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Digital Financial Participation and Consumer Financial Detriment in the Fintech Era: The Role of Online Financial Contracting

Erzsébet Németh [1], Szonja Jenei [2,*], Szilvia Módosné Szalai [2] and László Pataki [3]

[1] Marketing and Communication Institute, Budapest Metropolitan University, Nagy Lajos király útja 1–9, HU-1148 Budapest, Hungary; enemeth@metropolitan.hu
[2] Kautz Gyula Faculty of Business and Economics, Széchenyi István University, 9026 Győr, Hungary; modosne.szalai.szilvia.valeria@sze.hu
[3] Doctoral School of Management and Business Administration, John von Neumann University, Infopark sétány 1, H-1117 Budapest, Hungary; pataki.laszlo@nje.hu
* Correspondence: jenei.szonja@sze.hu

Abstract

Evidence linking fully online financial contracting to consumer financial detriment remains limited. This study examines changes in financial detriment in Hungary between 2018 and 2022 and associations with digital financial participation. We compare the 2018 (N = 1001) and 2022 (N = 1000) OECD/INFE surveys and estimate weighted logistic regressions for 639 internet users reporting income in 2022. Fully online contracting was associated with higher odds of provider disputes and refusals (OR = 3.18), remaining significant across binary specifications and under an assumed design effect of 2.0. Its association with fraud exposure and incidents (OR = 2.94) rested largely on disputed unrecognized transactions. Digital financial activity was associated with provider disputes and refusals, mainly through complaints. Because 2022 cluster identifiers were unavailable, standard errors were inflated using assumed design effects. At a design effect of 2.0, the contracting–fraud and activity–dispute associations were no longer significant after Holm correction. Overall detriment did not change significantly, and findings for financial knowledge were inconclusive. Distinguishing contract conclusion from activity frequency extends consumer-level evidence beyond fintech adoption research. The findings support monitoring complaints, refusals, and disputed transactions related to online contracting, without implying causality.

Keywords: remote customer onboarding; online financial contracting; digital financial activity; consumer financial detriment; financial consumer protection; fintech services

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1. Introduction

Between 2018 and 2022, Hungarian households faced several major economic challenges. The COVID-19 pandemic was followed by rising inflation and an energy crisis. Together, these developments placed pressure on household finances during a period of accelerating financial digitalization [1,2]. This period offers an opportunity to describe changes in the prevalence of financial detriment and examine its associations with different forms of digital financial participation.

Digitalization has changed how consumers access and use financial services. Checking account balances, making payments and transfers, and managing financial products

increasingly take place through mobile applications and online platforms [3,4]. Consumers can also establish new financial relationships remotely. For example, they can open bank accounts, apply for payment cards or loans, and purchase insurance without visiting a branch. Such processes rely on remote identity verification. Remote customer onboarding is an important fintech development and is subject to European regulatory guidelines [5,6]. Beyond conventional online banking, this study considers fintech services in a narrower sense: mobile payments, account aggregation, online securities trading, and automated online advice. These services may expand the range of financial options available to households. However, digital channels can also expose consumers to perpetrators operating outside the regulated financial system [7].

Digitalization may also have influenced the prevalence of financial detriment. As more financial activities moved online, new risks such as online scams emerged [8], while consumer protection requirements imposed on financial service providers became more stringent. These developments may have had opposing effects on financial detriment: stricter regulation may have reduced problems involving financial service providers, whereas the expansion of digital services may have increased exposure to fraud [9]. Consequently, it was uncertain whether the overall prevalence of financial detriment had increased or decreased, or whether the main changes concerned the types of detriment experienced. The available data do not permit direct tests of these competing explanations, so the comparison of the two survey waves is descriptive.

Hungary offers a relevant setting for examining consumer risks. In international comparisons, a relatively high proportion of Hungarian respondents achieve the minimum target score for financial knowledge. Yet the country's overall financial literacy score is low, particularly for financial behavior [10]. This contrast raises an important question: does financial knowledge protect consumers from detriment associated with digital channels, or are these outcomes more closely related to how consumers use those channels?

This study has two objectives. First, it examines how the prevalence and composition of financial detriment changed between 2018 and 2022 (RQ1). Second, it investigates the associations of fully online financial contracting, digital financial activity, and financial knowledge with the occurrence of fraud exposure and incidents and provider disputes and refusals (RQ2).

This study builds on our earlier analyses of the same survey data, which examined financial knowledge and self-assessment in relation to digital financial behavior [11], demographic patterns in managing financial problems [12], and the sociodemographic correlates of financial prudence, risk-taking, and satisfaction [13]. None of these studies analyzed financial detriment, distinguished fully online financial contracting from the frequency of digital financial activity, or compared the 2018 and 2022 waves; all analyses of financial detriment and of fully online financial contracting reported here are therefore new. The study makes three contributions to the existing literature. First, it examines digital financial activity separately from fully online financial contracting. Previous studies have primarily measured exposure to fraud by the frequency of online activities [7,9,14], whereas research on remote customer onboarding has focused mainly on service providers' and regulators' perspectives, particularly on the prevention of money laundering and identity fraud [5,6]. These studies have not specifically examined whether the mode of financial contracting is associated with consumer-reported financial detriment. Second, in exploratory analyses, this study distinguishes between basic digital financial activities and fintech services in the narrower sense, allowing their respective associations with financial detriment to be examined. Third, it investigates consumers' experiences of fraud and disputes with financial service providers, in contrast to most research on fintech adoption, which focuses primarily on attitudes and behavioral intentions [15,16]. The findings add evidence from Hungary

to the literature based largely on research conducted in Western Europe, North America, and Asia.

2. Literature Review

The theoretical framework integrates two complementary perspectives. Routine activity theory serves as a sensitizing framework for interpreting fraud exposure and incidents. It emphasizes exposure to potential offenders. However, the available data do not reliably measure capable guardianship, a key element of the theory (Supplementary File S3). We therefore cannot test the theory in full. Provider disputes and refusals do not necessarily involve a motivated offender. To interpret these outcomes, we draw on the integrative consumer vulnerability framework [17]. Research on remote customer onboarding and trust in fintech services also helps explain the risks associated with how financial contracts are concluded.

2.1. Routine Activity Theory, Consumer Vulnerability, and Exposure

Routine activity theory suggests that crime becomes more likely when a motivated offender encounters a suitable target without capable guardianship [18]. Leukfeldt and Yar applied this approach to digital settings. They argued that everyday online activities, like their offline counterparts, can increase exposure to potential offenders [7].

In digital settings, protection depends on security tools and cautious online behavior, not only on financial knowledge. Examples include antivirus software, other security applications, and safe browsing practices [19].

Previous research suggests that exposure to fraud attempts and actual victimization may be associated with different factors. In a study of online health scams among older adults in China, Chen et al. found that more frequent online activity and risky user behavior were primarily associated with greater exposure, whereas a lack of digital security skills was more closely related to actual victimization [8]. Other studies suggest that technical proficiency alone does not necessarily protect individuals against sophisticated deception techniques [20]. A study conducted in China found that higher financial literacy was associated with both greater exposure to fraud attempts and a higher likelihood of victimization [21]. In an experimental study, the ability to recognize phishing messages was more strongly associated with impulsivity and rapid, unreflective responses than with knowledge levels [22]. In contrast, studies conducted in Italy and the United States found that individuals with higher levels of objective financial literacy were less likely to become victims of online financial fraud [23,24]. Taken together, the literature provides mixed evidence as to whether greater financial knowledge protects consumers against fraud exposure and incidents. A bibliometric review focusing on the European Union similarly concluded that research examining the relationship between digital financial literacy and online financial fraud remains fragmented [25].

Problems involving financial service providers do not necessarily involve a motivated offender, and routine activity theory is therefore not directly applicable to such cases. The integrative consumer vulnerability framework provides a more suitable perspective for interpreting these outcomes; this framework treats vulnerability as a state shaped by consumers' personal circumstances and by their interactions with financial services [17]. The framework builds on the conceptualization of consumer vulnerability as a state of powerlessness arising from an imbalance in marketplace interactions [26] and on integrative reviews that link vulnerability to restricted access to and control over resources [27]. Financial regulators take a similar view: the UK Financial Conduct Authority describes vulnerable customers as those who, because of their personal circumstances, are especially susceptible to harm, particularly when a firm does not act with appropriate care [28]. In this frame-

work, low financial capability, material hardship, financial shocks, and greater exposure to financial services are conditions under which consumers are more likely to report mistreatment by providers [17]. The mode of contracting may be understood as an aspect of these interactions: without in-person contact, consumers have fewer opportunities to clarify terms before committing to a contract. A nationally representative US survey found that unfair treatment by financial service providers and unauthorized access to financial accounts were more common among consumers using a wider range of financial services and those with lower levels of financial capability [17]. Financial knowledge may therefore offer protection, particularly against provider disputes and refusals, by helping consumers understand contractual terms, costs, and their rights.

Routine activity theory may also help interpret changes between 2018 and 2022. However, the available data cannot test the mechanisms behind these changes. As noted in the Introduction, stricter consumer protection rules and the expansion of digital financial services may have affected the types of detriment reported, even if overall prevalence remained stable. A study in Hong Kong found that fraud cases during the pandemic exceeded predicted levels. The increase was associated with more time spent at home [9]. This finding suggests that changes in everyday activities may be associated with fraud exposure, alongside individual characteristics.

2.2. Remote Customer Onboarding and Trust in Fintech Services

How consumers conclude financial contracts may matter independently of their exposure to digital services. Remote customer onboarding allows identity checks, personal data submission, and contract signing to take place entirely online. These technologies replace face-to-face verification without eliminating the risks of financial crime [5]. The European Banking Authority requires financial institutions to assess remote onboarding systems before introducing them and throughout their use [6]. Regulation focuses mainly on money laundering and identity fraud. Evidence remains limited regarding associations between fully online contracting and problems reported by consumers. Research on remote onboarding has primarily considered the perspectives of providers and regulators rather than those of consumers [5,6]. Process-oriented studies of digital onboarding report customer-facing problems such as low satisfaction and limited access to products [29], identity fraud in which offenders open accounts using other people's personal data [30], and biometric verification errors that may wrongly reject legitimate applicants and push them back to branch services [31]. These studies, however, draw on institutional process data or conceptual analyses rather than on problems reported by consumers.

Routine activity theory and the consumer vulnerability framework suggest two possible links between fully online contracting and financial detriment. First, online contracting takes place through digital channels that fraudsters operating outside the regulated financial system can also use to reach consumers. It may therefore represent a distinct form of exposure. Second, without face-to-face contact, consumers may have fewer opportunities to clarify contract terms. This could contribute to later fee disputes, complaints, or rejected claims. EU rules address this concern. Directive (EU) 2023/2673, applicable from June 2026, requires clear explanations before distance financial contracts are concluded. It also gives consumers the right to request human intervention when using online tools [32]. Studies of fintech and marketplace lending have found that their borrowers default more often and accumulate more debt than comparable borrowers of traditional banks [33,34]. However, those studies compare providers, not contracting channels. Given these distinct potential mechanisms, we examine fraud exposure and incidents separately from provider disputes and refusals.

Research on fintech adoption often uses technology acceptance models, frequently extended with trust and perceived risk. A systematic review found that trust is linked to the intention to use fintech, both directly and indirectly. It also identified security as an element of perceived risk [16]. Another study found that perceived risk reduces trust, which in turn influences the intention to use fintech services [35]. Costs, limited digital skills, and low trust can discourage adoption, especially among older adults [15,36]. Yet the meaning, measurement, and consequences of trust remain insufficiently understood. A review identified the effects of fraud and scandals on trust as an important topic for further research [37].

Most research on trust in fintech examines intentions to use services rather than consumers' reported financial problems. The relationship between trust and financial detriment may be bidirectional. Greater trust may encourage use and increase exposure to risk, while fraud experiences may undermine trust. The cross-sectional data do not allow the direction of this relationship to be established. Accordingly, we examine the role of trust on an exploratory basis, without a predefined hypothesis.

2.3. Research Questions

Based on the theoretical framework presented above, this study addresses two research questions:

RQ1. How did the prevalence and composition of financial detriment change in Hungary between 2018 and 2022?

RQ2. How are fully online financial contracting, digital financial activity, and financial knowledge associated with the occurrence of fraud exposure and incidents and provider disputes and refusals?

The first research question is exploratory, while the hypotheses related to the second research question are formulated in Section 3.3.

3. Methodology

3.1. Data, Sample, and Measures

We draw on data from the OECD/INFE financial literacy surveys conducted in Hungary in 2018 and 2022 [38,39]. The Pénziránytű Foundation carried out both surveys with support from the Central Bank of Hungary. Both followed the OECD methodology and core questionnaire and used computer-assisted personal interviews (CAPI). In 2018, a two-stage sample covered 107 municipalities and Budapest districts, with selection proportional to population size and quotas for gender and age. Weights accounted for county, municipality size, gender, age, and education [40]. The 2022 survey took place from July 14 to August 6. It used a two-stage design covering 81 municipalities, again with gender and age quotas. Weights reflected gender, age group, region, settlement type, and education [41]. The 2022 results also appeared in the OECD's international report [10].

The 2018 and 2022 surveys included 1001 and 1000 respondents aged 18–79 years, respectively, yielding a combined sample of 2001. Post-stratification weights are available for both datasets. We used weighted data for comparisons between survey waves and for the 2022 regression models (Section 3.2). Sample characteristics and descriptive statistics are unweighted. No statistically significant differences were observed between the samples in gender or age-group composition. However, their income distributions differed, as did the unweighted shares with tertiary education (12.3% in 2018 and 17.7% in 2022; Table A1). After weighting, these shares were almost identical (18.8% and 18.9%, respectively).

The financial detriment items were largely comparable across the two survey waves. The main wording change concerns the item on unrecognized transactions. The 2018 Hungarian questionnaire asked whether respondents had noticed a transaction on their

statement that they did not recognize, whereas the 2022 questionnaire asked whether they had disputed such a transaction. Because the 2022 wording is narrower, it would be expected to reduce, rather than increase, the reported prevalence. Therefore, the increase observed between the two waves (from 2.0% to 3.8%; Table A3) is unlikely to be attributable to translation differences. Supplementary File S3 describes the remaining differences in wording, translation, and the coding of non-substantive responses.

The nine-item questionnaire section on digital financial activities was included only in the 2022 survey. The second item in this section of the OECD/INFE core questionnaire (QP9_2), concerning online top-ups of prepaid cards, was not included in the Hungarian questionnaire. The digital financial activity index was calculated as the mean of nine items measured on a four-point scale (1–4), requiring at least seven valid responses (N = 824). Two subindices were constructed: one comprising five basic digital financial activities (checking account balances, paying bills online, shopping online, transferring money, and managing financial products) and the other comprising four fintech services (mobile payments, account aggregation, online securities trading, and automated online advisory services). Both subindices showed acceptable internal consistency (Cronbach's $\alpha = 0.860$ and 0.767, respectively), with a correlation of $r = 0.47$. Among respondents with valid data, 44.1% reported using at least one fintech service sometimes or more frequently. The reported frequency of use of online securities trading platforms (18.6%) and automated online advisory platforms (17.7%; Table 1) appears high for Hungary. Only 7.8% and 7.6% of these users, respectively, reported holding shares, bonds, or an investment account, while 3.1% of all respondents held any of these products; the questionnaire item on bonds does not specify whether retail government securities are included. Some respondents may therefore have interpreted these two items broadly, for example as investment or information functions of their bank's mobile application or as the online purchase of retail government securities. The fintech analyses were therefore repeated without these two items (Section 4.6).

Table 1. Frequency of digital financial activities, 2022 (%).

Activity	Never	Sometimes	Often	Very Often
Checking account balances and transactions online	29.2	16.2	31.2	23.4
Paying bills online	40.9	12.1	27.7	19.3
Shopping online	33.7	28.4	25.9	12.0
Transferring money online	38.3	23.3	26.1	12.2
Managing financial products online	60.4	16.7	15.8	7.2
Paying by mobile phone in stores	65.1	9.6	14.9	10.4
Using an account aggregation app or website	74.1	10.2	11.1	4.6
Using an online securities trading platform	81.4	8.5	7.0	3.0
Using an automated online advisory platform	82.3	7.3	7.1	3.3

Source: Authors' calculations based on the OECD/INFE survey conducted in Hungary. Valid *n* ranges from 820 to 833.

For the nine digital financial activity items, 146 respondents selected "not applicable" for every item, while an additional 30 respondents could not be assigned an index score because of "don't know" or "refused to answer" responses. The 176 excluded respondents were older (59.5 vs. 44.7 years), belonged to lower income categories (1.30 vs. 1.93), and were less likely to hold a current account (42.0% vs. 76.8%). We therefore estimated additional models in which "not applicable" responses were coded as "never" (Table A4). The overall digital financial activity index demonstrated acceptable internal consistency ($\alpha = 0.850$), whereas the seven-item financial knowledge measure showed low internal consistency ($\alpha = 0.377$). The latter measure encompasses different domains of financial

knowledge, and the proportion of correct responses exceeding 84% on four items suggests a ceiling effect (Table A2).

Fully online financial contracting was measured using four yes/no items covering the opening of bank accounts, applications for debit or credit cards, the purchase of insurance, and taking out a loan, all without in-person interactions. An index ranging from 0 to 4 was constructed by summing affirmative responses (N = 854); missing values resulted from "not applicable" responses. All 146 respondents with missing values were non-internet users; all internet users answered each item with yes or no. The index measures the number of financial product types for which contracts were concluded entirely online, rather than the online management of existing contracts. Given the highly skewed distribution of the index (in the regression sample, 494, 126, and 19 respondents had concluded zero, one, and two or more product types online, respectively), the main analyses use a binary indicator of having concluded at least one financial contract entirely online. The count and a three-category version (none, one, two or more) are reported as sensitivity analyses. The contracting measure does not include investment products and therefore does not overlap conceptually with the financial detriment item concerning investments that turned out to be fraudulent. The potential association with the number of financial products held was controlled for in supplementary models (Supplementary File S3). The timing of contract formation was not recorded, so the temporal ordering cannot be established: an online contract may have given rise to the reported detriment, or a detriment (e.g., a refusal in a branch) may have prompted the online contract. Among respondents with valid data, 15.2% had purchased insurance online, while 10.2% had opened a bank account, applied for a card, or taken out a loan online. Banking products, which more closely correspond to remote customer onboarding in the regulatory sense, were therefore also examined separately from insurance products (Section 4.3).

Financial knowledge was measured by the number of correct answers to seven questions, producing a score from 0 to 7. Two questions covered closely related concepts: purchasing power and inflation. We therefore retained only the purchasing power question. The correlation between the seven-item and eight-item scores was 0.98. Our score therefore differs from the OECD/INFE knowledge score, which excludes the division question and includes the definition of inflation.

The ten financial detriment items were grouped into two categories. To reflect their content, the two categories are labeled fraud exposure and incidents and provider disputes and refusals. Five items concerned fraud or attempted fraud involving perpetrators operating outside the regulated financial system, while the remaining five addressed problems arising between respondents and their financial service providers. The term "financial detriment" is used as an umbrella term for events reported by respondents, which did not necessarily involve verified or direct financial losses. These categories were constructed for analytical purposes and do not represent legal classifications. Because only 0.7% of respondents reported losing money as a result of hacking or phishing, the first category primarily captures exposure to fraud attempts and incidents, rather than verified financial losses. The second category combines refusals, which reflect providers' decisions, with complaints, which may also reflect consumers' willingness and ability to assert their rights. We therefore also analyze the components of both categories separately (Section 4.3).

Complaints about high transfer fees were classified as provider disputes and refusals because they concern fee disputes with regulated financial service providers rather than actions by external perpetrators. Because this item may also be associated with the frequency of online transfers, the provider disputes and refusals model was additionally estimated without it (Supplementary File S3).

The item concerning disputed unrecognized transactions may be interpreted in more than one way. Its Hungarian wording also changed between the two survey waves (see above). Accordingly, we repeated the wave comparison and the main models of fraud exposure and incidents after excluding this item (Sections 4.2 and 4.3).

Two of the provider disputes and refusals items concern complaints made by respondents. As filing a complaint may also reflect consumers' ability to assert their rights, refusals and complaints are also examined separately (Section 4.3). Loan application rejection was also classified as provider disputes and refusals, although it may partly reflect respondents' creditworthiness. We therefore additionally estimated the provider disputes and refusals model excluding loan rejection, using the four items also included in the 2018 survey. Because loan rejection was included only in the 2022 questionnaire, comparisons between the two survey waves were based on nine items, whereas the 2022 cross-sectional analyses included all ten items.

Supplementary File S3 and Table A6 describe the variables used only in supplementary and exploratory analyses: educational attainment, number of financial products held, region, settlement type, willingness to take financial risks, financial monitoring, digital security items, and trust in fintech services.

3.2. Analytical Strategy

We compared the weighted proportions for 2018 and 2022 using design-adjusted Wald tests. We also calculated 95% confidence intervals for the differences. Standard errors were estimated by Taylor series linearization. In 2018, municipalities and Budapest districts served as primary sampling units (PSUs). We requested the municipality identifiers for the 2022 sample from the data provider (Pénziránytű Foundation), but these identifiers have not been made available to date.

Given the differences in income distributions between the two samples, changes across survey waves were also examined using weighted logistic regression on the pooled dataset. The model included survey wave, age, gender, and income category. Missing income data were treated as a separate category to retain respondents with missing income (N = 1999). We also assessed the results using chi-square tests with Yates' continuity correction on unweighted data. The comparisons were additionally repeated using only items with identical wording across both waves and accurate Hungarian translations. As the individual comparisons of the nine items were exploratory, Holm-adjusted p-values are also reported to account for multiple testing [42]. Because the 2022 dataset lacks cluster identifiers, the unweighted wave comparisons were also assessed under assumed design effects of 1.5 and 2.0, dividing the chi-square statistics by the design effect; the weighted Wald tests were not adjusted because none of the category-level weighted comparisons (Section 4.2) were significant.

Binary outcome variables from the 2022 survey were analyzed using weighted logistic regression. The 2022 post-stratification weights were applied, and design-adjusted standard errors were calculated using Taylor series linearization. Because the dataset included survey weights but no stratum or cluster identifiers, the sampling design was treated as a single-stage weighted sample. As a result, the standard errors may be underestimated. To assess the sensitivity of the confirmatory results to unmodeled clustering, we multiplied the standard errors by the square root of assumed design effects of 1.5 and 2.0 and recalculated the Holm correction (Table A9). These assumed values were benchmarked against design effects estimated from the 2018 data, which include municipality identifiers, and against clustering on region × settlement-type cells in the 2022 data (Section 4.3). Respondents with missing values were excluded through listwise deletion. Under this sampling specification,

linearized standard errors are virtually identical to those obtained using subpopulation estimation, so listwise deletion does not distort the variance estimates.

For each model, we report the unweighted numbers of respondents and events. We present results as odds ratios (ORs), 95% confidence intervals (CIs), and *p*-values. McFadden's pseudo-R^2 was used to assess explanatory power. We evaluated discrimination using the area under the weighted receiver operating characteristic curve (AUC). To aid the substantive interpretation of the odds ratios, we calculated average predicted probabilities. For each calculation, we assigned the variable of interest a specified value for all respondents and kept the other variables at their observed values. We then averaged the predicted probabilities using survey weights. Confidence intervals were calculated with the delta method.

Fraud exposure and incidents and provider disputes and refusals were examined in separate logistic regression models using the same explanatory variables. The number of explanatory variables was restricted in view of the number of events: given the relatively low event counts, the models included five explanatory variables, or six when fully online financial contracting was added. Region and settlement type were included only in supplementary models (Supplementary File S3). Including both sets of variables would have resulted in a fraud exposure and incidents model with up to 16 explanatory variables based on only 53 events. Because these models were unstable, we did not interpret their results substantively and used them only to check the direction of the coefficients. To examine possible estimation bias arising from the small number of events, we also estimated unweighted Firth's penalized likelihood logistic regression models with profile-likelihood confidence intervals [43,44].

Methodological research suggests that the conventional rule of thumb requiring at least ten events per explanatory variable is not universally applicable, as the required sample size also depends on effect sizes and event prevalence [45,46]. The two outcome models were based on 53 and 65 events, respectively. We therefore report coefficients alongside their confidence intervals and interpret them primarily as indications of the direction and approximate magnitude of the associations.

Sensitivity analyses examined missing income (through multiple imputation and a separate category), additional controls, alternative measures of fully online contracting, overlapping items, respondents at risk of refusals, selection-related covariates, and digital financial activity subindices. Supplementary File S3 provides details. Weighted comparisons between survey waves were conducted in R (version 4.3.3; R Foundation for Statistical Computing, Vienna, Austria) with the survey package (version 4.2.1) [47]. The weighted models were estimated using the Complex Samples module in IBM SPSS Statistics 31 (IBM Corp., Armonk, NY, USA). Firth's penalized logistic regression, multiple imputation, the item-response model, and the power simulation were performed in R using the logistf (https://CRAN.R-project.org/package=logistf, accessed on 24 November 2026) and mice (version 3.16.0) packages. The SPSS syntax and the R script are provided as Supplementary Materials S1 and S2.

Five statistical tests were designated as confirmatory. In the main models presented in Section 4.3, we examined the associations of fully online financial contracting (H1) and digital financial activity (H2) with both types of financial detriment, and the association of financial knowledge (H3) with provider disputes and refusals. Both unadjusted and Holm-adjusted *p*-values are reported for these five tests. The hypotheses were not pre-registered. H1 was initially operationalized using the number of product types for which contracts were concluded fully online (0–4). Because this count was highly skewed (Section 3.1), a binary indicator of at least one fully online contract was adopted for the main models after the initial results were known. The confirmatory tests based on the count are therefore also reported

(Table A9). Given the low internal consistency of the seven-item financial knowledge score, H3 was also assessed using three alternative measures: a three-item score based on the three most difficult items (lowest proportions of correct answers: simple interest, compound interest, and diversification); a binary indicator of at least five correct answers (analogous to the OECD/INFE minimum target score, whose seven items include the definition of inflation instead of the division question); and the first principal component of the seven items. A two-parameter logistic item-response score was examined as an additional check.

All remaining analyses were exploratory. These included examinations of the association between financial knowledge and fraud exposure and incidents, contract types, digital financial activity subindices, trust in fintech services, digital security items, and interaction effects. We formulated no hypotheses for these analyses, and their *p*-values are interpreted descriptively. To aid interpretation of the non-significant finding for financial knowledge, we also estimated the statistical power of the provider disputes and refusals model (Model 1) through simulation (400 replications; for each assumed odds ratio, the intercept was calibrated to the observed weighted prevalence, and the same random draws were used for all odds ratios).

3.3. Hypotheses

Three hypotheses were formulated to address the second research question (RQ2). The first research question (RQ1) is exploratory, so we did not formulate a hypothesis for it.

H1. *Fully online financial contracting is associated with a higher prevalence of both fraud exposure and incidents and provider disputes and refusals.*

From the perspective of routine activity theory, concluding financial contracts through digital channels may increase exposure to fraud. From the perspective of the consumer vulnerability framework, the absence of in-person interactions may make it more difficult to clarify contractual terms, which may lead to subsequent disputes and refusals [17]. Technological verification measures cannot completely eliminate the risk of financial crime [5]. H1 therefore focuses on the mode of financial contracting, distinguishing it from the frequency of financial service use.

H2. *More frequent digital financial activity is associated with a higher prevalence of both fraud exposure and incidents and provider disputes and refusals.*

Routine activity theory suggests that more frequent digital financial activity may create additional opportunities for offenders to reach users [18]. In the consumer vulnerability framework, greater exposure to financial services is itself a condition associated with mistreatment by providers [17]. We therefore expect more frequent financial detriment of both types [7,17]. Capable guardianship could not be measured reliably (Supplementary File S3), so this element of routine activity theory is not tested. Exploratory analyses also examine whether financial detriment is more closely associated with basic digital financial activities or the use of fintech services.

H3. *Consumers with higher levels of financial knowledge are less likely to experience provider disputes and refusals.*

Financial knowledge may help consumers understand contractual terms, costs, and their rights. Within the consumer vulnerability framework, low financial capability is one of the conditions that increase vulnerability to mistreatment, and a US study found that individuals with lower levels of financial capability were more likely to report unfair

treatment by financial service providers [17]. Financial knowledge may therefore play a protective role against provider disputes and refusals. However, given the conflicting findings of previous research, no prior expectation is formulated regarding the direction of the association between financial knowledge and fraud exposure and incidents [21,23,24]. Because of the low internal consistency and ceiling effects of the financial knowledge measure, three alternative measures and an item-response score are examined. Section 3.2 describes the methods used to test the hypotheses, the Holm correction, the alternative financial knowledge measures, the statistical power simulation, and the distinction between confirmatory and exploratory analyses. Figure 1 illustrates the hypotheses and the different roles of the two survey waves, while Table A6 provides detailed descriptions of the variables.

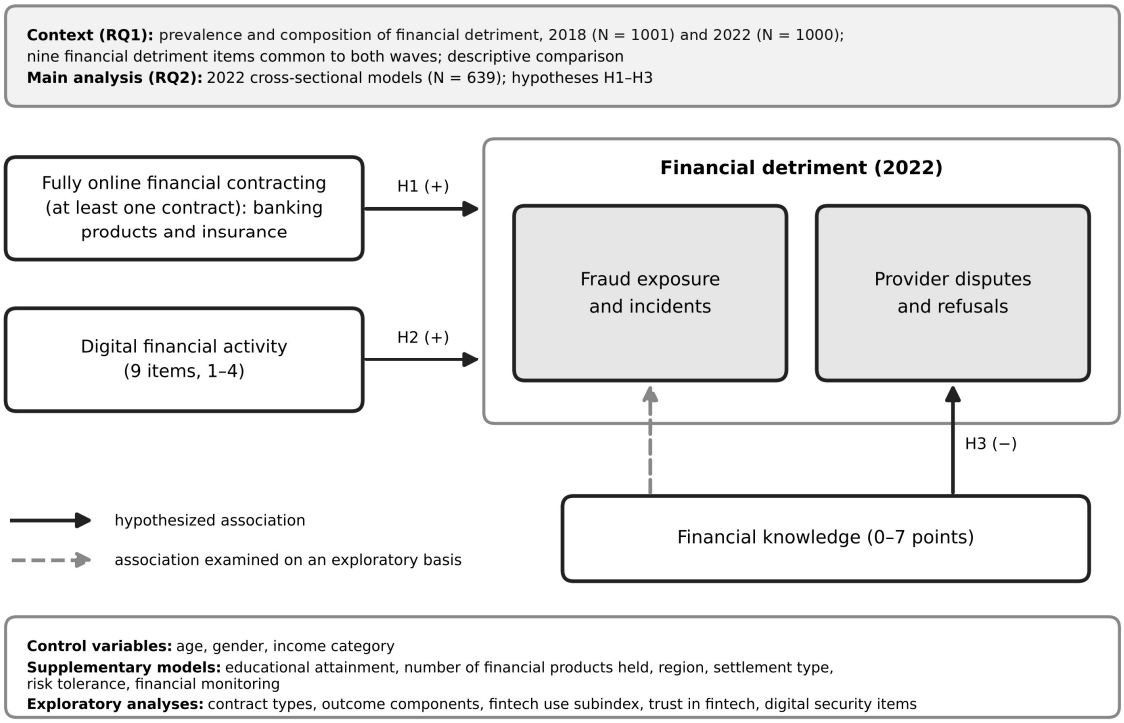


Figure 1. Research model. Source: Authors' own elaboration. Solid arrows represent the hypothesized associations, with (+) and (−) indicating their expected directions. Arrows pointing to the frame refer to both types of financial detriment. The dashed arrow represents an association examined on an exploratory basis without a predefined hypothesis: financial knowledge is also included in the fraud exposure and incidents model.

4. Results

We first describe the 2022 data and then compare the two survey waves; differences in sample composition between the waves (Table A1) are important when interpreting the results.

4.1. Descriptive Results

The 2022 data reveal substantial differences in how frequently respondents used basic digital financial services and fintech services in the narrower sense (Table 1).

Fully online financial contracting was considerably less common than the regular use of digital financial services. Overall, 19.7% of the full sample and 23.1% of respondents with valid responses had concluded at least one financial contract entirely online. Among respondents who provided valid responses, purchasing insurance online was the most common (15.2%), followed by opening a bank account (5.6%), applying for a payment card (4.2%), and taking out a loan (1.6%). Among those who had concluded at least one

financial contract entirely online, 16.2% reported fraud exposure and incidents, compared with 6.4% of other respondents (unweighted; $p < 0.001$). The corresponding proportions reporting provider disputes and refusals were 15.7% and 6.8%, respectively ($p < 0.001$).

More than half of respondents with valid responses (54.6%) checked their account balances and transactions online often or very often. By contrast, most did not use fintech services in the narrower sense. More than four-fifths had never used an online securities trading platform or an automated online advisory service.

The proportions of correct responses also varied substantially across the financial knowledge questions (Table A2). Respondents answered most calculation-based and conceptual questions correctly, but only one-third answered the compound interest question correctly. Table 2 presents descriptive statistics for the regression variables.

Table 2. Descriptive statistics of model variables, 2022.

Variable	N	Mean	SD	Min.	Max.
Digital financial activity (1–4)	824	1.83	0.67	1.0	3.9
Financial knowledge (0–7)	1000	5.39	1.18	0	7
Age (years)	998	47.27	16.18	18	79
Income category (1–3)	789	1.81	0.75	1	3
Fully online financial contracting (0–4)	854	0.27	0.53	0	4
Fully online financial contracting, at least one (0/1)	854	0.23	0.42	0	1
Number of financial products held	1000	1.56	1.29	0	13
Trust in fintech services (1–5)	917	2.64	1.38	1	5

Source: Authors' calculations based on the OECD/INFE survey conducted in Hungary.

4.2. Changes in Financial Detriment, 2018–2022

We first examine changes in the prevalence of financial detriment between 2018 and 2022. The comparison uses nine items included in both survey waves. Table 3 reports the overall prevalence of each type of financial detriment. Table A3 provides results for individual items.

Table 3. Financial detriment by type, 2018 and 2022 (%).

Type	2018	2022	Change (95% CI)	*p*	2018 (Substantive Responses Only)	*p*	Adjusted OR (95% CI)	*p*
Fraud exposure and incidents (5 items)	5.5	7.8	+2.3 (−0.6; 5.2)	0.127	5.1 ($n = 962$)	0.061	1.46 (0.88–2.42)	0.147
Fraud exposure and incidents, excluding the unrecognized transaction item (4 items)	4.3	4.4	0.0 (−2.6; 2.6)	0.980	–	–	1.02 (0.54–1.91)	0.954
Provider disputes and refusals (4 items)	8.2	5.9	−2.3 (−5.5; 0.9)	0.159	8.0 ($n = 965$)	0.202	0.75 (0.47–1.20)	0.226
Any financial detriment	11.1	12.8	+1.7 (−2.2; 5.6)	0.399	10.4 ($n = 955$)	0.222	1.22 (0.83–1.80)	0.304

Source: Authors' calculations based on the OECD/INFE surveys conducted in Hungary. Weighted proportions; design-adjusted Wald tests, accounting for municipality-level clustering in the 2018 sample.

Loan application rejection was included only in the 2022 questionnaire and was therefore excluded from the comparison between the two survey waves. Accordingly, provider disputes and refusals were assessed using four items in Table 3. When loan rejection was included, the prevalence of the five-item provider disputes and refusals measure in 2022

was 9.3% in the weighted data and 8.7% in the unweighted data. The column labeled "2018 (substantive responses only)" and its corresponding *p*-value refer to respondents in the 2018 survey who provided substantive responses to all items within the respective category. As no "don't know" or refused responses were recorded in 2022, the estimates for that wave were identical under both coding approaches. The adjusted OR represents the odds ratio for the 2022 survey wave in the pooled weighted logistic regression model controlling for age, gender, and income category, with missing income treated as a separate category (N = 1999). Unweighted comparisons are reported in File S3, Section S3.3. Changes were calculated from unrounded proportions and may therefore differ from the differences between the rounded values.

The overall prevalence of financial detriment did not change significantly. Fraud exposure and incidents increased from 5.5% to 7.8%, but this difference was not statistically significant in the weighted comparison ($p = 0.127$) or in the model adjusted for sample composition (OR = 1.46; $p = 0.147$). Almost the entire increase was attributable to an ambiguously worded item concerning disputes over unrecognized transactions. After excluding this item, the prevalence was virtually unchanged: 4.3% in 2018 and 4.4% in 2022 (adjusted OR = 1.02; $p = 0.954$).

Provider disputes and refusals declined from 8.2% to 5.9%, but the change was not statistically significant ($p = 0.159$). This conclusion also held under the assumed design effects, and overall detriment did not change significantly when the comparison was restricted to the four items with identical wording and accurate translations (File S3, Section S3.3). Only the unweighted tests, which did not account for clustering in either wave, indicated nominally significant changes in the two outcomes ($p = 0.032$ and 0.044; Supplementary File S3).

4.3. Digital Financial Participation and Financial Detriment

The regression findings pertain to internet users who reported their income and should not be generalized to the Hungarian adult population as a whole. Due to missing data, the regression sample was smaller, and the weighted prevalence of financial detriment was slightly higher than in the full 2022 sample (fraud exposure and incidents: 8.4% vs. 7.8%; provider disputes and refusals, five items: 11.4% vs. 9.3%). The 361 respondents excluded from the regression analyses were older (mean age: 50.9 vs. 45.3 years), belonged to lower income categories (mean: 1.31 vs. 1.93), and were less likely to hold a current account (58.2% vs. 77.8%) than those included in the analyses. The potential implications of missing data were assessed through the multiple imputation analysis presented in Supplementary File S3. Of the 361 excluded respondents, 176 had no valid digital financial activity score, including 146 who did not use the internet; a further 183 had a valid activity score but did not report their income; and 2 had missing age (Table S1). Provider disputes and refusals were less common among the excluded respondents (6.1% vs. 10.2%), so the excluded group does not appear to carry a higher burden of this outcome.

We examined fraud exposure and incidents separately from provider disputes and refusals. For each outcome, Table 4 reports two models. Model 1 excludes fully online financial contracting, while Model 2 includes it.

In Model 1, higher digital financial activity was associated with a higher prevalence of both types of financial detriment. When fully online financial contracting was included (Model 2), having concluded at least one financial contract entirely online was significantly associated with both fraud exposure and incidents (OR = 2.94; 95% CI: 1.54–5.62) and provider disputes and refusals (OR = 3.18; 95% CI: 1.78–5.67). The inclusion of fully online contracting also increased the explanatory power of both models. Digital financial activity remained significantly associated with provider disputes and refusals (OR = 1.53;

$p = 0.034$), but its association with fraud exposure and incidents was no longer statistically significant (OR = 1.28; $p = 0.343$). The two digital variables were moderately correlated (r = 0.30). After applying the Holm correction to the five confirmatory tests, three associations remained statistically significant: those between fully online financial contracting and both types of financial detriment, and between digital financial activity and provider disputes and refusals. The association between digital financial activity and fraud exposure and incidents was not statistically significant (adjusted $p = 0.084$). Financial knowledge was also not significantly associated with provider disputes and refusals (H3; Model 1: OR = 0.84; $p = 0.229$). Similarly, the exploratory analysis found no significant association between financial knowledge and fraud exposure and incidents, although the odds ratios were below one. The interaction between financial knowledge and digital financial activity was also not statistically significant ($p = 0.726$ and $p = 0.719$).

Table 4. Factors associated with financial detriment: weighted logistic regression, 2022.

Variable	Fraud Exposure and Incidents: Model 1	Fraud Exposure and Incidents: Model 2	Provider Disputes and Refusals: Model 1	Provider Disputes and Refusals: Model 2
Fully online financial contracting (at least one)	–	2.94 (1.54–5.62); 0.001	–	3.18 (1.78–5.67); <0.001
Digital financial activity	1.60 (1.02–2.50); 0.042	1.28 (0.77–2.12); 0.343	1.91 (1.31–2.79); <0.001	1.53 (1.03–2.25); 0.034
Financial knowledge	0.89 (0.68–1.15); 0.366	0.90 (0.67–1.21); 0.487	0.84 (0.64–1.12); 0.229	0.85 (0.63–1.15); 0.299
Age	1.01 (0.99–1.03); 0.183	1.01 (0.99–1.03); 0.193	1.00 (0.98–1.02); 0.851	1.00 (0.98–1.02); 0.932
Male	1.15 (0.64–2.08); 0.643	1.03 (0.57–1.86); 0.927	0.92 (0.52–1.64); 0.787	0.80 (0.44–1.45); 0.462
Income category	0.87 (0.55–1.36); 0.537	0.83 (0.54–1.29); 0.414	0.76 (0.49–1.17); 0.207	0.72 (0.47–1.11); 0.138
Pseudo-R^2	0.018	0.048	0.034	0.070
AUC	0.612	0.677	0.638	0.686

Source: Authors' calculations based on the OECD/INFE survey conducted in Hungary. Cells report OR (95% CI), p-values. N = 639 in all models; events = 53 (fraud exposure and incidents) and 65 (provider disputes and refusals). Holm-adjusted p-values for the five confirmatory tests: fully online financial contracting (Model 2), 0.003 (fraud exposure and incidents) and <0.001 (provider disputes and refusals); digital financial activity (Model 1), 0.084 (fraud exposure and incidents) and 0.003 (provider disputes and refusals); financial knowledge (Model 1, provider disputes and refusals), 0.229.

Because the 2022 dataset contains no cluster identifiers, the confirmatory tests were also repeated with standard errors inflated by assumed design effects (Table A9). With a design effect of 1.5, all associations that were significant after the Holm correction remained so (adjusted $p \leq 0.025$). With a design effect of 2.0, only the association between fully online financial contracting and provider disputes and refusals remained below 0.05 (adjusted $p = 0.029$), whereas the adjusted p-values for fully online contracting and fraud exposure and incidents and for digital financial activity and provider disputes and refusals were 0.071. With the count specification (Section 3.2), the Holm-adjusted p-values for H1 were 0.040 and 0.038 without and 0.136 with an assumed design effect of 1.5, so H1 is sensitive to both the coding of the exposure and the unmodeled clustering. As a benchmark, the design-based wave comparisons in Table 3, which account for municipality-level clustering in the 2018

sample, imply design effects of about 1.8–2.1 for the differences in prevalence, compared with 1.19 due to weighting alone in the full 2022 sample (1.14 in the regression sample). Because each difference combines a clustered 2018 estimate with a 2022 estimate that reflects weighting only, we also estimated the 2018 design effects directly. Specifically, we estimated weighted 2018 models for the four detriment indicators, including the covariates common to both waves (age, gender, and income category, with missing income treated as a separate category), both with and without municipality-level clustering (107 municipalities and Budapest districts as PSUs). Relative to the weighted estimates without clustering, the design effects were 2.38–2.65 for the prevalences (2.68–3.28 relative to simple random sampling, the reference of 1.8–2.1 above) but 0.89–1.68 (median 1.25) for the coefficients of age, gender, and income category; values of about 2–3 occurred only for the sparse missing-income indicator. Both surveys used two-stage designs with municipalities as PSUs and quotas for gender and age, making similar clustering in 2022 plausible. However, this cannot be verified because the municipality identifiers for the 2022 sample were requested but have not been made available by the data provider. Rescaled to the 2022 cluster size ($\text{deff} = 1 + (\text{m} - 1)\rho$; about 7.9 respondents per municipality in the regression sample and 12.3 in the full sample, compared with 9.4 in 2018), the median coefficient design effect was 1.20–1.33 and the maximum 1.56–1.93. As an additional check with the 2022 data, clustering the standard errors on the 26 region × settlement-type cells of the regression sample yielded design effects of 1.04–2.20 for the five confirmatory coefficients; both H1 associations remained significant after the Holm correction (adjusted $p = 0.018$ and 0.023), whereas H2 for provider disputes and refusals was borderline (adjusted $p = 0.060$). Because these cells are larger and fewer than the municipalities, this check is approximate. A design effect of 1.5 can therefore be regarded as a realistic scenario and 2.0 as a conservative scenario for the regression coefficients.

Because purchasing insurance was the most common form of fully online financial contracting, banking products (bank account opening, payment card applications, and taking out a loan) and insurance products were also analyzed separately (Table 5). In the regression sample, 61 respondents had concluded at least one banking product contract entirely online, 97 had purchased insurance online, and 13 had done both. The statistically significant associations were concentrated mainly among banking products. When banking products and insurance were included in the same model, fully online contracting for banking products was associated with higher odds of both fraud exposure and incidents (OR = 2.79; $p = 0.010$) and provider disputes and refusals (OR = 3.16; $p = 0.002$). Online insurance contracting was not significantly associated with either type of financial detriment, although the banking and insurance coefficients did not differ significantly (Wald test: $p = 0.604$ and 0.202). These analyses were conducted on an exploratory basis, and only 61 respondents had concluded a banking product contract entirely online; consequently, the estimates had wide confidence intervals.

Because the two outcome measures encompass heterogeneous events, their components were also analyzed separately (Table 6). When the ambiguously worded item on disputed unrecognized transactions was excluded from fraud exposure and incidents (33 events), the association with fully online financial contracting was smaller and not statistically significant (OR = 2.10; $p = 0.057$), whereas digital financial activity was associated with the remaining four items (OR = 1.68; $p = 0.046$). The unrecognized transaction item itself was associated with fully online contracting (OR = 3.81; $p = 0.006$) but not with digital financial activity (OR = 0.92; $p = 0.827$). Among provider disputes and refusals, fully online financial contracting was associated with both refusals (36 events; OR = 2.72; $p = 0.009$) and complaints (34 events; OR = 3.86; $p < 0.001$), whereas digital financial activity was associated only with complaints (OR = 1.90; $p = 0.014$; refusals: OR = 1.36; $p = 0.182$).

The association of digital financial activity with complaints may partly reflect consumers' propensity to assert their rights rather than harm. As the component results show, the association between fully online contracting and fraud exposure and incidents rests largely on the disputed-transaction item, which may also reflect vigilance or assertiveness rather than harm. These component-level analyses should be regarded as exploratory.

Table 5. Fully online contracting by contract type: banking products and insurance, weighted logistic regression, 2022.

Specification	Fraud Exposure and Incidents: Banking Products	Fraud Exposure and Incidents: Insurance	Provider Disputes and Refusals: Banking Products	Provider Disputes and Refusals: Insurance
Entered separately	2.87 (1.34–6.15); 0.007	2.09 (0.96–4.54); 0.062	3.19 (1.54–6.60); 0.002	1.56 (0.81–3.02); 0.187
Entered jointly	2.79 (1.29–6.07); 0.010	2.04 (0.93–4.47); 0.074	3.16 (1.50–6.63); 0.002	1.54 (0.77–3.08); 0.223

Source: Authors' calculations based on the OECD/INFE survey conducted in Hungary. Cells report OR (95% CI); *p*-value. Binary indicators: fully online account opening, card application, or taking out a loan (at least one); insurance purchased entirely online. All models include age, gender, income category, digital financial activity, and financial knowledge; N = 639; events = 53 (fraud exposure and incidents) and 65 (provider disputes and refusals).

Table 6. Components of the outcome measures: weighted logistic regression, 2022.

Outcome Component	Items	Events	Fully Online Contracting (At Least One)	Digital Financial Activity
Fraud exposure and incidents without the unrecognized transaction item	QP10_1, 2, 3, 10	33	2.10 (0.98–4.49); 0.057	1.68 (1.01–2.81); 0.046
Disputed unrecognized transaction	QP10_4	23	3.81 (1.47–9.84); 0.006	0.92 (0.43–1.98); 0.827
Refusals (account opening, insurance claim, loan application)	QP10_6, 7, 8	36	2.72 (1.29–5.72); 0.009	1.36 (0.86–2.15); 0.182
Complaints (formal complaint, transfer fees)	QP10_5, 9	34	3.86 (1.96–7.61); <0.001	1.90 (1.14–3.17); 0.014

Source: Authors' calculations based on the OECD/INFE survey conducted in Hungary. Cells report OR (95% CI); *p*-value. Models follow Model 2 in Table 4 (age, gender, income category, financial knowledge, digital financial activity, fully online financial contracting); N = 639. These analyses are exploratory.

4.4. The Magnitude of the Associations

Table 7 translates the odds ratios into average predicted probabilities to illustrate their practical magnitude. Panel A sets fully online financial contracting to none and to at least one for all respondents. These estimates are based on Model 2 in Table 4. Panel B shows predicted probabilities at the 10th, 50th, and 90th percentiles of digital financial activity, using Model 1 in Table 4. These are model-based predicted probabilities, not observed proportions for the respondent groups.

Had all respondents concluded at least one financial contract entirely online, the average predicted probability of fraud exposure and incidents would have been 16.0%, compared with 6.2% had none done so; the corresponding probabilities for provider disputes and refusals were 21.7% and 8.2%. These differences are substantial, although the wide confidence intervals indicate considerable uncertainty.

Table 7. Average predicted probabilities, 2022 (%, 95% CI).

Outcome	None (n = 494)	At Least One (n = 145)	–
Panel A. By fully online financial contracting			
Fraud exposure and incidents	6.2 (4.0–8.3)	16.0 (8.9–23.2)	–
Provider disputes and refusals	8.2 (5.4–11.0)	21.7 (14.1–29.2)	–
Panel B. By level of digital financial activity			
	Low (10th percentile)	**Median**	**High (90th percentile)**
Fraud exposure and incidents	5.8 (2.9–8.7)	8.1 (5.7–10.4)	12.4 (7.5–17.2)
Provider disputes and refusals	6.8 (3.7–9.9)	10.5 (7.6–13.4)	18.5 (12.8–24.2)

Source: Authors' calculations based on the OECD/INFE survey conducted in Hungary. The focal variable was set to the specified value for every respondent, while other variables were held at their observed values, yielding weighted averages with delta-method confidence intervals. Percentiles refer to the weighted regression sample. Column headings report the observed number of respondents in each group, whereas the predicted probabilities are counterfactual values calculated for all 639 respondents.

4.5. Robustness of the Findings

Supplementary File S3 reports the robustness checks in detail. Fully online financial contracting remained associated with both outcomes in all binary specifications in Table A5 (ORs 2.3–3.7) and in the count specification (ORs 1.91 and 1.87), after removing the items that overlap between exposure and outcome (Table A8), among respondents at risk of refusals and rejections (Table A10), and after adjustment for risk tolerance, financial monitoring, and the number of products held (Table S4). E-values calculated using the conservative square-root transformation of the odds ratios were 2.82 and 2.96 (1.79 and 2.00 for the lower confidence limits). When calculated using the risk ratios implied by Table 7 (2.61 and 2.65), the corresponding E-values were 4.66 and 4.74 (2.32 and 2.67 for the lower confidence limits). The association of digital financial activity with provider disputes and refusals was robust in models without fully online contracting but not in several of the models that included it. The H3 results were similar for all alternative knowledge measures, including an item-response score, but power was low (34% for OR = 0.85). The digital security items were largely unrelated to the outcomes (Table S3), and the interactions with age and income were not significant but underpowered (Table S5).

4.6. Basic Digital Financial Activities and Fintech Services

We first examined the two components of digital financial activity by including both subindices in the models instead of the overall index (Table 8). When examined simultaneously, the basic digital financial activities subindex was associated with provider disputes and refusals, whereas the fintech subindex showed no independent association with either type of financial detriment (Panel A), although the two subindices did not differ significantly (Wald test: $p = 0.710$ and 0.187). When fintech use was measured as a binary variable, users had higher odds of fraud exposure and incidents (OR = 2.03; $p = 0.028$; Panel B). However, this association was no longer statistically significant after fully online financial contracting was included (OR = 1.85; $p = 0.067$), while fully online financial contracting was associated with both types of financial detriment (Panel C). In this model, the association between basic digital financial activities and provider disputes and refusals was also attenuated (OR = 1.35; $p = 0.093$). The binary fintech use indicator was constructed specifically for this analysis; like all subindex analyses (Section 3.2), the results in Table 8 are exploratory. When the online securities trading and automated advisory items were excluded, the fintech measure was not associated with either type of financial detriment in any panel (all $p > 0.23$),

including fraud exposure and incidents in Panel B (OR = 1.33; $p = 0.389$), whereas the estimates for basic digital financial activities and fully online financial contracting remained similar (Table S2).

Table 8. Basic digital financial activities and fintech use in the narrower sense: weighted logistic regression, 2022.

Outcome	N	Events	Basic Digital Financial Activities	Fintech Use	Fully Online Contracting (At Least One)
Panel A. Both subindices as mean scores (1–4)					
Fraud exposure and incidents	639	53	1.34 (0.88–2.02); $p = 0.172$	1.17 (0.77–1.78); $p = 0.466$	–
Provider disputes and refusals	639	65	1.66 (1.15–2.38); $p = 0.006$	1.13 (0.82–1.56); $p = 0.439$	–
Panel B. Fintech use as a binary variable					
Fraud exposure and incidents	639	53	1.19 (0.83–1.70); $p = 0.339$	2.03 (1.08–3.81); $p = 0.028$	–
Provider disputes and refusals	639	65	1.58 (1.12–2.22); $p = 0.009$	1.51 (0.84–2.71); $p = 0.169$	–
Panel C. Panel B model additionally adjusted for fully online contracting					
Fraud exposure and incidents	639	53	1.01 (0.69–1.48); $p = 0.956$	1.85 (0.96–3.57); $p = 0.067$	2.83 (1.44–5.55); $p = 0.003$
Provider disputes and refusals	639	65	1.35 (0.95–1.93); $p = 0.093$	1.35 (0.74–2.45); $p = 0.329$	3.04 (1.71–5.39); $p < 0.001$

Source: Authors' calculations based on the OECD/INFE survey conducted in Hungary. Odds ratios with 95% confidence intervals in parentheses. All models include age, gender, income category, and financial knowledge. The binary fintech indicator identifies respondents who use at least one of the four services at least sometimes (43.7% of the regression sample; 44.1% of all valid respondents).

Trust in fintech services was associated with fintech use but not with fully online financial contracting. Adding trust to the main models did not materially change the findings (Table A7). Respondents who had experienced fraud exposure and incidents reported somewhat lower trust, but the association did not reach statistical significance (OR = 0.75; $p = 0.058$).

4.7. Summary of Hypotheses

The results of hypothesis testing are summarized in Table 9. H1 was supported for both types of financial detriment in the main models. The association with provider disputes and refusals remained significant in all binary specifications, whereas the association with fraud exposure and incidents no longer reached the 5% significance threshold after excluding the item concerning unrecognized transactions (OR = 2.10; $p = 0.057$) or under an assumed design effect of 2.0 (Holm-adjusted $p = 0.071$). H2 was supported for provider disputes and refusals, although this result did not withstand an assumed design effect of 2.0 (Holm-adjusted $p = 0.071$); for fraud exposure and incidents, it was not supported in the confirmatory test ($p = 0.042$; Holm-adjusted $p = 0.084$), and the association was not significant once fully online financial contracting was included. H3 was not supported by any of the alternative measures of financial knowledge. Given the low reliability and ceiling effect of the knowledge measure and the limited power of the test, this result is inconclusive rather than evidence of no association.

Table 9. Summary of hypothesis testing results.

Hypothesis	Assessment	Supporting Evidence
H1. *Fully online financial contracting is associated with a higher prevalence of both fraud exposure and incidents and provider disputes and refusals.*	Supported in the main models, although the result for fraud exposure and incidents is sensitive to the outcome definition and to assumed design effects, and both lose significance when count coding is combined with a design effect of 1.5 (Table A9).	Fraud exposure and incidents: OR = 2.94 ($p = 0.001$; Holm-adjusted $p = 0.003$); without the unrecognized transaction item, OR = 2.10 ($p = 0.057$; Table 6). Provider disputes and refusals: OR = 3.18 ($p < 0.001$; Holm-adjusted $p < 0.001$); significant in all binary specifications (Table A5). Holm-adjusted p under an assumed design effect of 2.0: 0.071 and 0.029 (Table A9).
H2. *More frequent digital financial activity is associated with a higher prevalence of both fraud exposure and incidents and provider disputes and refusals.*	Partially supported: the association was more consistent for provider disputes and refusals.	Provider disputes and refusals: OR = 1.91 ($p < 0.001$; Holm-adjusted $p = 0.003$); Holm-adjusted $p = 0.025$ and 0.071 under design effects of 1.5 and 2.0 (Table A9); not significant in several models with fully online contracting. Fraud exposure and incidents: OR = 1.60 ($p = 0.042$; Holm-adjusted $p = 0.084$); OR = 1.28 ($p = 0.343$) with fully online contracting (Table 4).
H3. *Consumers with higher levels of financial knowledge are less likely to experience provider disputes and refusals.*	Inconclusive (not supported in the confirmatory test); the data are uninformative because of the low reliability, the ceiling effect, and limited power.	OR = 0.84 ($p = 0.229$; Holm-adjusted $p = 0.229$; Table 4). No alternative knowledge measure or item-response score was significant in Model 1 or Model 2 (all $p > 0.24$; File S3, Section S3.4); power was 34% for OR = 0.85.

Source: Authors' own compilation.

5. Discussion

This study applies the exposure perspective of routine activity theory to a specific financial event, namely the establishment of a contractual relationship online, rather than to general online activity; however, the result for fraud exposure and incidents rests largely on one ambiguous item, so this contribution is tentative. Our findings indicate that the mode of financial contracting is associated with financial detriment even after accounting for digital financial activity. Because capable guardianship could not be measured, routine activity theory served as a sensitizing framework rather than being tested in full, and the findings for provider disputes and refusals are interpreted within the consumer vulnerability framework [17].

For the first research question, we found no significant change in the overall prevalence of financial detriment. The non-significant increase in fraud exposure and incidents was almost entirely attributable to disputes over unrecognized transactions, which may reflect more frequent account monitoring rather than more fraud, and the decline in provider disputes and refusals was not statistically significant ($p = 0.159$). These comparisons are descriptive because we did not test the competing mechanisms outlined in the Introduction.

The main finding concerns H1. Having concluded at least one financial contract entirely online was associated with higher odds of both types of financial detriment, and the binary estimates remained significant in all binary specifications in Table A5; in the three-category specification, only the small group with two or more online contracts did not differ significantly in provider disputes and refusals ($p = 0.061$). The association with fraud exposure and incidents was, however, not significant without the disputed-transaction item ($p = 0.057$), and with the count specification (Section 3.2), H1 did not withstand an assumed

design effect of 1.5 (Table A9). Two qualifications are important. First, in exploratory analyses, the associations were significant for banking products (account opening, payment card applications, and taking out a loan) but not for online insurance contracting, although the difference between the two was not statistically significant (Wald test: $p = 0.604$ and 0.202); because only 61 respondents had concluded a banking product contract online, this distinction requires confirmation in larger samples. Studies and regulatory guidelines on remote onboarding also note that technological verification does not entirely eliminate risks [5,6]. Second, the association with fraud exposure and incidents rests largely on the disputed-transaction item, which may reflect vigilance or assertiveness as much as harm; without it, the association was smaller and no longer statistically significant. Thus, the evidence for H1 is stronger for provider disputes and refusals than for fraud exposure and incidents.

We examined several alternative explanations (Supplementary File S3). The association of fully online financial contracting with financial detriment was not explained by the number of financial products held, the overlap between the contracting and refusal items, complaints about transfer fees, or self-reported risk tolerance and financial monitoring, and it persisted among respondents at risk of refusals. The E-values for the lower confidence limits, calculated using the conservative square-root transformation of the odds ratios, were 1.79 and 2.00. An unmeasured confounder associated with both online contracting and the outcome by risk ratios of approximately 1.8–2.0 could therefore shift the lower confidence limits to the null. When calculated using the risk ratios derived from the average predicted probabilities in Table 7 (2.61 and 2.65), the E-values were 4.66 and 4.74 for the point estimates and 2.32 and 2.67 for the lower confidence limits. Thus, the values obtained using the square-root transformation provide a lower bound on the strength of confounding required. Other selection factors, such as the number of banking relationships or the type of provider (incumbent or digital bank), were not recorded. Differences in the detection of unrecognized transactions cannot be ruled out. This item was associated with fully online contracting, and removing it weakened the association with fraud exposure and incidents. Finally, fully online contracting was associated with both refusals and complaints, so the finding cannot be explained solely by complaint-filing behavior.

The findings for H2 were less straightforward. Digital financial activity was associated with provider disputes and refusals in all specifications without fully online contracting but not in several specifications that included it (File S3, Section S3.4), consistent with the view that greater exposure to financial services increases opportunities for disputes [17]. However, in the exploratory component analyses, it was significant for complaints (OR = 1.90) but not for refusals (OR = 1.36; Table 6). This may partly reflect a greater willingness among digitally active consumers to assert their rights. For fraud exposure and incidents, the association was not significant once fully online contracting was included, although, unlike the contracting association, it did not depend on the ambiguous unrecognized transaction item (Table 6), so it remains unclear whether this risk is more closely related to digital financial activity or to the mode of contracting. Previous studies based on routine activity theory have linked more frequent online activity mainly to exposure to fraud attempts [8,14]; our findings suggest that the form of digital participation may also matter. In exploratory analyses of the subindices, provider disputes and refusals were significantly associated with basic digital financial activities but not with fintech services in the narrower sense, although the two subindices did not differ significantly (Wald test: $p = 0.187$). The apparent association between fintech use and fraud exposure depended on two items that some respondents probably interpreted broadly, and the small number of events (53 and 65) limits statistical power, so the absence of an association does not imply that they are risk-free.

The exploratory finding concerning trust is consistent with previous research explaining fintech adoption [16,35]. However, the direction of the relationship between trust and financial detriment cannot be established from cross-sectional data, and the use of a single-item measure warrants further caution [37]. Detailed results are presented in Table A7.

H3 was not supported. However, the knowledge measure had low reliability and a ceiling effect, and the test had limited power. The finding is therefore inconclusive and should not be interpreted as evidence that no association exists. The negative association, which reached the significance threshold only in the unweighted and Firth models, is consistent in direction with the higher prevalence of unfair treatment among consumers with lower financial capability reported by Lim and Letkiewicz [17]. For fraud exposure and incidents, studies from Italy and the United States found that higher objective financial literacy was associated with a lower likelihood of online fraud victimization [23,24], whereas a study in China found higher exposure and victimization among those with higher financial literacy [21]. Because our measure mainly captures exposure rather than losses, the exploratory non-significant finding is compatible with either pattern.

We could not reliably measure protection against digital financial risks. The digital security items did not form a coherent scale and were largely unrelated to the outcomes. This may indicate shortcomings in how the items capture security-related behavior; it does not establish that protective measures are ineffective [19]. The interactions with age and income were underpowered. Age is a plausible moderator, as older adults may be more exposed to online scams and face specific barriers in using fintech services [8,36], but larger samples of older digital users would be needed to test this.

The findings identify associations; they do not demonstrate that changing the mode of contracting would itself alter financial detriment. Moreover, they apply only to internet users who reported their income, as older adults with lower incomes who do not use digital channels were not represented in the regression analyses. Three practical implications follow, subject to these limitations. First, consumer outcomes associated with fully online contracting, particularly for banking products (an exploratory finding), may merit monitoring by providers and supervisors alongside efforts to prevent money laundering [6]. Second, because fully online contracting was associated with both refusals and complaints, the explanations given before remote contracts are concluded and the handling of related complaints merit attention; the duty to provide adequate explanations and the right to request human intervention under Directive (EU) 2023/2673 address the first of these, and their effects could be examined in future survey waves [32]. Third, our exploratory analyses suggest that consumer risk monitoring should not focus only on fintech services in the narrower sense, because provider disputes and refusals were associated mainly with widely used digital financial activities.

Given the low reliability and ceiling effect of the financial knowledge measure, the findings do not support conclusions about the protective role of financial education. In addition to financial knowledge, digital security awareness and the ability to recognize deceptive practices may be important for fraud prevention. Security measures implemented by financial service providers may also contribute to consumer protection independently of individuals' financial knowledge.

6. Conclusions

This study examined changes in the prevalence of financial detriment between 2018 and 2022 and assessed how different forms of digital financial participation were associated with detriment reported in 2022.

The overall prevalence of financial detriment did not change significantly. Fully online financial contracting was associated with higher odds of both fraud exposure and incidents and provider disputes and refusals, although the former finding was sensitive to the outcome definition and to assumed design effects, and both lost significance when count coding was combined with a design effect of 1.5. Digital financial activity was associated with provider disputes and refusals in most specifications, although this association was also sensitive to assumed design effects; exploratory analyses suggest that it concerned basic activities and complaints. The results for financial knowledge were inconclusive.

Overall, the findings suggest that different forms of digital financial participation are associated with distinct patterns of consumer-reported financial detriment. Assessments of fintech adoption should therefore consider not only the frequency of financial service use but also the mode of financial contracting; the exploratory finding for banking products merits further research.

Several limitations should be acknowledged. The data are cross-sectional, and the dates of fully online contracting were not recorded, so the temporal order of contracting and detriment is unknown: an online contract may have given rise to the detriment, or a rejection in a branch may have prompted a switch to online channels. The survey did not record product applications, so the population at risk of refusals could only be approximated by product holding and recent product choice; because holding a product partly reflects successful applications, this approximation excludes some refused applicants (4 of the 33 respondents reporting a refused account opening or loan rejection neither held nor had recently chosen a banking product). The models had limited explanatory power (pseudo-R^2 = 0.018–0.070) and weak discriminatory performance (AUC = 0.612–0.686). They should therefore be used to describe associations, not to predict individual outcomes.

The financial detriment models were based on relatively few events (53 and 65, respectively), limiting the precision of the estimates and the number of variables that could be included. Financial detriment was measured through self-reports, meaning that fraudulent activities not recognized by respondents were not captured in the data. The measure of fraud exposure and incidents includes both attempted fraud and actual incidents, and one of its items is open to multiple interpretations. Excluding this item reduces the number of events to 33.

The measures also have limitations. Trust in fintech services was assessed with a single item, the items on online securities trading and automated advice were probably interpreted broadly, and the digital security items did not form a scale, so capable guardianship could not be measured. The financial knowledge score had low reliability and ceiling effects, which, together with limited power, made the data uninformative about H3. Each fintech service in the narrower sense was used by only 17.7–34.9% of respondents with valid responses (Table 1). In addition, the Hungarian questionnaire omitted items on non-bank mobile payment accounts, prepaid cards and crowdfunding.

Comparisons between the two survey waves were limited by differences in income distributions, the lack of municipality identifiers in 2022, revised translations of several items, and the different recording of non-substantive responses. The regression sample comprised 639 of the 1000 respondents in 2022; excluded respondents were older and had lower incomes, and although imputation and recoding yielded similar results, selection bias cannot be ruled out. The focus on a single country further limits the generalizability of the findings.

Moreover, the findings cannot be generalized to adults who do not use the internet or who did not report their income. Because the 2022 dataset did not contain stratum or cluster identifiers, the design-adjusted standard errors may be somewhat underestimated. Under an assumed design effect of 2.0, two of the three significant confirmatory associ-

ations exceeded the 0.05 threshold after the Holm correction (adjusted $p = 0.071$). The 2018 data suggest that design effects for regression coefficients are typically about 1.2–1.3 after rescaling to the 2022 cluster size; under these design effects, all three associations remained significant. However, clustering in 2022 could not be assessed directly, so both sets of results should be interpreted with caution. Finally, the analysis included several exploratory comparisons using the same dataset; only the five tests associated with hypotheses H1–H3 were designated as confirmatory. These hypotheses were not pre-registered, and the operationalization of H1 was revised after the initial results were known (Section 3.2). The Holm correction accounts for multiple testing across the five tests but does not, in itself, make them strictly confirmatory.

Future research should determine whether financial contracting preceded reported detriment, for example through longitudinal panels or providers' customer records. It should also distinguish traditional banks, digital banks, and non-bank fintech providers. Measurement could also be improved: financial knowledge should be measured with more items of varying difficulty, financial detriment items could record the amount of loss and whether it was reported or recovered, and digital security behavior and trust should be measured with multi-item instruments [37]. If a future OECD/INFE survey retains the digital financial activity section, changes in digital participation and detriment could also be tracked over time.

Supplementary Materials: The following supporting information can be downloaded at https://www.mdpi.com/article/10.3390/fintech5040113/s1: File S1: SPSS syntax (variable construction, descriptive statistics, and survey-weighted logistic regression models); File S2: R script (2018–2022 comparisons, Firth regression, multiple imputation, predicted probabilities, design-effect sensitivity analysis and benchmarks, E-values, item-response model, and power analysis); File S3: Supplementary methods and results (Section S3.1: Measurement details; Section S3.2: Additional analytical procedures; Section S3.3: Changes between 2018 and 2022: additional results; Section S3.4: Robustness analyses; Table S1: Characteristics of respondents included in and excluded from the regression sample, 2022; Table S2: Fintech use without the online securities trading and automated advisory items, 2022; Table S3: Digital security items: inter-item correlations and associations with financial detriment, 2022; Table S4: Selection-related covariates and E-values for fully online financial contracting, 2022; Table S5: Age-stratified associations, 2022). References [38–41,48–50] are cited in the Supplementary Materials.

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Institutional Review Board Statement: This study involved secondary analysis of anonymized data from the 2018 and 2022 OECD/INFE financial literacy surveys conducted in Hungary. The datasets were provided by the Pénziránytű Foundation. The authors did not conduct any additional data collection or interact directly with participants. The analysis was based exclusively on anonymized survey data. Ethical review and approval were therefore not required for this study.

Informed Consent Statement: This study involved secondary analysis of previously collected, anonymized survey data. No additional participants were recruited or contacted for the present research.

Data Availability Statement: The data used in this study were obtained from the Pénziránytű Foundation, which conducted the 2018 and 2022 OECD/INFE financial literacy surveys in Hungary with the support of the Central Bank of Hungary. The anonymized datasets were provided to the authors by the Foundation. Access to the original datasets is subject to the data provider's

conditions and permissions. The SPSS syntax and the R script used for the analyses are provided as Supplementary Materials S1 and S2. The design-based 2018–2022 comparisons (Tables 3 and A3, the pooled wave model, and the identical-wording comparison in File S3, Section S3.3), the weighted 2018 education share, and the 2018 design effects (Section 4.3) were computed in R from the provider's 2018 file with weights and municipality identifiers, which is available from the data provider. Municipality identifiers for the 2022 sample were requested from the data provider but have not been made available to date.

Conflicts of Interest: The authors declare no conflicts of interest.

Appendix A

Table A1. Demographic composition of the two survey waves (%).

Characteristic	2018	2022	*p*
Gender			
Male	47.4	48.2	
Female	52.6	51.8	0.738
Age group			
18–29	19.7	17.0	
30–39	14.9	17.4	
40–49	22.5	21.4	
50–59	14.8	16.7	
60–69	18.1	16.8	
70–79	10.1	10.7	0.306
Income category			
Low	28.5	31.3	
Middle	34.1	31.2	
High	22.9	16.4	
Refused	12.8	17.8	
Don't know	1.8	3.3	<0.001
Education			
Tertiary education	12.3	17.7	<0.001

Source: Authors' calculations based on the OECD/INFE surveys conducted in Hungary. Effect sizes (Cramér's V) were 0.007 for gender, 0.055 for age group, and 0.114 for income category. The 2022 share of respondents with tertiary education excludes two missing responses; the weighted shares were 18.8% in 2018 and 18.9% in 2022. Income percentages were calculated using the full sample. The chi-square test for income treats refusals and "don't know" responses as separate categories.

Table A2. Financial knowledge items: percentage of correct responses, 2022.

Item	Correct Responses
Division (equal sharing of a gift)	96.4
Interest on a loan	95.7
Higher returns entail higher risk	90.6
Inflation and purchasing power	84.9
Simple interest	69.1
Diversification reduces risk	68.2
Compound interest	33.7

Source: Authors' calculations based on the OECD/INFE survey conducted in Hungary.

Table A3. Financial detriment by item, 2018 and 2022 (%).

Item	2018	2022	Change	*p*	Holm-Adjusted *p*
Fraud exposure and incidents					
Invested in a product subsequently found to be fraudulent	2.3	1.5	−0.8	0.242	1.000
Disclosed financial information in response to a fraudulent email or call	1.8	1.0	−0.8	0.238	1.000
Card details used without authorization	0.9	1.5	+0.6	0.283	1.000
Disputed an unrecognized transaction	2.0	3.8	+1.8	0.030	0.207
Lost money due to hackers or phishing	1.1	0.7	−0.4	0.462	1.000
Provider disputes and refusals					
Filed a formal complaint with a provider	2.2	0.7	−1.5	0.021	0.184
Bank account opening was refused	1.5	1.0	−0.4	0.522	1.000
Insurance claim was rejected	3.6	1.3	−2.3	0.020	0.184
Complained about high money transfer fees	4.4	3.1	−1.3	0.219	1.000

Source: Authors' calculations based on the OECD/INFE surveys conducted in Hungary. Weighted proportions; design-adjusted Wald tests. The loan rejection item was added in 2022 and is therefore excluded from this comparison. Holm-adjusted *p*: *p*-value adjusted for nine comparisons. In unweighted chi-square tests with Yates correction, the decrease in rejected insurance claims remains significant after Holm correction ($p = 0.002$), whereas the increase in the unrecognized transaction item does not ($p = 0.205$). Changes were calculated from unrounded proportions and may therefore differ from differences between rounded values.

Table A4. Sensitivity of weighted logistic models to measurement decisions, 2022.

Model	N	Events	Digital Activity OR	95% CI	*p*	Knowledge OR (*p*)
Fraud exposure and incidents (Model 1)	639	53	1.60	1.02–2.50	0.042	0.89 (0.366)
Fraud exposure and incidents (N/A = never)	761	59	1.69	1.07–2.65	0.024	0.86 (0.188)
Fraud exposure and incidents, 4 items (excluding the unrecognized transaction item)	639	33	1.94	1.18–3.18	0.009	0.89 (0.546)
Provider disputes and refusals, 5 items (Model 1)	639	65	1.91	1.31–2.79	<0.001	0.84 (0.229)
Provider disputes and refusals, 4 items (excluding loan rejection)	639	45	2.38	1.56–3.64	<0.001	0.88 (0.348)
Provider disputes and refusals, 5 items (N/A = never)	761	75	1.63	1.10–2.41	0.015	0.89 (0.337)

Source: Authors' calculations based on the OECD/INFE survey conducted in Hungary. Models follow the specification of Model 1 in Table 4 (age, gender, income category, digital financial activity, financial knowledge). "N/A = never": "not applicable" responses in the digital financial activity block were assigned to "never" instead of being treated as missing.

Table A5. Odds ratios for digital variables under alternative specifications, 2022.

Specification	Fraud Exposure and Incidents: Online Contracting	Fraud Exposure and Incidents: Digital Activity	Provider Disputes and Refusals: Online Contracting	Provider Disputes and Refusals: Digital Activity
With fully online financial contracting				
Model 2 (Table 4)	2.94 (1.54–5.62); 0.001	1.28 (0.77–2.12); 0.343	3.18 (1.78–5.67); <0.001	1.53 (1.03–2.25); 0.034
+education, number of products	2.82 (1.46–5.46); 0.002	1.29 (0.80–2.07); 0.300	3.27 (1.77–6.02); <0.001	1.52 (1.01–2.29); 0.043
Firth logistic regression	2.48 (1.32–4.60); 0.005	1.35 (0.87–2.07); 0.182	3.10 (1.75–5.49); <0.001	1.64 (1.09–2.43); 0.017
Firth + education, number of products	2.33 (1.23–4.37); 0.010	1.30 (0.81–2.02); 0.270	3.17 (1.78–5.65); <0.001	1.58 (1.02–2.41); 0.039
Imputed income (N = 822)	2.88 (1.66–4.99); <0.001	1.29 (0.85–1.97); 0.234	2.52 (1.48–4.30); <0.001	1.62 (1.13–2.32); 0.009
Missing income as a separate category (N = 822)	2.82 (1.63–4.89); <0.001	1.30 (0.86–1.98); 0.213	2.70 (1.57–4.62); <0.001	1.56 (1.09–2.24); 0.015
Unweighted	2.52 (1.34–4.72); 0.004	1.34 (0.87–2.09); 0.187	3.16 (1.78–5.63); <0.001	1.64 (1.10–2.46); 0.016
+region, settlement type	3.36 (1.67–6.76); <0.001	1.06 (0.62–1.81); 0.832	2.92 (1.55–5.50); <0.001	1.68 (1.11–2.54); 0.014
+five digital security items (N = 587)	3.70 (1.84–7.45); <0.001	1.38 (0.79–2.41); 0.257	3.30 (1.77–6.17); <0.001	1.33 (0.85–2.09); 0.212
Online contracting as a count (0–4)	1.91 (1.14–3.20); 0.013	1.39 (0.85–2.26); 0.191	1.87 (1.17–3.00); 0.009	1.68 (1.14–2.50); 0.010
Three categories: one vs. none	2.55 (1.24–5.25); 0.011	1.28 (0.76–2.16); 0.345	3.18 (1.74–5.83); <0.001	1.53 (1.03–2.25); 0.034
Three categories: two or more vs. none	5.53 (1.84–16.63); 0.002	–	3.17 (0.95–10.57); 0.061	–
Provider disputes and refusals, excluding transfer-fee complaints	–	–	3.11 (1.54–6.27); 0.002	1.44 (0.92–2.26); 0.111
Without fully online financial contracting				
Model 1 (Table 4)	–	1.60 (1.02–2.50); 0.042	–	1.91 (1.31–2.79); <0.001
+education, number of products	–	1.58 (1.03–2.43); 0.035	–	1.94 (1.31–2.87); 0.001
Firth logistic regression	–	1.60 (1.06–2.39); 0.026	–	2.00 (1.38–2.91); <0.001
Imputed income (N = 822)	–	1.63 (1.11–2.38); 0.013	–	1.95 (1.36–2.78); <0.001
Missing income as a separate category (N = 822)	–	1.64 (1.12–2.39); 0.011	–	1.91 (1.34–2.71); <0.001

Table A5. *Cont.*

Specification	Fraud Exposure and Incidents: Online Contracting	Fraud Exposure and Incidents: Digital Activity	Provider Disputes and Refusals: Online Contracting	Provider Disputes and Refusals: Digital Activity
Unweighted	–	1.60 (1.06–2.41); 0.024	–	2.02 (1.39–2.94); <0.001
+region, settlement type	–	1.39 (0.86–2.27); 0.182	–	2.12 (1.42–3.16); <0.001
Provider disputes and refusals, excluding transfer-fee complaints	–	–	–	1.80 (1.17–2.79); 0.008

Source: Authors' calculations based on the OECD/INFE survey conducted in Hungary. Cells report OR (95% CI), *p*-value. Unless otherwise stated, N = 639, with 53 events of fraud exposure and incidents and 65 of provider disputes and refusals; all models include age, gender, income category, and financial knowledge. Models with imputed income include 71 and 74 events, respectively (40 imputations; pooled using Rubin's rules). Firth models are unweighted, with profile-likelihood confidence intervals. Models including region and settlement type are unstable because of the large number of parameters and are not interpreted substantively. Models treating missing income as a separate category include 71 and 74 events; models with the five digital security items include 587 respondents with 42 and 62 events (among the 52 respondents excluded because of missing security items, 11 experienced fraud exposure or incidents [21.2%, compared with 7.2% among those included], and 3 experienced provider disputes or refusals); and the provider disputes and refusals model excluding the transfer-fee item includes 42 events. Unless otherwise stated, fully online financial contracting is entered as a binary indicator (at least one contract vs. none); the three-category rows come from a single model, with digital activity reported in the first row.

Table A6. Item wording, coding, and role of variables in the analysis, 2022.

Variable	Items (OECD/INFE Code) and Item Wording	Coding	Role/Hypothesis
Fraud exposure and incidents	In the past two years, did the respondent: accept investment advice concerning a product subsequently found to be fraudulent (QP10_1); inadvertently disclose financial information in response to a fraudulent email or telephone call appearing genuine (QP10_2); discover that someone had used their card number without authorization (QP10_3); dispute a transaction on an account statement that they did not recognize (QP10_4); or lose money because of hackers or phishing (QP10_10)?	1 = "yes" to at least one item; 0 = otherwise. Non-substantive responses (2018) coded as "no". Sensitivity analysis excluding QP10_4.	Outcome variable; RQ1, H1, H2
Provider disputes and refusals	In the past two years, did the respondent: file a formal complaint about a service provided by a bank or another financial provider (QP10_5); attempt to open a bank account and be refused (QP10_6); have an insurance claim rejected that should have been covered (QP10_7); have a loan application rejected (QP10_8, 2022 only); or complain to a money transfer provider about high charges (QP10_9)?	1 = "yes" to at least one item; 0 = otherwise. QP10_8 excluded when comparing survey waves; sensitivity analyses excluding QP10_8, QP10_9, or QP10_6 and QP10_8 (Table A8); refusals (QP10_6–8) and complaints (QP10_5, QP10_9) also analyzed separately (Table 6).	Outcome variable; RQ1, H1, H2, H3

Table A6. *Cont.*

Variable	Items (OECD/INFE Code) and Item Wording	Coding	Role/Hypothesis
Fully online financial contracting	The respondent opened a bank account (QP8_1), applied for a debit or credit card (QP8_2), purchased insurance (QP8_3), or took out a loan (QP8_4) entirely online, without visiting a branch.	Main models: 1 = at least one affirmative response, 0 = none; sensitivity analyses: number of affirmative responses (0–4) and three categories (0, 1, 2+); "not applicable" (non-internet users) = missing. Banking product: at least one of QP8_1, QP8_2 or QP8_4; insurance: QP8_3.	Main explanatory variable; H1
Digital financial activity	Frequency of: checking account balances and transactions online (QP9_1); paying bills online (QP9_3); shopping online (QP9_4); transferring money online (QP9_5); managing financial products online (QP9_6); paying by mobile phone in stores (QP9_7); using an account aggregation app or website (QP9_8); using an online securities trading platform (QP9_9); and using automated online advisory services (QP9_10).	1 = never, 2 = sometimes, 3 = often, 4 = very often; mean of nine items if at least seven valid responses. "Not applicable", "don't know" and refusal = missing (sensitivity analysis: "not applicable" = "never"). Basic activities: QP9_1, QP9_3–6; fintech: QP9_7–10 (binary form: at least one item rated "sometimes" or higher).	Main explanatory variable; H2
Financial knowledge	Equal division of a gift (QK2); inflation and purchasing power (QK3); interest on a loan (QK4); simple interest (QK5); compound interest (QK6); higher returns entail higher risk (QK7_1); diversification reduces risk (QK7_3).	Correct = 1; incorrect, "don't know", and refusal = 0; sum (0–7).	Main explanatory variable; H3; exploratory for fraud exposure and incidents
Trust in fintech services	"I trust the financial services offered by online banks and FinTech companies" (QS4_5).	Five-point scale, reverse-coded: 1 = not at all; 5 = completely.	Exploratory variable; Table A7
Digital security items	Sharing passwords (QS2_6); checking whether the provider is regulated before buying a financial product online (QS2_7); changing passwords regularly (QS3_11); considering public Wi-Fi secure for online shopping (QS4_1); considering it important to check the security of a website before making an online transaction (QS4_2).	Five-point scales; QS2_7, QS3_11 and QS4_2 reverse-coded so that higher values indicate more cautious behavior; analyzed individually because the items do not form a scale.	Exploratory variable
Number of financial products held	Whether the respondent currently holds each financial product (QP2_1–QP2_13, QP2_16, QP2_17).	Number of affirmative responses (0–15).	Control variable in supplementary models

Table A6. *Cont.*

Variable	Items (OECD/INFE Code) and Item Wording	Coding	Role/Hypothesis
Control variables	Age (QD7); gender (QD1); monthly household income (QD13); education (QD9); region (QD2); settlement type (QD3).	Age in years; male = 1; income: 1 = low, 2 = middle, 3 = high; refusal and "don't know" coded as missing (separate category in sensitivity analysis); education: college degree or higher = tertiary, secondary school = upper secondary, lower levels = at most lower secondary.	Age, gender, and income: control variables in all models; education, region, and settlement type: supplementary models only

Source: Authors' compilation based on the 2022 OECD/INFE core questionnaire and Hungarian questionnaire [39,41]. The item descriptions are abbreviated English translations of the Hungarian wording; item codes follow the OECD/INFE core questionnaire. Valid sample sizes by variable: financial detriment, 2018–2022, N = 2001; online contracting, N = 854; digital activity and subindices, N = 824; financial knowledge, N = 1000; trust, N = 917; digital security items, N = 915–978 per item; education, N = 998; financial products and territorial variables, N = 1000; valid income responses, N = 789. Basic digital activities and the fintech subindex comprise five and four items, respectively; online contracting was analyzed as a binary indicator in the main models and, in sensitivity analyses, as a count, in three categories, and by contract type. In the 2018–2022 comparison, provider disputes and refusals were based on four items (five in 2022); the indicator of fraud exposure and incidents comprised five items (four in the sensitivity analysis). Region has eight categories and settlement type five; survey wave (2018/2022) is the grouping variable for RQ1.

Table A7. Associations of trust in fintech services: weighted logistic regression, 2022.

Outcome	N	Events	Trust (1–5)	Fully Online Contracting (At Least One)	Digital Activity
Fintech use (at least one service)	604	278	1.20 (1.05–1.38); 0.009	–	–
Fully online contracting (at least one)	626	143	1.14 (0.97–1.33); 0.110	–	–
Fraud exposure and incidents	604	49	0.75 (0.56–1.01); 0.058	2.86 (1.46–5.60); 0.002	1.55 (0.99–2.45); 0.056
Provider disputes and refusals	604	61	1.11 (0.84–1.47); 0.457	2.92 (1.61–5.30); <0.001	1.54 (1.03–2.31); 0.035

Source: Authors' calculations based on the OECD/INFE survey conducted in Hungary. Cells report OR (95% CI); *p*-value. All models include age, gender, income category, and financial knowledge. Trust is measured with a single item; higher values indicate greater trust.

Table A8. Fully online financial contracting and provider disputes and refusals after removing overlapping items, 2022.

Specification	N	Events	OR	95% CI	*p*
Main specification (Table 4, Model 2)	639	65	3.18	1.78–5.67	<0.001
Outcome without refused account opening and loan rejection	639	37	4.02	2.07–7.81	<0.001
Contracting without online account opening and taking out a loan	639	65	2.37	1.29–4.35	0.005
Both exclusions	639	37	4.22	2.04–8.70	<0.001

Source: Authors' calculations based on the OECD/INFE survey conducted in Hungary. Models follow the specification of Model 2 in Table 4 (age, gender, income category, digital financial activity, financial knowledge, fully online financial contracting as a binary indicator). OR refers to having concluded at least one contract entirely online.

Table A9. Sensitivity of the confirmatory tests to assumed design effects, 2022.

Test	OR	Observed SE: 95% CI; *p*; Holm *p*	Design Effect 1.5	Design Effect 2.0
H1: fraud exposure and incidents (Model 2)	2.94	1.54–5.62; 0.001; 0.003	1.33–6.50; 0.008; 0.025	1.18–7.35; 0.021; 0.071
H1: provider disputes and refusals (Model 2)	3.18	1.78–5.67; <0.001; <0.001	1.56–6.46; 0.001; 0.007	1.40–7.21; 0.006; 0.029
H2: fraud exposure and incidents (Model 1)	1.60	1.02–2.50; 0.042; 0.084	0.92–2.77; 0.097; 0.193	0.84–3.01; 0.150; 0.300
H2: provider disputes and refusals (Model 1)	1.91	1.31–2.79; <0.001; 0.003	1.20–3.04; 0.006; 0.025	1.12–3.27; 0.018; 0.071
H3: provider disputes and refusals (Model 1)	0.84	0.64–1.12; 0.229; 0.229	0.60–1.19; 0.326; 0.326	0.57–1.25; 0.395; 0.395
H1 with the count (0–4): fraud exposure and incidents (Model 2)	1.91	1.14–3.20; 0.013; 0.040	1.02–3.59; 0.044; 0.136	0.92–3.96; 0.080; 0.265
H1 with the count (0–4): provider disputes and refusals (Model 2)	1.87	1.17–3.00; 0.009; 0.038	1.05–3.33; 0.034; 0.136	0.96–3.64; 0.066; 0.265

Source: Authors' calculations based on the OECD/INFE survey conducted in Hungary. Cells report the 95% CI of the OR; *p*-value; Holm-adjusted *p*-value for the five confirmatory tests. Standard errors from Table 4 (count rows: count models in Table A5) were multiplied by the square root of the assumed design effect. The design effect due to weighting alone (Kish) is 1.14 in the regression sample and is already reflected in the observed standard errors. For comparison, municipality-level clustering in the 2018 data, relative to weighting alone, implied design effects of 2.38–2.65 for prevalences and 0.89–1.68 (median 1.25) for the coefficients of the covariates common to both waves. After rescaling to the 2022 cluster size, the median was 1.20–1.33 (Section 4.3; File S2, part 2a). Clustering on region × settlement-type cells in the 2022 data gave design effects of 1.04–2.20 for the five confirmatory coefficients. For the count rows, the Holm correction is applied to the five confirmatory tests with H1 operationalized as the count (Section 3.2).

Table A10. Fully online financial contracting among respondents at risk of provider disputes and refusals, 2022.

Outcome	Exposure	Events	Weighted OR (95% CI); *p*	Firth OR (95% CI); *p*
Panel A: holders or recent choosers of a banking product (n = 528)				
Provider disputes and refusals	At least one online contract	54	3.69 (2.01–6.79); <0.001	3.31 (1.77–6.18); <0.001
Provider disputes and refusals	Online banking product	54	4.56 (2.11–9.89); <0.001	3.51 (1.66–7.17); 0.001
Refused account opening or loan rejection	At least one online contract	29	3.07 (1.39–6.81); 0.006	2.54 (1.11–5.69); 0.027
Refused account opening or loan rejection	Online banking product	29	6.89 (2.75–17.23); <0.001	4.56 (1.82–10.86); 0.002
Complaints	At least one online contract	28	4.89 (2.33–10.28); <0.001	4.33 (1.91–10.05); <0.001

Table A10. *Cont.*

Outcome (n)	Exposure: online vs. not online, % (events/n)	Events	Fisher's exact *p*
Panel B: product-specific comparisons (holders or recent choosers of the product)			
Account opening refused (n = 504)	Online account opening: 3.6% (1/28) vs. 1.1% (5/476)	6	0.292
Loan application rejected (n = 175)	Online credit: 25.0% (2/8) vs. 4.8% (8/167)	10	0.069
Insurance claim refused (n = 178)	Online insurance: 3.1% (1/32) vs. 0.7% (1/146)	2	0.328

Source: Authors' calculations based on the OECD/INFE survey conducted in Hungary. Panel A models follow Model 2 in Table 4 (age, gender, income category, digital financial activity, financial knowledge); Firth models are unweighted. Panel B reports unweighted percentages; the survey did not record product applications, so holders and recent choosers approximate the population at risk.

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