Anon, 1962b, p. 3)

From the 1920s onwards, the appropriate authorities on both sides had done their best to get hold of publications, both scientific and otherwise, from the opposite side of what became the Iron Curtain. In the USSR, this was typically taken up by the Academy of Sciences, whereas in the West, efforts to obtain Soviet publications were much less centralised and often involved several different bodies in each country. Though coverage was by no means comprehensive, a very broad range of Western publications appears to have been available in the East and vice versa. Thus, although it is tempting to blame one side's lack of knowledge of the other side's work on the inaccessibility of publications (and this was indeed the case in some instances, as we will see), this certainly does not provide a full explanation. An additional, extremely important, factor was *language*.

Soviet scientists appear to have had a very good knowledge of at least one of twentieth-century Western science's three principal languages (namely, English, French, and German). Thus, provided the appropriate Western publications were available, Soviet scientists would usually not have much trouble in reading them. On the other hand, the typical Western scientist had a good knowledge of English, French, and German but would be rather less likely to be able to read Russian. The mere physical accessibility of a Russian source thus did not automatically guarantee that Western scientists would be able to keep abreast of Soviet advances. Indeed, Western scientific commentaries and personal reminiscences of Western scientists from the relevant period are littered with comments regarding the language problems. In the specific case of semigroup theory, for example, we have the following remark made by W. D. Munn, whose work we will meet in later chapters:

> We in the West became aware of much work in Russia, although we weren't always able to read it — and were subsequently accused of ignoring known results![3]

Indeed, the unwitting duplication of Soviet results by Western mathematicians led in some cases to a certain sense of injustice among their Russian counterparts when Soviet priority was not acknowledged (see below). Indeed, this phenomenon was present in Soviet science more generally; the historian Loren R. Graham (1972, p. 16), for example, referred to

> the long years in which appreciation of Russian science and technology by non-Russians was obstructed by linguistic barriers, ethnic prejudices, and simple ignorance.

I believe that, with regard to East-West contacts, the biggest obstacle emanating from the Soviet side was state interference: bureaucracy, postal censorship, and refusal of permission to travel. I will argue, however, that a more significant problem on the Western side was the language barrier.

Whatever its causes, the ignorance on one side of the work of the other led to many instances of duplication of research. For example, identical studies of the structure of certain proteins were conducted in Czechoslovakia and the USA (Medvedev, 1971, pp. 116–117), while Norbert Wiener (USA) and A. N. Kolmogorov (USSR) carried out parallel work in cybernetics (Gerovitch, 2002, p. 58). Mathematics seems to be particularly well supplied with examples of such duplication, as the author Masha Gessen (2011, p. 7) has observed:

> Soviet and Western mathematicians, unaware of one another's endeavors, worked on the same problems, resulting in a number of double-named concepts such as the Chaitin–Kolmogorov complexities and the Cook–Levin theorem.

This is certainly true in semigroup theory: in Chapter 10, we will meet, for example, the Wagner–Preston representation, and, in Chapter 11, 'Munn–Ponizovskii theory'.

The duplication of work occasionally led to accusations of plagiarism,[4] but it was usually recognised simply for what it was: an unfortunate result of communications difficulties. The semigroup theorist Boris M. Schein, who subsequently emigrated to the United States, commented:

> When I lived in the USSR, I took offence that often Western mathematicians did not reflect the priority of Soviet scientists, nor did they reference them in their work. In the West, I saw the other side of the coin. The vast majority of Western mathematicians did not refer to their Soviet counterparts only because it was almost impossible to learn anything about their results. Requests for offprints, sent to the USSR, remained unanswered. Letters sent to the USSR disappeared.[5]

The situation for mathematicians (or scientists more generally) in the other communist countries of Central and Eastern Europe seems, for the most part, to have been somewhat easier. The Czechoslovakian and Hungarian mathematicians whom we will meet in later chapters appear to have had easy access to Western sources, though, perhaps for political reasons, the range of Soviet sources available to them seems to have been more limited. Travel to and from these countries was generally easier, both for Soviet scientists and for Westerners. For example, the participation of Central and Eastern European mathematicians in Paul Dubreil's Paris algebra seminar (Section 7.1) will be noted in Section 12.2. Moreover, the prominent Slovak semigroup theorist Štefan Schwarz was able to travel to the 1974 International Congress of Mathematicians in Vancouver (p. 188), though to what extent his standing within the Czechoslovak Communist Party made this possible is not clear. The staging (by Schwarz) of the first international conference on semigroups near Bratislava in 1968 was probably connected with the (relative) ease of travel to Czechoslovakia at that time — see Section 12.3.

The material of this chapter is arranged as follows. We begin in Section 2.1 with a general discussion of communications between scientists in East and West from the 1920s up to the 1980s. We will see that, as noted above, contacts were initially quite easy, though they diminished under Stalin, only to recover after his death. Even at this stage, however, a number of problems plagued East-West contacts; I outline these. The focus in Section 2.1 is upon *personal contacts* between scientists, by which I mean correspondence and face-to-face meetings, usually at conferences. This section also contains a discussion of the attempts by the Soviet authorities to impose a Marxist ideology on mathematical research. This subject is of great relevance here since state ideology was often the filter through which Soviet officials viewed potential contacts and exchanges with other countries. It is, however, rather convoluted, given the inconsistent and often contradictory nature of ideological pronouncements; I attempt to give a simplified exposition of the relevant details.

Issues surrounding access to publications are addressed in Section 2.2, which is divided into two subsections. The first concerns *physical access*: the availability of the publications of one side in the libraries of the other, the ability to exchange offprints, and the general appraisals of Soviet science that were produced in the West. The second part of Section 2.2 deals with linguistic matters: the ability of one side merely to *read* the publications of the other and the assistance that was rendered in the form of, for example, systematic translations.

At certain places in this chapter, most notably in Section 2.1, it will seem that a great deal more space is being given to a discussion of the situation in the Soviet Union than to that in the West. The reason for this, as I will argue below, is that most of the communications difficulties (at least those of a political character) that occurred during the years in question originated in the USSR, through the policies of the state.

2.1. Communication down through the decades

Following the upheaval of the October Revolution and the subsequent Russian Civil War, efforts seem to have been made by Soviet scientists to re-establish contacts with their counterparts in other countries. At this stage, there do not appear to have been any bars to correspondence, and Soviet scientists were still able to travel. The topologist P. S. Aleksandrov, for example, travelled widely in Western Europe during the 1920s, establishing contacts with such figures as Emmy Noether (see Aleksandrov 1979). We might also note the fact that 37 Soviet delegates (27 Russians and 10 Ukrainians) were listed as members of the 1928 International Congress of Mathematicians in Bologna (Bologna, 1929).[6] In 1925, the Soviet authorities even organised an international conference to commemorate the 200th anniversary of the Academy of Sciences. Many foreign delegates, in fields ranging from mathematics to oriental studies, attended at state expense. They were entertained lavishly and, as well as attending a range of lectures, were permitted to tour Soviet factories and laboratories. Although, in the words of one British attendee, the conference “had been organised largely with an eye to its propaganda-value” (Bateson, 1925, p. 681), the foreign delegates appear to have enjoyed free interaction with their Soviet counterparts. A further conference celebrating the Academy’s bicentenary was held in London later the same year and was attended by many Soviet academics (Anon, 1925). Furaev (1974) lists many more instances of Soviet scientists attending foreign conferences, and of American scientists visiting the USSR, during the 1920s.

Thus, as far as can be determined at this distance in time, mathematicians on either side of what later became the Iron Curtain seem to have continued, in the 1920s, to enjoy a level of communication comparable to that available before the First World War. Indeed, both sides viewed scientific exchange as being of the highest importance (Medvedev, 1979, p. 16). The expense of travel, as well as linguistic considerations, may have placed limits on contacts between mathematicians, but this had always been the case.

By the end of the 1920s, however, the situation in the Soviet Union was beginning to change, with increasing state control of the academic sphere. The principal manner in which this control was exerted was through the demand for ideological orthodoxy. From the first days of the USSR, attempts had been made to remodel all academic disciplines in order to make them consistent with state ideology: the Marxist philosophy of *dialectical materialism*. This was the philosophical scheme whereby the world was to be understood only in terms of ‘real-life’ experience (that is, without reference to supernatural agents), and the historical development of the world at large was to be explained in terms of the notion of a *dialectic*: a ‘tension’ between contrasting ideas which drives change. In the sciences, this dialectical materialist insistence upon ‘real-world’ experience translated into an emphasis on experimental sciences. Indeed, its evidence-based nature made science in general particularly attractive to Marxist philosophy.[7]

Mathematics, however, posed a problem for Soviet philosophers.[8] Where mathematics was concerned, the ideological position was clear: applied mathematics was to be favoured over pure. The more abstract branches of mathematics, with no apparent grounding in the real world, were to be branded as ‘idealistic’ (the gravest of accusations within Soviet philosophy). Mathematics, like all other disciplines, was to be remoulded along dialectical materialist lines. When it came to achieving this end, however, a major hurdle faced the Soviet philosophers of the 1920s: their knowledge of mathematics, particularly of newer areas, such as set theory, was inadequate. In fact, such inadequacies were not limited to mathematics; as Kojevnikov (2004, p. 280) has commented,

> despite their professed respect towards science, Bolsheviks with very few exceptions did not possess even basic scientific literacy and could be highly suspicious of scientists in real life.

The construction of an ideologically acceptable version of mathematics required, for one thing, the identification of appropriate dialectics that could be regarded as having driven the development of mathematical thought down through the ages, but the ideologues simply did not have the necessary understanding of mathematics or its history. Unable to propose a dialectical materialist alternative, they were reduced merely to condemning instances of perceived idealism in mathematics. Soviet mathematicians in this period were able to continue with their work largely unhindered, though their foreign contacts occasionally drew criticism: the 'idealistic' notions of the 'decadent' West were easy targets for the ideologues.

As Stalin strengthened his grip on power at the end of the 1920s and in the 1930s, attempts to make ideological inroads into the sciences, including mathematics, were stepped up, one of the most infamous examples of Soviet ideological interference in science being Lysenkoism in genetics (Joravsky, 1970). The focus now was not merely on the specific concepts that Soviet mathematicians studied, but also on the ideas that they were exposed to, particularly those coming from outside the Soviet sphere of influence: the state did not want the minds of Soviet researchers to be 'polluted' by the 'idealistic' notions of the West. Moreover, it was not merely mathematics that came under scrutiny from the ideologues during this period, but also, increasingly, the mathematicians themselves. Those with extensive foreign contacts were regarded with particular suspicion. Some were accused of a variety of offences, ranging from 'philosophical idealism' to being counter-revolutionaries.

The most high-profile example of an ideological attack on a Soviet mathematician was that launched against the Moscow-based analyst N. N. Luzin in 1936. A series of anonymous articles appeared in *Pravda* (see, for example, Presidium of the Academy of Sciences of the USSR 1936), accusing him of, among other things, plagiarism and seeking to undermine Soviet science by publishing his best work in foreign journals. Luzin certainly had strong connections with the set theorists in Paris, having spent some time there in 1905–1906 (see Graham and Kantor 2009 for more on the French influence on Luzin). Accusations of sabotage aside, Luzin's attackers had plenty of ammunition to throw at him: of the 93 publications listed for Luzin in the survey volume *Mathematics in the USSR after forty years* (*Математика в СССР за сорок лет*) (Kurosh *et al.*, 1959, vol. 2, pp. 420–422), 45 were published abroad (see Table 2.1). Notably, his final foreign publication was in 1935.

The case against Luzin was considered by a special commission of the Academy of Sciences, where a number of his former students, including, for example, P. S. Aleksandrov, spoke against him. We see here the inconsistency of such attacks on Soviet scientists: Aleksandrov, for one, was a frequent contributor to foreign journals (see Table 2.1). Inevitably, the commission ruled against Luzin, and he was dismissed from all his official positions. However, the punishment went no further than this. In spite of the large amount of research that has been conducted into the now-infamous 'Luzin affair',[9] the reasons for this relatively mild treatment remain somewhat mysterious. One of the more plausible explanations is that Stalin was sending a message to Ernst Kolman, the particularly rabid ideologue who led the campaign against Luzin and who probably penned the attacks published in *Pravda*: that he (Stalin), and he alone, would control the purges (see Graham and Kantor 2009, p. 160); another suggestion is that the case was useless for propaganda purposes — it would have been very difficult to get the general Soviet public to engage with the idea of 'sabotage' in mathematics (Gessen, 2011, p. 6). The judgement against Luzin was finally overturned in January 2012 (see Kutateladze 2013).

As already noted, the year of the attack marked the end of Luzin's foreign publications. Indeed, one of the major effects of the campaign against Luzin appears to have been the discouragement of Soviet mathematicians from submitting their papers to foreign journals, lest they find themselves on the receiving end of similar 'anti-Soviet' accusations. We may see this if we look again at the survey volume *Mathematics in the USSR after forty years*, which lists the publications of Soviet mathematicians up to 1957. By no means did all Soviet authors who were active before 1936 have a history of publishing in foreign journals, but if we examine the publications lists of those who had, then we may see the effect alluded to here. Table 2.1 provides some figures for the numbers of foreign publications (in relation to the total numbers of publications) of a selection of prominent Soviet mathematicians who were active within the relevant period and who, prior to 1936, had an extensive history of publishing abroad.[10] These figures show that, although many of these mathematicians continued to publish abroad after 1936, there was a dramatic drop in publications sent to foreign journals.

This drop-off in the number of foreign publications by Soviet mathematicians had in fact been underway since the start of the 1930s: the Luzin affair merely strengthened the pre-existing trend.[11] Aside from the state's suspicion of any Soviet citizens who chose to publish their work abroad, this trend appears to have arisen from certain nationalistic considerations: as G. G. Lorentz (2002, p. 194) put it, during the 1930s, the

> glorifying of elements of the Russian past ... led to ignoring the achievements of non-Soviet scientists and to the isolation of Soviet sciences.

Perhaps because it was a cheap pursuit in a country where resources were limited (the so-called 'blackboard rule'[12]), Soviet mathematics was, at this stage, already assuming the world-leading position that it would occupy for several decades. It was thus felt by some that important Soviet advances in mathematics ought to be published in Soviet journals.[13] For example, the first issue of the 1931 volume of the journal *Matematicheskii sbornik* (*Математический сборник* = *Mathematical Collection*) began with a short editorial piece entitled 'Soviet mathematicians, support your journal!' ('Советские математики, поддерживайте свой журнал!'). This editorial challenged the popular view that publication in foreign journals was a good way to disseminate Soviet research around the world:

> Among the majority of Soviet mathematicians, there was preserved a tradition of publishing their best work in foreign journals. What is more, there was also a widely-held point of view that saw the publishing of a large number of our works abroad as a positive development .... This view is of course incorrect: scattered throughout journals in Germany, France, Italy, America, Poland, and other bourgeois countries, Soviet mathematics does not appear as such, unable to show its own face.
>
> The growth in [the number of] scientific personnel within the USSR ... sets before us the task of creating a journal reflecting these changes and organising Soviet mathematics in the direction of active participation in socialist construction.
>
> ...
>
> A group of Moscow mathematicians addressed the editors in a letter, in which they undertake to publish their articles, in the first place, in "Matematicheskii sbornik", and appeal to the other mathematicians of the Soviet Union to do likewise.[14]

TABLE 2.1. Numbers of foreign publications (as listed in Kurosh *et al.* 1959) of prominent Soviet authors who were active in the 1920s and 1930s and who had an extensive history of publishing abroad prior to 1936.

Name	**Number of publications up to 1936 (inclusive)**	**Number of foreign publications up to 1936 (inclusive) (also as percentage of preceding column)**	**Number of publications 1937–1957 (inclusive)**	**Number of foreign publications 1937–1957 (inclusive) (also as percentage of preceding column)**
P. S. Aleksandrov	64	51 (80%)	79	10 (13%)
S. N. Bernstein	94	50 (53%)	93	6 (6%)
L. V. Kantorovich	49	17 (35%)	65	7 (11%)
A. Ya. Khinchin	79	31 (39%)	70	3 (4%)
M. A. Lavrentev	36	15 (42%)	35	2 (6%)
N. N. Luzin	67	45 (67%)	26	0
D. E. Menshov	20	17 (85%)	35	0
L. S. Pontryagin	27	17 (63%)	44	4 (9%)
V. I. Smirnov	25	9 (36%)	32	0
A. N. Tikhonov	15	10 (67%)	16	0

Such gentle encouragement later gave way to the rather less subtle hints afforded by the Luzin affair. The editors of the above piece also made some comments on the use of foreign languages within *Matematicheskii sbornik*; we will return to these in Section 2.2.2, where I will also make some comments on foreign authors and *Matematicheskii sbornik*.

Thus, as the 1930s progressed, efforts by Soviet mathematicians to maintain contacts with their foreign counterparts were gradually strangled, in spite of the fact that many foreign specialists were still being brought into the USSR in order to assist in the building up of Soviet industry (Medvedev, 1979, p. 28). The submission of papers to overseas journals became rather dangerous to attempt, and personal correspondences with individuals in other countries were abandoned, in fear of accusations of 'collaboration' with foreign powers (Josephson, 1992, p. 597). Soviet scientists in general were also barred from attending international conferences, not only to prevent them from being 'infected' by 'alien' ideas (concerning both science and politics), but also to stem a potential Soviet 'brain drain', an issue that came to be of enormous concern in Soviet newspapers (see Medvedev 1971, p. 155): two high-profile scientists, the geneticist T. G. Dobzhanskii and the physicist G. A. Gamov, had travelled abroad and never returned.[15] Only a select few trustworthy 'Party scientists' were now permitted to travel abroad, a situation that endured for decades. Conversely, Western scientists were not permitted to travel widely within the USSR during the 1930s.

Against the backdrop of the Second World War, a spirit of cooperation emerged between the mathematicians of East and West, even if this did not translate into cooperation on any practical level: wartime communications problems were added to the general difficulties experienced by anyone trying to establish a connection across the Soviet border. Nevertheless, in 1941, 93 American mathematicians made a statement of solidarity with their Soviet counterparts in a letter delivered to the Soviet embassy in Washington and subsequently printed in *Nature*:

> We ... send our greetings and express our heartfelt sympathy to our colleagues of the Soviet Union in their struggle against Hitler fascism [*sic*]. ... We are deeply impressed by the heroic stand of the Soviet peoples and know that the mathematicians of the Soviet Union are doing their part in this supreme effort. The bonds between mathematicians in the United States and the Soviet Union are particularly strong since during the past two decades the center of world mathematics has steadily shifted to these two countries. We know many of you personally and more of you through your scientific writings. ... With best wishes for a successful fight against the evil forces of fascism, we remain, fraternally, your colleagues in the United States. (Anon, 1941a)

The authors of this statement were evidently allowing themselves a little 'wartime licence' in referring to the "particularly strong" bonds between the mathematicians of the USSR and the USA. A response, signed by 64 Soviet mathematicians and written in the same high-flown language, was printed in *Nature* just a few weeks later:

> Your splendid message, dear colleagues, found wide response in the hearts of the scientists of our country. We read it with feelings of all the more appreciation and satisfaction in that it again

> emphasized the community of thought and the friendly ties between the mathematicians of the U.S.A. and the U.S.S.R. Many years we jointly worked with you on the development of our science, many of our American colleagues were our welcomed guests, while with a still greater number of American scientists we conduct friendly scientific correspondence. This mutual cooperation was very fruitful and led to a number of important scientific discoveries. ...
>
> ...
>
> Our science too has been placed at the service of our country for the destruction of Nazism. Soviet mathematicians, like all Soviet scientists, participate in this fight in common with the whole people. ... On this momentous day your message, dear friends, has been received by us as proof of the unity of Soviet and American scientists and their determination to fight the twentieth-century vandals till the end. Let the friendship of the Soviet and American scientists be the surety of the friendship of our great nations, the surety of the victory of democracy over the dark forces of Hitlerism. (Anon, 1941b)

Once again, presumably in the spirit of wartime cooperation, the connections between Soviet and American scientists were overstated, though it would be nice to think that the noble sentiment expressed at the end of the letter was more than just wartime rhetoric.[16] The struggles of mathematicians that were alluded to here, however, were by no means exaggerated — the wartime experiences of some Soviet semigroup theorists will be recounted in later chapters.

Although the letters quoted above expressed the wartime solidarity of mathematicians in East and West, they do not appear to have improved the general degree of communication. Indeed, following the defeat of Nazi Germany, the spirit of wartime unity quickly dissolved, and contacts across the newly descended Iron Curtain returned to their pre-war levels, owing, in no small part, to Stalin's further strengthening of his grip on power:

> [i]mmediately after the Second World War many intellectuals in the Soviet Union hoped for a relaxation of the system of controls that had been developed during the strenuous industrialization and military mobilizations. Instead, there followed the darkest period of state interference in artistic and scientific realms. (Graham, 1972, p. 18)

During the second half of the 1940s, foreign publication by Soviet scientists continued to be frowned upon. In 1947, for instance, *Pravda* published an attack on a number of leading scientists for their continued publication of articles in foreign journals, which the paper described in terms of "unpatriotic acts" and "servility to the West" (Gerovitch, 2002, p. 15). During this period, ideology once again became the basis for assaults on science and scientists. For example, a group of Leningrad-based mathematicians was singled out for criticism in 1949 in connection with their supposedly 'idealistic' research pursuits. Among these was the prominent semigroup theorist E. S. Lyapin; we will examine the attack on him in Section 9.1.

As in all other areas of Soviet life, Stalin's death in 1953 marked the beginning of a new era for Soviet academics. This was the time of the famous 'thaw', during which the Soviet Union became (slightly) more liberal. Although Lysenkoism, for instance, was not formally abandoned until 1964 (owing to Khrushchev's continued support of Lysenko), the application of state ideology to the sciences became a little less dogmatic. This was certainly the case in connection with mathematics. The relative autonomy that mathematicians had enjoyed for years now seemed secure. A general pride in the international standing of Soviet mathematics protected it from the depredations of those few Marxist philosophers who still wanted to restructure it on the basis of dialectical materialism. Moreover, these were the more fanatical ideologues, whose deep knowledge of Marxist thought was still not paired with a decent understanding of mathematics. Their pronouncements on mathematical issues therefore continued to sound rather empty. Nevertheless, state ideology had not gone away, and Soviet mathematicians were required at least to pay lip service to it.[17] This was often done by means of a few vague, positive comments about dialectical materialism at the beginning of a published work (more so in longer publications like books than in papers), before getting down to the more important business of mathematics. With regard to abstract algebra, Soviet mathematicians had never withdrawn from this area, in spite of the occasional attacks upon it for its 'idealism'. Its study had generally been justified, often quite tentatively, using an argument that had been advanced by A. N. Kolmogorov in the mid-1930s: that, far from being removed from real-world applications, greater abstraction in mathematics enables one to encompass a wider range of applications in a single theory (Kolmogorov, 1934). In the post-Stalin era, Soviet mathematicians became much bolder in their assertion of this principle.

Khrushchev's thaw also appears to have opened up a greater possibility of communication between Soviet scientists and their counterparts in other countries. As Gerovitch (2002, p. 155) puts it:

> Soviet scholars could now publish abroad, attend international conferences, receive foreign literature, and invite their foreign colleagues to visit. The division into "socialist" and "capitalist" science no longer held; claims were made for the universality of science across political borders.

Indeed, the fact that foreign scientists were now able to travel into the USSR much more easily is demonstrated by an account of the 1956 Third All-Union Mathematical Congress in Moscow: it notes the presence of around 60 foreign delegates, hailing from the UK, Bulgaria, Hungary, both East and West Germany, India, Italy, China, Norway, Poland, Romania, the USA, France, Czechoslovakia, Sweden, and Yugoslavia (Vinogradov, 1956). For a contemporary appraisal of Soviet science in the few years after Stalin's death, see Turkevich (1956) — this is an early example of a type of article that began to appear rather frequently during the years of the Cold War: an American assessment of Soviet academic advances and capabilities. I will say a little more about such articles in Section 2.2.1.

In some respects, the Soviet authorities even went so far as to *encourage* international cooperation, probably with a view to catching up with the West in those disciplines in which Soviet research lagged behind (Medvedev, 1979, Chapter 6). Gerovitch (2002, pp. 156–157) notes that

> detailed instructions [were issued] on how to obtain the permission for a foreign trip, how to invite foreign colleagues, how to obtain the permission to publish an article abroad, and how to

> maintain correspondence with foreign scholars and scientific institutions. Restrictive as they were, these instructions nevertheless legitimized what had been unthinkable in the late Stalinist period: regular contacts and exchanges between Soviet scientists and their Western colleagues.

However, the circumstances surrounding international contacts in this period were by no means utopian: procedures may have been in place to enable Soviet scientists to make contact with their foreign counterparts, but this did not mean that the necessary permissions were easy to obtain, nor that the bureaucracy had any intention of making it a smooth process. A good account of the difficulties experienced by someone trying to use this system is provided in the writings of the biologist, and subsequent dissident, Zhores Aleksandrovich Medvedev (Жорес Александрович Медведев). In the late 1960s, Medvedev authored two essays, detailing his difficulties (during the 1960s and late 1950s) in establishing and maintaining contacts with biologists in the West. The first, 'International cooperation of scientists and national frontiers' ('Международное сотрудничество ученых и национальные границы') detailed the bureaucratic hurdles that a Soviet scientist needed to overcome in order to be allowed to take up an invitation to attend a foreign conference. In a very measured tone, Medvedev related some of his own difficulties in this regard and outlined some very precisely worded complaints to the relevant authorities. The second essay, 'Secrecy of correspondence is guaranteed by law' ('Тайна переписки охраняется законом') described Medvedev's suspicions regarding postal censorship in the USSR, which was officially illegal under the Soviet constitution. These treatises were first circulated in the USSR through 'samizdat' (self-publication) before falling into the hands of a British publisher.[18] English translations of the two essays, the first under the title 'Fruitful meetings between scientists of the world', were published in the UK in 1971 in a single volume entitled *The Medvedev papers*. A sense of urgency in connection with the issues raised in Medvedev's essays was created by the inclusion of the subtitle *The plight of Soviet science today*.[19] Indeed, the urgency was present in Medvedev's writing: employing a biological metaphor, he explained that something needed to be done about what was, in his view, the lack of vitality in Soviet science that had resulted from too much 'in-breeding' (Medvedev, 1971, p. 151).

Establishing Medvedev's credibility for a Western readership was evidently very important for the British publishers. In the blurb on the dust jacket, they stressed his scientific credentials and noted that the book was not intended to be anti-Soviet: instead, they saw it as a critique of certain issues pertaining to Soviet science policy. Indeed, Medvedev did not simply write about the failings of the system: perhaps more in hope than in expectation, he also suggested ways in which the Soviet system might be adapted in order to facilitate international contacts. Medvedev's writings therefore provide us with a credible account of some barriers to international scientific cooperation.[20] Most of Medvedev's specific examples concern the situation facing Soviet biologists, but, in fact, very few of these are discipline-specific, so Medvedev's essays may be taken, in conjunction with other sources to be mentioned below, as a reflection of the problems experienced by mathematicians also.

Judging by Medvedev's account, one of the biggest problems facing any Soviet scientist who wanted to travel abroad, or even to send an international letter, was bureaucracy. Moreover, it was not merely the highly complicated nature of the bureaucracy, but also the fact that it was contradictory: different officials in different institutions had conflicting interpretations of what was or was not allowed. Indeed, even in cases when it was generally understood that a certain action was permissible (the sending of a particular parcel, say), an official might still refuse to endorse it in the absence of explicit written permission to do so: no minor functionary wanted to take the initiative, for fear that policies would change and that their signature or personal stamp would later identify them as someone who had (retrospectively) violated a new directive.

In order to apply for permission to travel abroad, a Soviet citizen was required to prepare a collection of documents termed an 'exit dossier', which they then submitted to the relevant authority. In the case of Medvedev's efforts, which he outlined in the first section of 'International cooperation of scientists and national frontiers', to take up an invitation to the Fifth International Congress of Gerontology in San Francisco in 1960, the appropriate authority was the Soviet Ministry of Health, which had already agreed that the USSR should be represented at the congress. Medvedev described the necessary arrangements as follows:

> An 'exit dossier' ... consists ... of a series of forms similar to 'security forms' for those about to work in a secret establishment. These forms include the usual questions on near relatives, any terms of imprisonment, and a description of all the posts which the intending traveller has held in his entire life. In addition, the 'exit dossier' includes a detailed autobiography, copies of the birth certificates of children, a copy of the marriage certificate, medical report, itinerary of the journey indicating the duration and purpose of the visit, and a character reference which constitutes the main document. All the papers are made out in duplicate, and to them must be affixed twelve photographs. The character reference, which must indicate political maturity and moral stability, must be endorsed in triplicate by all one's immediate public and administrative superiors and confirmed at a meeting of the Party Bu[r]eau or Party Committee and then at a meeting of the Bureau of the Regional Committee of the Communist Party of the Soviet Union. After this it is endorsed with the Regional Committee seal. All these papers make up the 'exit dossier', which must be forwarded to the Ministry [of Health] and then to the Central Committee of the Communist Party of the Soviet Union. (Medvedev, 1971, p. 13)

If such an application passed the Party Central Committee, it would next be sent for approval by the Section of Science and Higher Education, before being forwarded to an 'exit commission'. This commission included representatives of the KGB and was the stage of the process where the contents of the applicant's state security file were considered. If the application managed to pass all of these hurdles, it would go finally to the Ministry of External Affairs, who would prepare a foreign passport for the potential traveller and apply directly to the appropriate embassy for a visa. The traveller might only receive his or her passport and visa a few hours before departure. Of course, a vast proportion of applicants would not even come close to these latter stages — the process could be blocked, with no reason given, at any point (this is what happened in Medvedev's case). An entirely separate series of bureaucratic obstacles might also have needed to be navigated in order to make the necessary travel arrangements (see Medvedev 1971, pp. 272–274). Indeed, given the convoluted nature of the process and the suspicion to which they would be subjected, many scientists simply did not apply in the first place and never attempted to attend international conferences. Naturally, if the applicant was a Party member, and therefore automatically deemed 'sound', the process was somewhat simpler. In fact, delegates sent by the Soviet Union to international conferences were very often chosen on the basis of their Party membership status rather than their academic credentials; much to the frustration of Western conference organisers, the eventual Soviet attendees of conferences were often not those whom they had invited, but 'politically acceptable' replacements, selected by the Soviet authorities, who didn't necessarily have any deep knowledge of the field in question. The problems outlined here thus make it particularly easy to scoff at the following comments made in the *Pravda* editorial column on 11 September 1966, under the heading 'International connections of scientists' ('Международные связи ученых'):

> A weighty contribution to the treasure house of knowledge has been made by Soviet scientists. In addition they have gained an opportunity of studying the achievements of foreign colleagues, in order to use them in the interests of the further development of Soviet science and technology and the successful building of communism. ... Soviet scientists visit many countries of the world to deliver lectures, to take part in consultations and joint projects with foreign specialists. (Translated in Medvedev 1971, p. 68)

For further details on the application procedure outlined above, see Medvedev (1971, pp. 195–208) or Levich (1976). From around 1960, a shorter application process, requiring fewer documents to be submitted, was adopted for travel to other socialist countries (Medvedev, 1971, pp. 208–215). This will be particularly pertinent in the final chapter when we consider the staging of the world's first international conference on semigroups in Czechoslovakia in 1968.

In his essay, Medvedev described the letters of frustration that he had received over the years from Western scientists who were trying to bring him to conferences in the United States, for example, but who had not been able to finalise plans because official permissions had not yet been granted. He recounted also those instances in which he had been scheduled to give lectures abroad but had been forced to cancel at the last minute when his official approval had been withdrawn for no discernible reason. In such instances, his superiors had directed him to make up an appropriate excuse: family illness, heavy workload, etc. Medvedev thus came to be concerned about the impression that these types of cancellations and excuses would be making on the scientists of other countries. In his essays, he noted what he perceived to be the impatience and the lack of understanding of some of his British and American correspondents (Medvedev, 1971, pp. 33, 62, 299). However, he appears to have been cheered somewhat by a spoof letter, written by the British physicist John Ziman, that was published in *Nature* in 1968. In this piece, entitled 'Letter to an imaginary Soviet scientist', Ziman described to his fictional addressee an (equally fictional?) experience that would have been familiar to many Western conference organisers. In January, a "mutual friend" sought to invite a Russian delegate to a conference that he was planning for December and so wrote directly to the invitee. By the end of March, he had received no reply, so, being determined to include Soviet contributions, he next wrote to the Academy of Sciences and the Ministry of Education, inviting six Russian researchers by name. These letters were not answered until October, when a curt note informed the organiser that the Soviet delegates at his conference would be Y and Z, neither of whom was on the list of six invitees, nor were they names that were familiar. No further communication was received from the Soviet authorities until three days before the start of the conference, when a demand was made for accommodation for eight Russian scientists, none of whom subsequently turned up. Finally, in the last two days of the conference, a trio of Russian delegates arrived unannounced. One of them did not appear to be particularly familiar with the subject of the conference, but they all insisted on reading out their unscheduled lectures anyway.[21]

Having examined Medvedev's comments on Soviet bureaucratic difficulties in connection with attendance of foreign conferences, it is interesting to get some indication of how this matter was perceived in the West, where it was impossible to know what was really going on behind the Iron Curtain. There is a light humour in Ziman's letter that indicates that he had some idea of what was happening, and Medvedev (1971, p. 131) appears to have taken some comfort in this: that there was at least one Western scientist who recognised that their Soviet counterparts were not, in general, being deliberately rude and awkward. For our purposes, Ziman's letter confirms that these types of problems were not confined to the biological contexts described by Medvedev but also cropped up in physics and presumably in academia more generally. I suggest that Ziman's reasons for writing this letter were twofold. First of all, he wanted to encourage his Western colleagues to be a little more sympathetic towards the plight of their Soviet counterparts and to continue to attempt to invite them, even in the face of such apparent rudeness. Secondly, there was also a message here for the Soviet officials who were causing the problems in the first place. Knowing that *Nature* was circulated in the USSR, Ziman referred to the "buffoonery" in the story told above and made a strong case for unhindered contacts between scientists across the Iron Curtain. We can in fact say with certainty that at least some Soviet officials read Ziman's article and recognised it for what it was: the Soviet biochemist W. A. Engelhardt wrote a reply to the letter, refuting Ziman's allegations and accusing him of seeking to undermine British-Soviet scientific relations (Engelhardt, 1968). However, there is evidence that Ziman's letter did not have a wide circulation in the USSR, as we will see in Section 2.2.1. As a final comment here, we note that it was Ziman who provided the foreword for *The Medvedev papers*.[22]

As noted above, the theme of the second of Medvedev's essays was the issue of postal censorship in the USSR. He related how he had suspicions that many of his letters abroad, particularly those sent to the United States, were being intercepted by the Soviet authorities and were not reaching their destinations. Any enquiries that he made into this matter were met with the indignant assertion that postal censorship was illegal in the Soviet Union. Once again, a great deal of bureaucracy, with contradictory rules, regulations, and demands for official permissions, stood in the way of a Soviet scientist sending anything abroad, be it a paper for publication or merely a letter to a foreign colleague. The submission of papers to foreign journals, for example, was no longer explicitly discouraged, but, as with travel to foreign conferences, the associated bureaucratic processes were by no means easy to navigate. We have, for instance, the following comments of Boris M. Schein, made in connection with a paper (Schein, 1973) that was eventually published in the *Czechoslovak Mathematical Journal*:

> The KGB representative at my university refused to initiate a long and involved process determining whether the contents of [my paper], if published abroad, might subvert the interests of the Soviet state and told me that the fewer papers I wrote, the easier his life was made. Few people are so frank as secret police in evaluating your research output! So I smuggled the manuscript ... from the USSR illegally and, for the subsequent six years, lived under Damocles' sword. (Schein, 2002, p. 154, footnote 9)[23]

Moreover, as Medvedev related, the Soviet authorities did not merely place restrictions on material that was sent out of the USSR, but also on that coming in — more comments will be made on this issue in Section 2.2.1. With reference to postal censorship, it is interesting to note that it was not merely *Soviet* scientists who were afflicted in this regard (although it was certainly a bigger problem in the USSR): in the United States, there were concerns that new legislation, designed to block incoming political propaganda, would affect the receipt of Soviet scientific literature (DuS., 1961a,b, 1962).

So far in this account of contacts across the Iron Curtain, I have said a great deal more about the situation on the Soviet side than that in the West. One of the main reasons for this is that, arguably, there is much more *to* say: the vast majority of obstacles to international communication originated with the Soviet authorities in their suspicion of capitalist countries and fear of 'ideological pollution'. In many respects, Western academics were reduced merely to *reacting* to the changing policies of the USSR in regard to international contacts: obtaining Soviet publications wherever and whenever possible, protesting to the Academy of Sciences at Professor X's lack of permission to travel to a Western conference, and so on. Even travel to the USSR from the West became easy in the 1960s, at least in comparison to the situation in the opposite direction (Medvedev, 1971, pp. 216–222). The communications difficulties that originated from Western sources were perhaps a little less likely to be of a political character: they stemmed more often from, for example, linguistic problems, such as those to be discussed in Section 2.2.2. Indeed, this is why, in the earlier discussion of the issue of 'foreign publication', I focused on the submission of Soviet papers to journals outside the USSR. Instances of Western authors publishing in Soviet journals are exceedingly rare. Insofar as I have been able to determine, there were no particular political bars to this; the main obstacle appears to have been language, perhaps in conjunction with the feeling that Soviet journals would not reach as wide a readership as Western ones.

As an illustration of the greater difficulties experienced by Soviet scientists in their attempts to forge links with their foreign counterparts, we might quote some figures given in *The Medvedev papers* regarding the number of Soviet scientists who were able to travel abroad in 1966 and the number of non-Soviet scientists who visited the USSR in the same year (Medvedev, 1971, pp. 129–130). The source of these figures was the Academy of Sciences, so we might automatically regard them with some suspicion, although they do seem plausible. The figures (which are not

TABLE 2.2. Soviet participation in International Congresses of Mathematicians, 1936–1962 (figures taken from Lehto 1998, pp. 69, 187).

Year	**Location**	**Number of Soviet delegates**
1936	Oslo	0
1950	Harvard	0
1954	Amsterdam	4
1958	Edinburgh	32
1962	Stockholm	42

broken down by discipline in any way) state that in 1966, 3,459 Soviet scientists travelled abroad under the auspices of the Academy of Sciences, while 9,305 foreign visitors were hosted by the Academy. Of these foreign visitors, 2,183 originated in the USA, as compared to the mere 95 Soviet scientists who went in the opposite direction. Although, as Medvedev pointed out, there were then ten times more scientists in the USSR than in the UK,[24] 820 British scientists visited the USSR in 1966, while the UK received only 326 Soviet visitors through the Academy of Sciences. Knowing that these figures referred only to foreign trips arranged through the Academy of Sciences, Medvedev attempted to scale up the numbers in order to gauge the proportion of Soviet scientists who were able to travel abroad in 1966; he arrived at a rough estimate of only one in thirty, or perhaps just one in forty. He suggested that the corresponding ratio for a Western European country might be between a half and three quarters, although he admitted that he did not have the data to back up this assertion. Whatever the true figure, it seems likely that it would have been considerably higher than that for Soviet scientists since the people of most other scientifically active nations were not subject to the same restrictions of movement. In the specific case of mathematics, it should be noted that 1966 was the year in which the International Congress of Mathematicians was held in Moscow, attended by a large number of Western delegates.[25] In the years after the 1928 Bologna ICM, Soviet participation had dropped away to nil but then started to pick up again in the 1950s (see Table 2.2). The 1966 congress put the seal on continued Soviet involvement: the next congress (Nice, 1970) was attended by over a hundred Soviet delegates (Comité d'Organisation du Congrès, 1971).[26]

Although, as argued above, Western scientists attempting to forge connections with their Soviet counterparts were less affected by the policies of their own governments than by those of the USSR, they were not entirely immune to homegrown political considerations. In the case of the United States, for example, officials at the State Department "often regarded the efforts of scientists to maintain international contacts as synonymous with communist sympathies" (Doel and Needell, 1997, p. 69). This was particularly so at the height of McCarthyism in the 1950s, when anyone with left-wing leanings could find him- or herself in the line of fire after the least provocation (see, for example, Wolfe 2013, pp. 33–37). Although specific, well-documented examples are lacking, one would imagine that an attempt by, say, an American mathematician to contact a Soviet counterpart would qualify, in this instance, as 'provocation'. Certainly, several American mathematicians with suspected Communist sympathies were prosecuted and lost their jobs. Indeed, the situation became serious enough for the American Mathematical Society (AMS) and the Mathematical Association of America (MAA) to form a joint Committee to Prevent the Loss to Mathematics of those Dismissed for Political Reasons; this committee helped to find new jobs for those who had been dismissed but did not attempt legal defences (Duren, 1989b, p. 431). High-profile cases, such as the prosecution, subsequent imprisonment, and dismissal from the University of Michigan of the mathematician Chandler Davis on the grounds of his left-leaning politics (Davis 1989; Choi and Rosenthal 1994) and also the refusal by US authorities to readmit Paul Erdős to the country in 1952 (Hoffman, 1998, pp. 128–129) may well have caused those seeking to establish correspondence with Soviet colleagues to think twice, lest they find themselves accused of Communist sympathies. Perhaps it is not unreasonable for us to draw loose parallels between these examples and the infamous 'Luzin affair'? In later years, the US State Department hindered international exchanges by placing severe restrictions on Soviet visitors to the USA (Shapley, 1974).

By the 1960s, however, there was a steady flow of scientific knowledge across the Iron Curtain, in both directions, and exchange programmes were even being organised (Byrnes, 1962). Debru (2013, pp. 64–65) summarises the situation very nicely in the following terms:

> scientists from the Soviet union and satellite countries were able to communicate with their colleagues from the Western world even in the 1950s and 1960s in spite of the mental walls erected by communist authorities in the Eastern block [sic] countries, and in spite of occasional difficulties. The situation of individual scientists did, however, vary depending on local circumstances, on the various disciplines and on the big institutions.

As we have already seen, during this period, scientists from one side of the Iron Curtain were attending conferences on the other. In many cases, they took the opportunity, upon returning home, to report on what they had seen.[27] For example, from around the same time, we have a very general report on Soviet science by one American observer (Bockris, 1958), an account of a Soviet mathematical congress by another (Lohwater, 1957), and several articles (published in volume 14 of *Uspekhi matematicheskikh nauk* for 1959) recording Soviet impressions of the 1958 Edinburgh ICM.[28] It is interesting to note that the presence of so many Russian mathematicians at this last conference was newsworthy enough to be reported in *The Times* (Anon, 1958d).

Thus, from the 1960s onwards, the ability of scientists (in particular, mathematicians) in East and West to communicate with each other continued to improve, although contacts were still plagued, from time to time (see Reid 1977), by the same difficulties that have been described in this section. For example, of the 36 Soviet mathematicians scheduled to attend the 1986 ICM in Berkeley, California, only 19 were, in the end, permitted to travel (Nathanson, 1986). By the 1980s, however, there no longer seem to have been any particular barriers to the exchange of publications, or even to Soviet mathematicians publishing in Western journals. By this stage, the major issue within the pages of Western periodicals, with regard to contacts with the Soviet Union, was the treatment of refusenik scientists, though I do not go into this here.[29]

2.2. Access to publications

In this section, I shift the focus from personal contacts between scientists to the issue of accessibility of published materials and the difficulties, both physical and linguistic, inherent in attempts to gain access.

2.2.1. Physical accessibility. On the whole, and perhaps surprisingly, physical access to materials from the opposite side of what became the Iron Curtain seems to have been quite good throughout the twentieth century, though, naturally, this statement requires some qualification. To begin with, there were notable periods (namely, two world wars) when the materials of one side simply did not reach the other (see, for example, Razran 1942). Another qualification to make here is that access to journals obviously depended very greatly upon where one was based: more materials would have been available in major academic centres (Oxford, Paris, Moscow, ...) than in small towns, for example. The point of origin of publications is of course also relevant: the journals published by major bodies, such as the Soviet Academy of Sciences, were more likely to be found in libraries abroad (indeed, were more likely to be *sought out* by libraries abroad) than those published at lesser-known institutions.

In contrast to the issues discussed in the preceding section, where whatever contacts existed between scientists in East and West were due largely to the efforts of individuals, national and local organisations had a much larger role to play in the exchange of publications. For example, shortly after the October Revolution, the British Committee for Aiding Men of Letters and Science in Russia was formed, with the goal of supplying British literary and scientific publications to the impoverished Russian intelligentsia.[30] Alongside this, the Soviet Academy of Sciences established a book exchange programme with the Smithsonian Institution in the USA (Furaev, 1974, English trans., p. 57). In the later years of the Soviet Union, *personal* subscriptions to Western journals were possible in certain circumstances (Medvedev, 1971, p. 343), but, as in the West, *institutional* subscriptions were the norm. The All-Union Institute for Scientific and Technical Information, or VINITI (ВИНИТИ = Всесоюзный институт научной и технической информации), was the body responsible for the bulk purchase and dissemination throughout the USSR of foreign scientific publications. Founded as a new branch of the Academy of Sciences in 1952, VINITI also published the *Referativnyi zhurnal* (*Реферативный журнал*), an abstracting journal, the mathematical series of which was established as a Soviet version of *Mathematical Reviews*; for some brief comments on Soviet abstracting activities, see DuS. (1956). In addition, VINITI published a monthly calendar of upcoming international conferences in all areas of science (Medvedev, 1971, p. 128). The institute continues today as the All-*Russian* Institute for Scientific and Technical Information.

Elsewhere in Central and Eastern Europe (for example, in Czechoslovakia and Hungary), a great deal of Western material appears to have been made available, principally through the efforts of the relevant Academies of Sciences. With regard to the work of, for example, Slovak authors that will be discussed in later chapters (in particular, Section 8.2), there is no indication that these researchers had any particular difficulties in accessing Western publications: we will see that these semigroup theorists cited Western work more often than Soviet. Their access to

Soviet materials, however, seems to have been rather more limited, if their infrequent references to Soviet publications are anything to go by, though of course this may have been politically motivated. As one final comment on Central Europe, I mention that Slovak mathematical publications were open to Western authors, as demonstrated, for example, by the paper Clifford (1963).

In the West, the acquisition of Soviet and, more generally, Eastern European scientific literature was not so centralised, with many different bodies working on the problem of obtaining Soviet sources. Nevertheless, as the American mathematician J. R. Kline noted in 1952:

> It is possible to secure Russian mathematical journals with comparative ease .... (Kline, 1952, p. 83)

In the United States, for example, the Library of Congress (Sherrod, 1958) and the Foreign Technical Information Center of the Department of Commerce (Anon, 1958a) both sought out Soviet (scientific) literature. As a guide to materials obtained, the Library of Congress published a *Monthly Index of Russian Accessions*; it was estimated that in 1958 the library was acquiring 60% of Soviet publications in the natural sciences (Sherrod, 1958, p. 958). The US National Science Foundation provided a great deal of funding for the purchase of Soviet materials (Anon, 1959). In the UK, both the British Library and the Bodleian Library in Oxford, for example, maintain a wealth of material published in the Soviet Union, though the occasional gaps in their collections may indicate that the acquisition of these did not always run smoothly. Avenues for individuals to obtain Soviet scientific materials were also open (Friedman, 1967).

The range of Western materials available to Soviet scientists appears to have been quite broad. Major national publications were certainly obtained by VINITI: Medvedev (1971, p. 45) indicated that he had access to the *Proceedings of the National Academy of Sciences of the USA*, for example. Moreover, we will see in later chapters that Soviet mathematicians were able to read, for example, David Rees's early papers on semigroups, which were published in the *Proceedings of the Cambridge Philosophical Society* (see Chapter 6).

Although many Western scientific sources were eventually made available in the USSR, their distribution was often delayed by several months. To a large extent, these delays were simply a result of the way in which VINITI operated. In order to save foreign currency, only one copy (or very few copies) of a given issue of a journal was purchased. This would then be reproduced and copies would be distributed to libraries across the USSR (Medvedev, 1971, pp. 124–125, 361–362) — the Soviet Union did not join the Universal Copyright Convention until 1973.

Censorship was also an issue for Soviet scientists wishing to read the work of their Western counterparts. All Western publications entering the USSR were checked for politically sensitive content, and any such material would be removed. Naturally, this applied rather more to publications of a general nature than to technical, mathematical texts. *The Medvedev papers*, for example, contains some comments on the censorship to which the journal *Science* was subject. Medvedev referred to the "complicated surgery" undergone by issues of *Science* prior to reproduction (Medvedev, 1971, p. 356) and discussed the lengths that censors would go to in order to disguise their handiwork:

> Deletions in reproduction are concealed whenever possible, and this entails more work. It is necessary to cut out and paste

> up a new contents, so that the titles of the articles removed do not give away the censor's action. But it is impossible to change the order of the pages in the journal, for in bibliographical notes, authors must, as a rule, cite the pages of the article cited. Therefore, in order to hide the deletion, the layout man replaces them by completely useless advertisements from other issues of the journal. If a sizeable portion of the text is withdrawn, then the wounds in the journal are left open; but so that the subscriber does not send the issue back as defective, below the journal's English contents list appears the sentence in Russian 'Certain pages are not included in this issue.' (Medvedev, 1971, p. 360)

The fact that *Science* was being censored by the Soviet authorities did not escape the notice of its editors: see Carey (1983) for an editorial concerning Soviet censorship, which would itself, one would assume, have been censored. Naturally, these redactions were not limited to *Science*: John Ziman's 'Letter to an imaginary Soviet scientist' (see p. 24) was removed from *Nature* before it reached the Soviet readership (Medvedev, 1971, p. 131).[31]

Western scientists faced different problems when it came to learning about Soviet research, one of the main ones being linguistic difficulties, which I discuss in Section 2.2.2. As already noted, however, physical access to materials was not a major issue. Via the various bodies mentioned above, a range of Soviet publications was made available in Western libraries. The Soviet Union was keen to export its technical publications in order to show off its scientific expertise, though the distribution of certain journals was sometimes blocked (Schwartz, 1951). Material of a political and ideological nature occasionally found its way into scientific journals (see, for example, the comments on page 41); as we have seen (p. 26), this did at one point put them at risk of censorship by the US customs, but this does not appear to have come to pass.

At the beginning of this section, I observed that the availability of a Soviet publication in the West would depend on several factors, one of them being its point of origin. National publications, such as those produced by the Academy of Sciences, had a wide circulation outside the USSR. Moreover, generally speaking, materials published in either Moscow or Leningrad stood a good chance of reaching Western libraries. In the case of mathematics, the journal *Matematicheskii sbornik*, published by the Moscow Mathematical Society (see Section 2.2.2), is one example of a Soviet periodical that was (and is) widely available in the West, as a perusal of modern electronic catalogues will reveal. Nevertheless, there are exceptions to the 'Moscow and Leningrad rule': although many of the publications of Leningrad State University, say, found their way into the West, those of the Leningrad State Pedagogical Institute, for example, in whose *Uchenye zapiski* (*Ученые записки* = *Scientific Notes*) E. S. Lyapin and his Leningrad-based semigroup school published a great deal of work, were not so widely circulated.

A large number of scientific periodicals was published outside the 'scientific core' formed by Moscow and Leningrad, but their availability in the West was rather more patchy. This is particularly unfortunate in light of what appears to have been the drive towards 'local publication' from the 1930s onwards: Soviet scientists were encouraged not merely to confine their publications to Soviet journals (in preference to foreign ones), but to publish much of their work in locally based journals. Thus, rather than submitting a paper to *Matematicheskii sbornik*, say, a mathematician based at Kiev State University, for example, would be expected to publish his or her work in the university's own journals. The export of such journals would almost certainly have been handled centrally, giving plenty of opportunity for them to be lost en route from Kiev to Moscow or to be mishandled by apathetical officials who cared little for 'provincial' publications. This was an issue of which some Western mathematicians seem to have been aware, for we find the following statement in a 1962 appraisal of Soviet mathematics:

> In the Soviet Union ... an important paper may turn up in the *Uchenye Zapiski* of a small pedagogical institute in Ulan-Ude or Irkutsk, buried among less noteworthy writings in the broad scientific field, and it may never be available outside the USSR. (Anon, 1962b, p. 13)

Nevertheless, some mathematical journals from far-flung corners of Russia and from other Soviet republics may be found in Western libraries. As a representative of the former, we have the journal *Algebra i logika* (*Алгебра и логика*), founded in Novosibirsk by A. I. Maltsev (Section 5.4) and seemingly quite influential in the West, even before it began to be translated systematically into English (see Table 2.5 on page 42). With regard to Western library holdings of journals from other Soviet republics, Ukraine is particularly well represented, but there are often many missing issues. In this connection, I should mention a Kharkov-based journal in which many of A. K. Sushkevich's early papers on semigroups were published. This was a mathematical journal produced jointly by the Ukrainian Scientific Research Institute of Mathematics and Mechanics (whose share was, however, later taken over by the mathematics department of Kharkov State University) and the Kharkov Mathematical Society.[32] This journal underwent several increasingly complicated name changes over the years, but for the purposes of this narrative, I refer to it by its original name: *Soobshcheniya Kharkovskogo matematicheskogo obshchestva* (*Сообщения Харьковского математического общества = Communications of Kharkov Mathematical Society*).[33] Efforts were made by the editors of the *Soobshcheniya* to make their journal *linguistically* accessible to a Western audience (see Section 2.2.2), though this did not of course solve the problem of physical availability. Indeed, the *Soobshcheniya* does not seem to have been (and still is not) an easy journal to get hold of outside of Ukraine.

A greater knowledge of the Soviet mathematical literature was also fostered in the West through the inclusion of abstracts for many Russian papers in the abstracting journals *Mathematical Reviews*, *Zentralblatt für Mathematik und ihre Grenzgebiete*, and, earlier, *Jahrbuch über die Fortschritte der Mathematik*. *Mathematical Reviews*, in particular, was founded (in 1940) with the goal of reviewing papers from as many languages as possible (Lehmer, 1989). Thus, researchers were at least able to learn about the broad strokes of a Soviet paper, if not the full details. Indeed, in its early years, *Mathematical Reviews* sold reproductions of all papers reviewed, though this service was soon withdrawn (Pitcher, 1988, pp. 72–73), probably owing to the sheer volume of available material. The coverage of Soviet sources in *Mathematical Reviews* was quite broad, a state of affairs that may have been striven for, in particular, by the editors S. H. Gould (1956–1962),[34] A. J. Lohwater (1961–1965), and J. Burlak (1971–1977), who were evidently interested in broadening access to Soviet material, since all three compiled Russian-English mathematical dictionaries and language guides (see Section 2.2.2). For more on the history of *Mathematical Reviews*, see Pitcher (1988, pp. 69–89).

As well as having library access to the relevant volumes of the appropriate journals, scientists in East and West were sometimes able to communicate their work to each other through the distribution of offprints. However, there were certain complications. To begin with, any exchange of offprints would be subject to the postal difficulties described in Section 2.1. The situation was particularly bad in this regard during Stalin's lifetime, though matters improved somewhat after his death: Boris M. Schein (2008) relates that V. V. Wagner, whom we will meet in Chapter 10, once received several years' worth of offprints from foreign mathematicians all in one package — under Stalin, these had been stored up and were finally passed on when Khrushchev came to power. However, an even more fundamental problem faced Soviet scientists when it came to sending out offprints: in many cases, they simply did not have any. Perhaps because of what seems to have been a chronic shortage of paper in the USSR, journals provided authors with very few offprints, if any at all (Medvedev, 1971, p. 124). Soviet authors were thus largely unable to distribute copies of their work even to their colleagues elsewhere in the USSR, let alone to their foreign counterparts, and sometimes found themselves criticised for a lack of cooperation in not sending out offprints (Medvedev, 1971, p. 125) — see the Schein quotation on page 13 of this book.

While many Western scientists, in the absence of any real scope for collaboration, were concerned solely with learning the specific details of Soviet work in their area so that they might build upon it, or at least avoid duplicating it, others were keen to gain a general overview of Soviet academic capabilities. At times, this would have been motivated purely by curiosity, but at others, it would presumably have owed at least a little to political considerations and a fear of 'the other side'. Thus, reports by Westerners of their trips to the USSR, such as those, for example, of Bockris (1958), Lohwater (1957), and Danckwerts (1983), would have made popular reading. Indeed, the same may be said about the reports of Soviet visitors to the West (Medvedev, 1971, pp. 118–119), though these would not always have had the same wide circulation as that enjoyed, for example, by the Soviet accounts of the Edinburgh ICM in 1958 (see p. 28).

Rather than settle for the occasional glimpse of Soviet science that was afforded by the reports of returning conference delegates, Western organisations began to commission detailed surveys of Soviet academic science, to stand alongside existing appraisals of Soviet technical capabilities. Many books and articles were therefore written on the organisation and planning of Soviet science; see, for example, Oster (1949), Turkevich (1956), Rabinowitch (1958), and White (1971).[35] Moreover, the American Association for the Advancement of Science organised a symposium devoted to Soviet science in December 1951.[36] Besides an appraisal of Soviet science in general (Zirkle, 1952), the symposium also dealt with certain specific areas, such as mathematics (Kline, 1952), and considered the issue of intellectual freedom in the USSR (Volin, 1952).[37] The state of Soviet mathematics was dealt with specifically in the book *Recent Soviet contributions to mathematics* (LaSalle and Lefschetz, 1962), which included subject-by-subject assessments, including, for example, one on Soviet algebra (Good, 1962).[38] The purpose of this book was made very clear in its introduction:

> it is not certain that our general scientific community quite realizes the intense scientific activity that prevails in the Soviet Union. It is hoped that this report will do its share toward clearing the fog. (LaSalle and Lefschetz, 1962, p. v)

One would assume that similar appraisals of Western science were produced in the USSR, but I have not found any: presumably, these would not have been for general consumption, which may explain why they do not seem to be available now.

Soviet education, particularly in the sciences, also received a great deal of attention from Western researchers. Among the materials published in connection with this subject, we have the exhaustive 856-page report *Education and professional employment in the USSR* of 1961, prepared by Nicholas de Witt and sponsored by the US National Science Foundation. This report outlined the Soviet educational system, from primary to postgraduate levels, with comments on the professional employment available to Soviet citizens with different educational backgrounds.[39]

A feature of some of these accounts of Soviet science that should be noted here is the fact that, in many cases, their authors did not merely want to inform the reader about advances in Soviet science, but to equip them with the necessary references to be able to find out about future developments for themselves. Thus, for example, the book *Recent Soviet contributions to mathematics* includes a guide to Soviet mathematical journals (Steeves, 1962). Regarding science more generally, two later sources with the purpose of helping the reader to come to grips with the Soviet scientific literature are Lieberman (1987) and Berry (1988, Chapter 12). The article Hoseh (1961) deals mostly with the Soviet chemistry literature, but many of the comments made therein may be applied more generally. All of these articles were intended to help Western scientists learn about developments in Soviet science, but they remain tremendously useful in the study of the history of Soviet science.[40]

2.2.2. Linguistic accessibility. In this subsection, I justify my earlier claim that while, broadly speaking, *physical* accessibility (together with its attendant issues, such as censorship) was the major problem facing Soviet mathematicians in their attempts to learn more about the work of 'the other side', it was *linguistic* accessibility that provided the major hurdle for Western mathematicians. Although I can offer no hard statistical evidence, I hope to make a convincing case by quoting various comments that are scattered throughout the literature.

Let us begin our discussion of languages by considering, as a representative of the Russian mathematical literature, the major journal *Matematicheskii sbornik*, which has already been mentioned several times in this chapter. Founded in 1866 as a forum for the Moscow Mathematical Society (Lyusternik 1946; Demidov 1996), the vast majority of papers published in *Matematicheskii sbornik* down the decades have been in Russian. Indeed, up to volume 30 (1916–1918) of the original series of the journal, no other languages appear to have been used. From volume 31 (1922–1924), however, things began to change, with *Matematicheskii sbornik* refounded as an international journal under the editorship of D. F. Egorov (Demidov, 2006, p. 793). In a post-revolutionary effort to make Soviet mathematics more accessible to a foreign audience, this volume of the journal featured twelve articles in French and one in English. In fact, this was just the beginning of a new trend: during the 1920s, the number of papers published in *Matematicheskii sbornik* in languages other than Russian increased dramatically. Some figures for the 1920s are provided in Table 2.3, where we see, for example, that non-Russian papers were in fact in

TABLE 2.3. Numbers of papers published in *Matematicheskii sbornik* in foreign languages during the 1920s (E = English, F = French, G = German, I = Italian).

Volume number	**Year(s)**	**Total number of papers**	**Number of papers in foreign languages**
31	1922–1924	50	1E+12F
32	1924–1925	60	1E+15F+6G
33	1926	26	2E+9F+6G
34	1927	13	3F+2G
35	1928	29	4F+8G
36	1929	33	1E+14F+6G+1I

the majority in both 1926 and 1929. Indeed, this trend continued throughout the 1930s, although it began to peter out during the 1940s. The last paper published in *Matematicheskii sbornik* in a foreign language (in this case, French) appears to have been in 1947. Thereafter, the journal became exclusively Russian. It should be noted that not all of the authors publishing in foreign languages were Russian: several foreign authors are represented in the statistics in Table 2.3. I will return to this point below.

Those papers that were published in *Matematicheskii sbornik* in foreign languages nevertheless carried a Russian summary at the end. What is more, the efforts to make the journal accessible to an international audience were also taken one step further: the papers published in Russian carried a French or German summary. However, the tradition of including these died away around the same time that *Matematicheskii sbornik* turned its back on foreign languages more generally, if not a little sooner.

Since, as I argued in Section 2.2.1, Western mathematicians had access to *Matematicheskii sbornik*, it would have been particularly useful for them to see papers in Western languages, or, failing that, French or German summaries of papers in Russian. Indeed, the editors of *Matematicheskii sbornik* appear to have been conscious of this: they took the trivial, but welcoming, step of including the French name *Recueil mathématique* on the journal's title page. Moreover, in the 1931 editorial piece that was quoted in Section 2.1 ('Soviet mathematicians, support your journal!' — see page 17), it was noted that

> Soviet mathematics can and should have a journal of international importance. Therefore, we continue to supply as normal foreign summaries of articles written in Russian, and to print articles in foreign languages. Experience has shown that mathematical articles written in Russian reach the foreign reader.[41]

In fact, *Matematicheskii sbornik* was not the only Soviet journal to adopt this policy for a time. For example, the Kharkov-based journal *Soobshcheniya Kharkovskogo matematicheskogo obshchestva* (mentioned in the preceding subsection) published papers in both French and German, as well as in Russian and Ukrainian. Indeed, the entirety of volumes VI–IX (1933–1934) were published in German in the hope that this would boost foreign readership (Marchevskii, 1956a). Those papers that appeared in French or German carried a summary in either Russian or Ukrainian, while those published in Russian or Ukrainian featured a French or German summary. However, in this case also, the practice of using foreign languages faded away in the second half of the 1930s, as did that of employing Ukrainian: by the 1940s, the *Soobshcheniya* was publishing papers almost exclusively in Russian.

A brief point should be made here about the other languages of the Soviet Union. Although Russian was the all-pervading language of the USSR, there were periods when the use of other national languages was encouraged, perhaps to create a semblance of independence. Thus, we have, for example, the use of Ukrainian in the *Soobshcheniya* during the 1930s. The policy of encouraging 'local' publication (see Section 2.2.1) often extended also to the use of national languages. Thus, when studying the Soviet mathematical literature, one often encounters Ukrainian, Armenian, Estonian, Georgian, and many other languages. However, in all cases, a paper in a national language was accompanied by a summary in Russian. Nevertheless, the policy of encouraging, or merely permitting, the use of national languages was neither constant nor consistent, and 'local' journals, such as the *Soobshcheniya*, often reverted to the strict use of Russian only. The linguistic policies of those nations that were not (always) under the direct control of Moscow also varied, though they too were conscious of reaching international audiences with their publications. The *Czechoslovak Mathematical Journal*, for example, was produced in two different versions: one in Russian, the other in English, French, and German. Moreover, these two were published in parallel with a closely related journal in Czech and Slovak. For further comments on mathematical publishing in Czechoslovakia, see Section 8.2. Elsewhere in communist Central and Eastern Europe, national languages were widely used in mathematical publications, but, in Hungary, for instance, where the use of German was traditionally widespread, we find works appearing in both Hungarian and German versions — see, for example, Steinfeld (1953).

To begin to justify the claim made at the beginning of this subsection, let us consider the matter of foreign publication. In Section 2.1, I had a great deal to say about the issue of Soviet scientists generally, and mathematicians in particular, publishing their work abroad. However, I have so far skirted around the issue of *Western* mathematicians publishing in *Soviet* journals. On the whole, this was rather rare, though not unknown. Let us reconsider, for example, the contents of Table 2.3. As already noted, not all of the papers published in foreign languages in *Matematicheskii sbornik* were by Russian authors — a number of them were written by foreigners, as Table 2.4, an addendum to Table 2.3, shows. Although the figures are not huge, they demonstrate a foreign participation in *Matematicheskii sbornik*, though none of the foreign authors contributed papers in Russian.[42] Such involvement continued at similar levels throughout the 1930s, though it began to fall off in the 1940s. One or two foreign papers appeared during the years of the Second World War, but, with the end of the journal's use of foreign languages in the late 1940s, international participation appears to have ceased.

There do not appear to have been any particular barriers to Western authors submitting their papers to Soviet journals, although one can easily imagine Soviet editors being wary of accepting such contributions, for fear of the criticism that it might bring from higher levels. On the other hand, the continued acceptance of foreign contributions even during the years of the Great Terror in the 1930s suggests that there was, in reality, no problem here as far as the Soviet authorities were concerned: the fact that Western scientists were sending their work to the

TABLE 2.4. Numbers of foreign authors in *Matematicheskii sbornik* during the 1920s, based on stated affiliations (A = American, C = Czechoslovak, F = French, G = German, I = Italian, P = Polish).

Volume number	**Year(s)**	**Number of papers in foreign languages**	**Number of foreign authors**
31	1922–1924	13	0
32	1924–1925	22	2F
33	1926	17	1A+1G+1I
34	1927	5	1F
35	1928	12	1G+1P
36	1929	22	1C+1F+2G+1P

USSR for publication at this time may indeed have been regarded as a recognition and legitimisation of the international standing of such journals as *Matematicheskii sbornik*. With regard to later decades and the decline in foreign contributions to Soviet journals, it might be argued that Western mathematicians simply had easier access to their own journals of international repute,[43] but of course this had always been the case. I contend, therefore, that the major reason that Western mathematicians in general stopped submitting their work to Soviet journals was simply the fact that they could not write Russian sufficiently well.

Throughout the twentieth century, the bulk of (Western) mathematical research was published in German, French, and English, with a possible bias towards German at the beginning of the century having transmuted into a preference for English by the end. Thus, the mathematicians of the twentieth century (at least those of Western Europe and North America) would typically be able to read the mathematical literature that was relevant to them, provided they had a working knowledge of these three languages. It might occasionally be necessary to supplement these with a crash course in some other language, but, in all likelihood, this would be another Western European language such as Spanish or Italian. Indeed, a glance through the bibliography of this book will reveal that the majority of those sources that are not in Eastern European languages are in German, French, or English, with a very small number of exceptions being in Spanish, Italian, and Japanese, among others. On the whole, therefore, it has been reasonable, throughout the twentieth century, to assume that a Western mathematician has at least a basic knowledge of English, French, and German, hence the use of these languages in Soviet journals such as *Matematicheskii sbornik*. Indeed, this probably would not have alienated too many Russian readers either, since the principle that a twentieth-century mathematician should have some knowledge of these three languages seems to have applied also to mathematicians in the Soviet Union. Western academic visitors to the USSR often noted their hosts' facility with Western languages (see, for example, Bockris 1958) — English, in particular, was widely taught in Russian schools.[44] In later chapters, when we come to consider the extent to which Soviet semigroup theorists read the work of their Western counterparts, we will not encounter any suggestions of linguistic difficulties.

On the other side, things were rather different: knowledge of Russian was not widespread among Western mathematicians.[45] There were of course exceptions, such as the British mathematician F. V. Atkinson, who published some work in Russian (Atkinson 1951; see Mingarelli 2005). However, the following comment by the semigroup theorist Gordon Preston (1991, p. 25) is fairly typical of the experiences of many Western mathematicians in their attempts to come to grips with the work of their Soviet counterparts:

> I had to face the fact that I could not read Russian. I tackled the paper armed with a dictionary and no knowledge of either Russian or the Russian alphabet. I remember that it took me three hours to translate the first sentence . . . .

See also the comment by Douglas Munn that was quoted in the introduction to this chapter (p. 12). As we will see in Chapter 10, Preston, for one, was helped out in his efforts to learn more of Soviet work by a survey article written by a French mathematician, Jacques Riguet. In the years before the systematic translation of Soviet sources began (see below), Riguet seems to have been one of the few Western researchers who was able to read Russian work and who tried, in a small way, to bring it to a wider audience; in the mid-1950s, he produced two survey articles (the texts of lectures delivered in Paul Dubreil's Paris algebra seminar: see Section 12.2) on Soviet semigroup-theoretic work: 'Travaux récents de Malčev, Vagner, Liapin' (Riguet, 1953) and 'Travaux soviétiques récents sur la théorie des demi-groupes: la représentation des demi-groupes ordonnés' (Riguet, 1956).

Efforts were made by many scientists in the West to gain a greater understanding of the Russian language in order to be able to read the work of Soviet colleagues (Lefschetz, 1949). This was particularly so in the United States, following the shock of the launch of Sputnik. As Gessen (2011, p. 7) has commented,

> there is a generation of American mathematicians who are more likely than not to possess a reading knowledge of mathematical Russian.

Nevertheless, the general understanding of Russian in the West seems to have remained quite low. In the following harangue, aimed at his fellow semigroup theorists, Boris M. Schein suggested that cultural misunderstanding and a disinclination to learn the language of 'the other side' may have played a part in this:

> Few people realize that by the mid-sixties every third paper in the algebraic theory of semigroups was in Russian. Some of them are very good. For an English speaker, Russian is not substantially more difficult than, say, German. Are you scared by the Cyrillic alphabet? Like the Roman one, it is derived from Greek, and you already know a half of it. Therefore, linguistic reasons do not fully explain the existing attitude. Born in the XX-th century, we are too close to it to see that it might be the worst century in human history—so far, at least. Powerful forces tried to split our planet, and for many readers Russian papers were—consciously or not—in the language of another planet, not merely another country. (Schein, 2002, p. 156)

These difficulties were acknowledged also by Jacob Chaitkin (1945, p. 301) at the beginning of a short article whose purpose appears to have been to encourage more American scientists to learn some basic Russian:

> The only cloak of mystery that envelops Soviet science is that of the Russian language. Is this language a true barrier, or is it merely a psychological obstacle that we ourselves have conjured up?

Chaitkin went on to note that

> [t]he Russians do not permit language to form a similar obstacle in their study of our scientific work. English is taught widely in the schools of the U.S.S.R. and is treated as second in importance only to the native languages of that country. There is no psychological barrier in the Soviets' attitude toward the study of English. (Chaitkin, 1945, p. 301)

Nevertheless, even in the absence of a widespread ability to read Russian, Western mathematicians were still able to glean some information on Soviet work thanks to the efforts of the contributors to the various abstracting journals (see Section 2.2.1). Moreover, if a Westerner did attempt to read a Russian paper, he/she may well have been helped out by the fact that much of the notation would have been familiar: Soviet mathematicians certainly used all the same symbols as those employed in the West, including the Latin and Gothic alphabets (perhaps influenced by a Germanic mathematical tradition?). Like that of other European languages, much Russian mathematical terminology is Greek- and Latin-based. Further, the Soviet Union's predilection for using Russian alongside, or in place of, the various national languages meant that a Westerner attempting to come to grips with Soviet publications would only have one language to contend with. Indeed, the following comments made by Medvedev suggest that the dominance of Russian also aided scientific communication *within* the USSR:

> The proceedings of republican academies ... started to be published in the local languages .... It was not unusual for the research institute in Estonia to receive an official letter written in the Uzbek language from the Uzbekistan Soviet Republic, and it could take months before anyone could be found who was able to read it. In retaliation, the reply would be written in Estonian, and this would create the same situation in Uzbekistan. ... After a few years of frustration, the Russian language again was made the official language for internal communication. (Medvedev, 1979, p. 128)

Further assistance was rendered to the Western mathematician attempting to read Russian work by the publication of several Russian-English mathematical dictionaries. The possibility of producing such a resource had been raised as early as 1933 by E. T. Bell, who lamented the fact that

> [m]uch Russian work in the theory of numbers, for example, is practically buried for years to most readers, and little of it ever receives adequate translation or review. (Bell, 1933b)

However, he observed that, when trying to discuss mathematics across language barriers, a

> remarkably small number of common, non-technical words [is] sufficient to lubricate the technical terms and to present a mathematical argument intelligibly. (Bell, 1933b)

He suggested therefore that, for any given language, perhaps only 300 non-technical words (conjunctions, prepositions, some common verbs, etc.) are required. In most instances, technical terminology needs no translation, since much of it is Greek- or Latin-based and therefore common to several languages, modulo the 'local variations' that give us, for example, the terms 'group', 'Gruppe', 'groupe', 'grupo', 'группа', etc., for what is essentially the same word. Bell went on to comment that

> [i]f the guess of 300 is anywhere near the truth, it would be worth someone's time to compile such common vocabularies for the languages in which mathematics is alive. (Bell, 1933b)

Such dictionaries would, in Bell's view, "find a publisher without difficulty".

Although Bell's suggestion does not appear to have been taken up immediately, there are now very many bilingual mathematical dictionaries available, including, for example, German-English (Macintyre and Witte, 1956), Romanian-English (Gould and Obreanu, 1967) and Japanese-English (Sūgakkai, 1968). Perhaps because of the problems outlined in this section, Russian-English mathematical (or, more generally, scientific) dictionaries are particularly common. We have, for example, Lohwater (1961) and Burlak and Brooke (1963), and also Borovkov (1994), which deals specifically with probability, statistics, and combinatorics. Rather than merely being dictionaries, the books Gould (1972) and Croxton (1984) attempt to provide an elementary course in Russian for mathematicians, while Gould (1966) is a manual for would-be translators of Russian mathematical literature. Russian is also represented in the multilingual mathematical dictionary Eisenreich and Sube (1982). Moreover, the production of such resources was not merely a Western concern: Soviet readers were provided with, for instance, Russian-Ukrainian (Shtokalo, 1960) and English-Russian-Armenian-German-French (Tonian, 1965) mathematical dictionaries.[46] See also the comments in DuS. (1956). As well as translating those technical terms that differ from language to language, these dictionaries typically include also some of the basic elements of the relevant language(s): conjunctions, conjugations of common verbs, and so on, together with some brief notes on grammar, thereby providing an elementary guide to reading mathematics in a foreign language.

However, although Soviet mathematicians seem, by and large, to have been able to read the work of their Western counterparts (with or without a dictionary), the same was not true in the opposite direction. In spite of the fact that they were often physically accessible, Soviet mathematical papers went largely unread in the West. Recognising this as an unsatisfactory state of affairs, Western mathematical societies came up with a solution: the systematic translation of major Soviet mathematical journals.

In the earlier decades of the twentieth century, there seems to have been a minor tradition of translating certain mathematical works in order to make them accessible to a larger audience. However, this appears to have applied, on the whole, only to works written in languages with relatively small readerships, as compared with German or French, say. Thus, for example, the 1903 book *Einleitung in die allgemeine Theorie der algebraischen Gröszen* by Julius König, which will be featured in Section 4.5, was a German translation of an earlier Hungarian dissertation. Several decades later, the Slovak mathematician Štefan Schwarz made a point of repeating the results of his 1943 Slovak work 'Teória pologrúp' in subsequent papers in both English and Russian, in order to bring them to a wider audience (see Section 8.2). However, there does not appear to have been any widespread tradition of translation from Russian or from English.

The situation changed in the 1940s[47] and the following decades with the launch of several systematic translations of Soviet journals. The American Mathematical Society appears to have taken the lead here, with the financial assistance of the US National Science Foundation (NSF), which also funded the translation of Russian literature from other scientific disciplines (Anon, 1956): as of 1958, the NSF was involved in the translation of 53 Soviet scientific journals (Anon, 1958c). The mathematical works produced included cover-to-cover translations (see below) and also translations of selected articles from the Soviet literature in a series entitled simply 'American Mathematical Society Translations', whose publication was sponsored by the US Office of Naval Research.[48] From 1948 to 1954, a total of 105 separate pamphlets was produced; these were subsequently grouped by topic and published in eleven volumes to form the first series of 'American Mathematical Society Translations'. A second series followed, incorporating selected Japanese articles also, and this continues to this day. Like the first series, the second publishes selected articles, grouped by topic. Volume 139 (1988) is of particular relevance for the present book, since it is entitled *Nineteen papers on algebraic semigroups* and contains translations of Russian papers going back to the 1960s.

In contrast to the highly selective nature of the 'American Mathematical Society Translations', systematic translations of specific (high-profile) Soviet journals were also launched in the 1960s by both the London and American Mathematical Societies.[49] These translations seem to have been particularly successful, for many of them remain in publication today, although some have changed publishers over the years. Details of a selection of on-going translations may be found in Table 2.5, though this certainly does not represent an exhaustive list.[50] These translations appear always to have been direct and have even included ideological articles (see, for example, Gnedenko 1970) and other pieces, such as a tribute to Lenin (Anon, 1970b), which, it might reasonably be argued, have little place in a mathematical journal. On rare occasions, the translations have also preserved some rather unsavoury features of the Russian originals, such as L. S. Pontryagin's anti-Semitic comments regarding Nathan Jacobson (Pontryagin, 1978, English trans., p. 23).[51] For a report on some of the AMS's early translation activities, see Anon (1960). See O'Dette (1957) for a more general account of translation activities in the USA, including a strong justification of the process.

Western mathematicians were helped out not only by the routine translation of journal articles, but also by that of Russian monographs. Once again, the AMS took the lead in this respect with the launch in the early 1960s of their 'Translations of Mathematical Monographs' series, which remains ongoing; so far, over 200 translations from Russian and Japanese have been produced. The third volume in the series is one that is cited throughout the present book: the world's first monograph on the algebraic theory of semigroups, E. S. Lyapin's *Полугруппы* of 1960, which was published in English as *Semigroups* in 1963. Indeed, the English translation of this book, as published by the AMS, ran to three editions: two more than the original Russian. I will say more about this monograph in Section 12.1.2. It should be noted that the LMS and AMS did not hold the monopoly on the translation of Russian mathematical works. The English translation of A. G. Kurosh's

Table 2.5. (Post-)Soviet mathematical journals of which translations are routinely published in the West (AMS = American Mathematical Society, LMS = London Mathematical Society, RAS = Russian Academy of Sciences, T = Turpion Ltd.).

Russian/Ukrainian title	**English title**	**Current publisher**	**Translated since**
Успехи математических наук	*Russian Mathematical Surveys*	LMS/T/RAS	1960
Доклады Российской Академии наук, formerly *Доклады Академии наук СССР*	*Doklady Mathematics*; formerly *Soviet Mathematics: Doklady*	Springer	1960
Сибирский математический журнал	*Siberian Mathematical Journal*	Springer	1966
Известия Российской Академии наук, серия математическая; formerly *Известия Академии наук СССР, серия математическая*	*Izvestiya: Mathematics*; formerly *Mathematics of the USSR: Izvestiya*	LMS/T/RAS	1967
Математический сборник	*Sbornik: Mathematics*; formerly *Mathematics of the USSR: Sbornik*	LMS/T/RAS	1967
Украинский математический журнал	*Ukrainian Mathematical Journal*	Springer	1967
Алгебра и логика	*Algebra and Logic*	Springer	1968
Теорія ймовірностей та математична статистика	*Theory of Probability and Mathematical Statistics*	AMS	1974
Труды Московского математического общества	*Transactions of the Moscow Mathematical Society*	LMS/AMS	1984
Алгебра и анализ	*St. Petersburg Mathematical Journal*, formerly *Leningrad Mathematical Journal*	AMS	1990

Lectures on general algebra (*Лекции по общей алгебре*), for example, was produced by the New York-based publisher Chelsea (Kurosh, 1960). Indeed, individuals also provided translations of Russian texts for circulation among their close colleagues — for example, a small number of people at Tulane University in New Orleans appear to have had access to a translation of an early text that will be of interest to us in later chapters: A. K. Sushkevich's *Theory of generalised groups* (see in particular Section 12.1.1).

Western mathematical works were also translated into Russian, although this appears to have been done on a rather more *ad hoc* basis. In particular, perhaps in light of the fact, argued above, that the linguistic problems of Soviet mathematicians were less pronounced than those of their Western counterparts, there were no systematic translations of Western journals, merely the occasional translation of articles deemed important (Reid, 1977, p. 484). This may also have been because the scale of censorship required, in order to check that the Western papers did not contain anything 'subversive', would simply have been too great. Either way, the only Western publications that were translated into Russian were longer works, such as monographs. Nevertheless, a comprehensive cross section of the Western mathematical literature appears to have been translated into Russian (see O'Dette 1957, pp. 579–580). The drive to do this probably stemmed from two closely connected sources: the simple desire of Soviet mathematicians to learn about the work of their Western counterparts, in conjunction with the Soviet government's burning need to catch up with, and to surpass, the West in all areas — or, arguably in the case of mathematics, to maintain the Soviet lead.

Although there had been Russian translations of Western mathematical texts in earlier decades (for example, a Russian edition of Galois's works: Chebotarev 1936), such translations began to appear more frequently from the late 1940s onwards. Many prominent Western texts were translated into Russian during these decades, in some cases only a few years after the originals appeared. As an illustration, Table 2.6 gives the details of a selection of well-known Western algebra texts and their Russian translations. Note the repeated appearance of the Moscow-based publisher I–L, whose full name was Izdatelstvo Inostrannoi Literatury (Издательство Иностранной Литературы = Publisher of Foreign Literature). As the name suggests, this was a publishing house devoted exclusively to the production of Russian translations of (carefully selected) foreign materials. Similarly, the publisher Mir (Мир = World/Peace) was set up in the years after the Second World War for the same purpose (Medvedev, 1979, p. 63).

As in the case of translations into English, these and other translations into Russian appear to have been direct, with no modification of the text, aside, perhaps, from the insertion of references to Soviet work in the relevant area. Even books whose subject matter was ideologically dubious from the Soviet point of view were translated directly (Vucinich, 2002, p. 22). However, in such cases, a foreword would often be added by the editors of the translation, or by an interested ideologue, in which the 'shortcomings' of the text, or the 'idealistic' views of the 'decadent' Western authors, were highlighted and condemned. Similar such comments might also be added in footnotes throughout the text. A particularly good example of such criticism in the foreword to a translation may be found in the 1949 Russian edition of Veblen and Whitehead's *The foundations of differential geometry*, where the

Table 2.6. Details of some well-known Western algebra texts and their Russian translations.

Author(s) and title	**Original publication**	**Russian translation**
M. F. Atiyah and I. G. Macdonald, *Introduction to commutative algebra*	Addison-Wesley, Oxford, 1969	Mir, Moscow, 1972
Garrett Birkhoff, *Lattice theory*	AMS, New York, 1948	I–L, Moscow, 1952
Nicholas Bourbaki, *Éléments de mathématiques, algèbre*	Hermann, Paris, 1942	GIFML, Moscow, 1962
Claude Chevalley, *Theory of Lie groups*, I	Princeton UP, 1946	I–L, Moscow 1948
P. M. Cohn, *Universal algebra*	Harper & Row, New York, 1965	Mir, Moscow, 1968
Marshall Hall, Jr., *The theory of groups*	Macmillan, New York, 1956	I–L, Moscow, 1962
Nathan Jacobson, *Structure of rings*	AMS, Providence, RI, 1956	I–L, Moscow, 1961
Serge Lang, *Algebra*	Addison-Wesley, Reading, MA, 1965	Mir, Moscow, 1968
Hermann Weyl, *The classical groups*	Princeton UP, 1939	I–L, Moscow, 1947

editors took exception to the very abstract approach to geometry that the authors propounded. We will examine this condemnation in more detail in Section 10.2.

In addition to those algebra texts listed in Table 2.6, a Russian translation was also produced of what was, for many years, one of the principal Western monographs on semigroups: Clifford and Preston's *The algebraic theory of semigroups*. The original two volumes of this book appeared in 1961 and 1967, and both were published in Russian translation in 1972 (see Section 12.1.3). Although Western semigroup theorists appear to have had access to a range of relevant sources, both books and papers, that had been translated from Russian, *The algebraic theory of semigroups* seems to have been the only Western semigroup-theoretic source that was translated into Russian. Its influence around the world will be considered in the final chapter.